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REVIEW  
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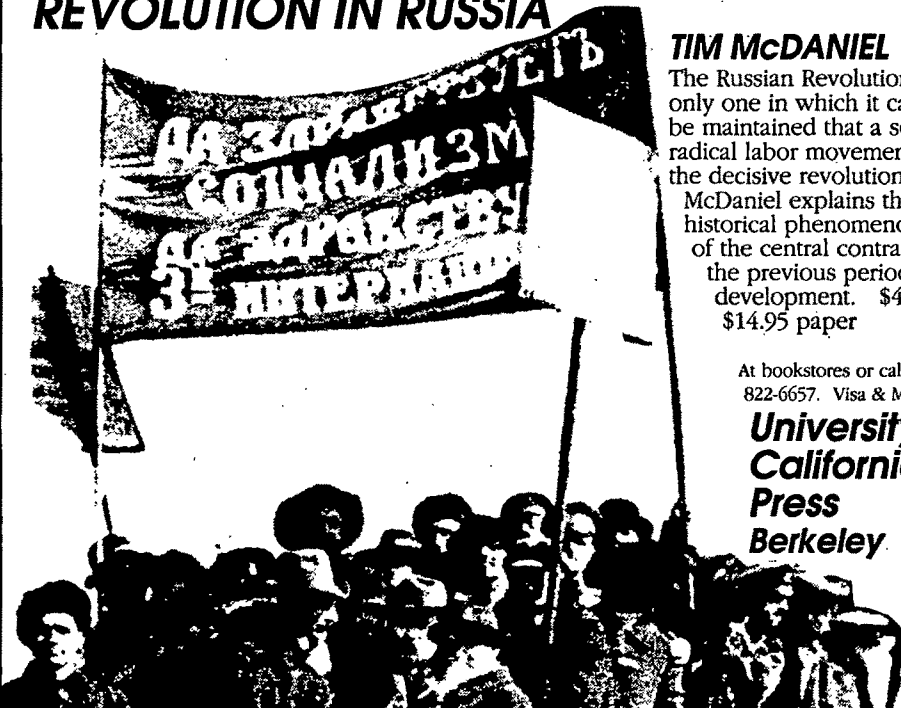
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## ARTICLES

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## ONE SOUL AT A TIME: POLITICAL SCIENCE AND POLITICAL REFORM

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SAMUEL P. HUNTINGTON

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*Political scientists want to do good. They want to expand knowledge about political life, but also they wish to use knowledge for political reform. Usually this means desiring to promote "democratization." Historically democracy and political science have tended to develop together. In modest ways political science can contribute to the emergence of democracy. Political reform succeeds best if it occurs incrementally, in the spirit of "one soul at a time."*

Let me begin with what may be an unsettling and perhaps even shocking observation about our profession but one that I derive from forty-four years of watching political scientists at work: by and large, political scientists want to do good. To mitigate the impact of this statement, let me quickly add two additional points. First, because political scientists want to do good does not mean that they actually do good. They may fail miserably to realize their intentions, although that does not mean that they therefore do evil. Second, by *want to do good* I do not mean that political scientists simply want to expand our understanding of politics. Political scientists want to do that: that is the professional calling of political science. The expansion of knowledge is, of course, a good in itself and may be pursued as such. What I am suggesting is that political scientists want to expand our knowledge of politics because they usually see or sense a link between this expanded knowledge and broader social goals or public purposes. The exact nature of these purposes varies, of course, among members of the profession. Prominent among them are the promotion of justice, well-

being, order, equity, liberty, democracy, responsible government, security for individuals and states, accommodation among groups, peace among nations. In some measure these goals may be subsumed under the broad category *political goods* that Roland Pennock elaborated over two decades ago (1966, 415-34).

In particular circumstances these goals may conflict with each other. Few people, however, would deny ultimate legitimacy to any of them, even if they disagreed vigorously over their relative priority in situations where choices have to be made. It is hard, indeed, to think of a major work in political science that is not inspired in some measure—perhaps remotely but often very directly—by one of these purposes. Political science, in short, is not just an intellectual discipline; it is also a moral one. Morality, observes Albert Hirschman (who, I might note, was elected most deservedly although belatedly to the National Academy of Sciences last spring), "belongs into the center of our work; and it can get there only if the social scientists are morally alive and make themselves vulnerable to moral concerns—then they will produce morally significant works, consciously or other-

wise" (1981, 305). From this it follows that works in the social sciences should be judged not only on their intellectual merit but also by the contributions they make to achieving moral purposes. We may intellectually admire "neat" and highly sophisticated analyses, quantitative or otherwise, of esoteric scholarly issues; but implicitly we do not accord them the same status we do the works of Dahl probing the preconditions of democracy, Walzer grappling with the nature of just war, Verba illuminating the dimensions of equality, or Horowitz exploring the ways of resolving ethnic conflict.

The phrase *doing good* is rather sweeping in scope, in that it could encompass all sorts of personal and public goals, political and otherwise. It also conveys a somewhat mushy impression. Let me, therefore, dispense with that phrase and instead use one more directly emphasizing doing good in politics, that is, political reform. By *political reform* I mean the peaceful—hence gradual, hence usually negotiated—enhancement of liberty, justice, equality, democracy, and responsibility in politics. The impetus to do good in the sense of promoting political reform is, I would argue, embedded in our profession.

The historical relation between political science and political reform, it should be noted, has a very specific meaning in the American context. The emergence of political science was "part of a broad movement of Progressive reform in American intellectual and political life" at the end of the nineteenth century. The major figures in political science, among them A. Lawrence Lowell, Woodrow Wilson, Frank Goodnow, Albert Bushnell Hart, and Charles Beard, were associated with the Progressive movement (Huntington 1974, 7). "To call the roll of the distinguished social scientists of the Progressive era," Richard Hofstadter noted, "is to read a list of men prominent in their criticism of vested interests or in their support

for reform causes" (quoted in Huntington 1974, 5).

This particular historical association between political science and political reform rests on a logical basis. Political science, as Lowell and Wilson emphasized, is or should be devoted to studying the realities of politics, the how and the why of political behavior. This heavy emphasis on the empirical exploration of the realities of politics tends to drive political scientists in two directions. First, politics turns out to be extremely complex and ambiguous; hence there are not only no easy solutions but also generally speaking no simple or obvious solutions to political problems. The nature of politics tends to reinforce nonideological, nondoctrinaire, and pragmatic tendencies among those who study it. Now obviously I do not want to push this argument too far. We all know political scientists—much-admired political scientists—who are ideological if not doctrinaire and who have advanced simple, if not simplistic, remedies to complex problems. I am arguing only that the logic of political inquiry works in the opposite direction, not that everyone is necessarily persuaded by that logic.

Second, while the exploration of political reality—understanding things as they are and why they are as they are—may push political scientists in a conservative direction, it also tends to leave them in a dissatisfied mood. Knowing how things work in practice can only remind one of the large gap between that and how they ought to work in theory. As political scientists quickly discover, the behavior of politicians, bureaucrats, and voters and the operations of political institutions and processes often leave much to be desired. In a sense, political scientists suffer an intense disciplinary version of what on a broader scale I once called the *101 gap*. And a not unnatural response to this gap on the part of political scientists is, implicitly or explicitly, to suggest ways—

## Political Science and Reform

usually modest and occasionally practical—for reforming things. Quincy Wright once said that after a “general education in political science . . . it is hard to see how a political scientist can be either a Nazi or a Communist” (quoted in Ricci 1984, 170). It is also hard to see how a political scientist can be entirely happy with the status quo.

The study of political science thus tends to highlight both what’s wrong with politics and how difficult, complex, tedious, ambiguous, and uncertain it is to correct what’s wrong. Political scientists come to learn only too well the truth behind Oxenstierna’s famous lament, “If you only knew, my son, with what little wisdom the world is governed!” But they come to learn equally well the familiar biblical truth that “the children of this world are in their generation wiser than the children of light.” The problem of combining both truths, both perspectives, is perhaps most dramatic in our teaching. How do we make our students wise about politics without also making them disillusioned with politics?

Despite the efforts of political scientists, wisdom about politics is in short supply in today’s world. Much of modern politics is, on the surface, abolitionist politics. It is devoted to efforts to do away with things: abolish war, abolish nuclear weapons, abolish injustice, abolish tyranny, abolish apartheid. Those demanding fundamental change typically describe the existing political system as the epitome of evil, the worst system possible. Hence all contact or cooperation with it must be avoided. Hence it must be totally destroyed. This is a simple and effective political strategy. Focusing on an evil, whose evilness cannot be denied, generates moral outrage, facilitates political mobilization, and sets forth a clear, simple, and unifying goal, while avoiding the divisive issue of what should replace that evil. If the existing political system is the worst possible system, then of course vir-

tually any means, any amount of violence, is legitimate in attempting to overthrow it.

The study of politics, however, shows clearly that the process of doing good involves far more than simply doing away with evil. It is difficult to imagine worse political systems than those of Hitler or Stalin. Yet however bad a given evil may be, a worse one is always possible and often likely: witness the unhappy recent experiences of Cuba, Vietnam, Cambodia, Ethiopia, Nicaragua, and Iran, among others. The political scientist, consequently, can only be skeptical of a claim that, as one Chilean dissident put it, “there is no way in which we are going to see anything worse than General Pinochet” or the argument of an opponent of apartheid that “the first African government of South Africa . . . could scarcely be any worse” than the current regime.

And let me hastily add, lest this be read as a political statement, that I can easily conceive of worse systems than that in the Soviet Union, even before Gorbachev began to change it. Fifty years ago, indeed, the Soviet system was much worse than it is now. Military authoritarianism, racial oligarchy, and Communist dictatorship are evil political systems; but the assumption that things could not become worse than they are in Chile, South Africa, or the Soviet Union defies logic and history.

Political reformers wise in the ways of politics understand this and hence tend to be leery of simple solutions and of revolution and revolutionaries. This skepticism has been well expressed in the statements of three notable contemporary democratic reformers from three different continents, one unsuccessful, one successful posthumously, and one as yet neither successful nor unsuccessful. First, “We know lots of revolutions, great revolutions, and magnificent people,” said Lech Walesa, “who after taking over power, produced systems that were much worse than the

ones they destroyed. We do not want to make this mistake." Second, "In a revolution," said Benigno Aquino in a speech drafted for delivery at Manila Airport, "there can really be no victors, only victims. We do not have to destroy in order to build." Third, "Bloody revolutions," Mangosutho Buthelezi warned, "fought against terrible oppression do not automatically bring about great improvements."<sup>2</sup>

An extremely close relationship exists between political science and one particular type of political reform. Robert Heilbroner once noted that it is impossible to have economists in the absence of commercial relations and that it was the development of the market system in early modern times that, in his phrase, "called forth the economists." Had Adam Smith lived in an earlier age, Heilbroner says, he "might have been a great moral philosopher," but "he could never have been a great economist; there would have been nothing for him to do" (1980, 18, 27). Somewhat similarly, it can be argued that it is impossible to have political scientists in the absence of political participation, and political science has only developed with the expansion of political participation. In a society in which there is no participation—no competition for power—political scientists would have nothing to do. Consequently, it is not surprising that a market propensity tends to prevail among economists and a participatory or democratic propensity among political scientists. Command economies have no use for economists, nor authoritarian politics for political scientists.

The connection between democracy and political science has been a close and continuing one. Nowhere was that connection more meaningful than in the event whose two hundredth anniversary we celebrate this year. The men who gathered in Philadelphia in the summer of 1787 were, in John Roche's felicitous phrase, "a nationalist reform caucus":

They were democratic politicians struggling to fashion a pragmatic compromise that would both create a strong national government and be acceptable to the people of the states (1961, 799). As Austin Ranney pointed out on this occasion twelve years ago, Washington, Hamilton, Adams, and Jefferson all stressed the central importance of the study of the "science of politics" and the "science of government" to the work they were engaged in of creating a new nation (1976, 141-43). A gathering at Harvard the year the Constitution was ratified voted that it was "more necessary in a Republic than in any other form of government that young men should be instructed in political science" (William Coolidge Lane, quoted in Crick 1959, 5).

The creation of a republic and the development of democracy called forth political science and political scientists. The relationship is nowhere more notable than in countries, such as Germany and Italy, that, prior to World War II, had strong traditions of scholarship in history, social theory, and sociology but not in political science. As Hans Kastendiek has pointed out in his study of Germany, "Because of the structure of politics in the 19th century and the political power relations which characterized the Weimar Republic and were mirrored in the academic system, conditions for a distinct political science simply did not exist. . . . The emergence of the discipline after 1945 was due to specific political constellations. . . . 'To build up democracy' and to install a distinct political science were needs directly connected to each other" (1987, 26).

Bernard Crick thus got it wrong in the title of his famous book about our discipline. There is not an *American* science of politics; there is a *democratic* science of politics, which developed first and fullest in the United States because the United States was the first and fullest democracy in the modern world. That science is a

## Political Science and Reform

universal science to the extent that democracy is a universal system of government. Where democracy is strong, political science is strong; where democracy is weak, political science is weak. Authoritarian societies may produce and in some cases have produced Nobel Prize-winning physicists, biologists, novelists, and statesmen; they do not produce great political scientists. The emergence of democracy encourages the development of political science, and the development of political science can and has in small ways contributed to the emergence and stabilization of democracy.

Since 1974, dictatorships have given way to democracies in many countries, including Portugal, Spain, Greece, Turkey, Brazil, Argentina, Uruguay, Peru, Ecuador, and the Philippines. A transition may be underway now in the Republic of Korea; modest steps toward broader political participation have taken place in Taiwan. In still other countries elections seem to be acquiring new popularity. If not reversed, all this bodes well for the future of democracy and for the future of political science. Political scientists are uniquely qualified to study these processes of change, to derive generalizations and lessons from them, and to suggest ways in which this knowledge may be applied to countries where democracy is yet to be realized. Many political scientists, including Linz, Stepan, Lipset, Diamond, O'Donnell, and Schmitter, as well as others, have responded to this challenge and have carried out studies that add importantly to our understanding of the conditions and processes of democratization (see Diamond, Linz, and Lipset n.d.; Linz and Stepan 1978; and O'Donnell, Schmitter, and Whitehead 1986).

I cannot summarize their findings here and I will not attempt to advance any systematic analysis of my own, but I do suggest that the most important lesson from the history of democratization is that what is difficult is not the termination of

the nondemocratic regime but the development of stable democratic institutions. In this process political science, if given a chance, may be able to play some small constructive role. Let me illustrate this by reference to three contemporary cases.

Brazil is perhaps the most striking example of democratization introduced from above by a military elite that realized and indeed supported the need to move toward democracy.<sup>3</sup> In 1973 Brazil was a military dictatorship; in 1985 it was a democracy. The genius of the Brazilian transition is that it is virtually impossible to pinpoint the time during the twelve years in between when Brazil stopped being a dictatorship and became a democracy. The Brazilian transition to democracy was in many respects a masterpiece of obfuscated incrementalism. Political science played a modest role in this process. The military leadership that guided the transition was aware of the experiences of other countries and sensitive to the need first to encourage "decompression" (*distensao*) and the "opening" (*abertura*) of their society so as to minimize the probability of extreme polarization and violent upheaval and second to proceed with changes incrementally, often on a two-steps-forward-one-step-backward basis and thus to make democratization, as President Geisel put it, "slow, gradual, and sure." The leading military figure guiding the democratization, General Golbery, was a long-term member of the American Political Science Association; he and other key figures in the democratization process solicited the advice of political scientists; and a younger generation of Brazilian political scientists—who, thanks to the Ford Foundation, had been trained during the 1960s at Stanford, UCLA, Harvard, MIT, Michigan, and elsewhere—played active roles in developing and articulating ideas that were central to the Brazilian process.

For several years China has been in the midst of economic reforms, some of



which are quite far-reaching. These changes naturally raise the question as to whether equally meaningful political reforms will follow. The centralized authoritarianism of the Chinese political system has, however, had its correlate in the undeveloped state of Chinese political science. Major universities do not have political science departments as such; what is termed *political science* has so far been generally limited to the study of foreign societies and public administration. Kent Morrison quotes one of his graduate students at Zhongshan University in Guangzhou as saying in 1984, "To be a political scientist in China is, I think, very dangerous" (Morrison and Thompson 1985, 1). The student is right and the reason is that ultimately, political science can only be very dangerous to the regime in China. Yet the Chinese government is now sending scores if not hundreds of graduate students to the United States to study political science. Inevitably, these students also learn democracy. One can only wonder what the Chinese leaders expect to achieve by doing this. It seems very likely that the leaders either will have to accept a massive brain drain from their society; or will have to find room in their society for a highly intelligent, articulate, and well-connected group of scholarly lobbyists pushing for truly meaningful democratization; or will have to expand their prison system. (In the Soviet Union, it should be noted, what is termed *political science* remains in a primitive state. The work of Soviet political scientists has, however, been closely associated with efforts to reform the Soviet system and to broaden participation in it (Hill 1980; also Brown 1986, esp. pp. 466-69). One test of the meaningfulness of Gorbachev's commitment to *glasnost* and democratization will be the extent to which Soviet scholars are permitted to develop a truly empirical political science, to broaden their contacts with Western political scientists, and to import elements

of Western political science into their research and teaching.

Perhaps nowhere in the world is political science more relevant today than in South Africa. Much of the debate over the South African future has been conducted in terms of concepts and hypotheses from political science.<sup>4</sup> Political scientists—white political scientists—have been in the vanguard of those working for the end of apartheid and some form of meaningful sharing of power among all racial groups. What has been most dramatically and tragically missing from the debate have been the voices of black political scientists. The reason is that almost none exist. Yet surely black political scientists should and could make a major contribution to the future political development of their country. Recognizing this need, last year the council of our association advanced a proposal, modeled in part on Ford's 1960s program for Brazil, to bring nonwhite South Africans to U.S. universities for graduate work in political science. I am happy to say that the Ford Foundation will provide a substantial grant to get this program started in the coming year.

Deng Xiaoping and Mikhail Gorbachev may be reformers but clearly neither has any intention of abandoning the Communist party's monopoly of power in his country. Similarly, P. W. Botha has made significant reforms in the South African system of apartheid. These reforms have, however, been followed by intensified and at times vicious repetition; and there are no signs of any intention to relax the National party's grip on power. In all three countries the intent of the reforms is to strengthen the existing system, not to change it. Under these circumstances, the question then becomes, How can democratic reformers exploit the opportunities offered by preservative reforms to promote more meaningful change? One notable historical case where an exercise in preservative reform was made over into an exercise in fundamental

## Political Science and Reform

reform was the constitutional convention of 1787. That process has perhaps been most closely approximated recently in the 1986 constitution-making efforts that occurred in South Africa.

For eight months, over fifty representatives of the government of the South African province of Natal, the government of the Zulu homeland of KwaZulu, and over thirty other organizations representing all South African racial groups met in Durban and hammered out a constitution for a racially integrated, democratic system of government for that region of South Africa. As in Philadelphia in 1787, provisions for majority rule had to be reconciled with the need to protect minority rights. As in Philadelphia, frequent references were made to the lessons of political science and the experiences of other societies. As in Philadelphia, a few delegates did not sign the final document, but over eighty percent of them did. As with the Philadelphia convention, the Durban Indaba was attacked from both Right and Left (by the revolutionaries of the African National Congress and its front groups on the one hand and by the Afrikaner standpatters on the other). As with the Philadelphia constitution, agreement on the Durban constitution was eased by the anticipation that one widely respected person (in this case Chief Buthelezi) would become the chief executive of the new government. As with the Philadelphia constitution, the Durban constitution also needs to be ratified. In this case, however, ratification means approval not by state assemblies but rather by the national government. To date, that approval has not been denied, but it also has not been forthcoming. It is not clear that even the persuasive logic of a Hamilton, a Madison, and a Jay could bring it about; but it is possible that hard-headed negotiations between the Indaba supporters and the National party may allow some version of the proposed institutions to come into being.

This effort at constitutional change has been virtually ignored in the United States. Closed meetings devoted to the undramatic bargaining and drafting required for political reform do not make good copy for the media; nor do they provide the opportunity, as do police brutality and necklacing, for expressions of moral outrage at the evils of apartheid. The Durban constitution is no overall solution to the traumas of South Africa. Within the context of this one province, however, it is a constructive and hopeful effort to create democratic institutions to replace apartheid. Like the Philadelphia constitution, it is the compromise product of political practitioners informed by political science working to reconcile conflicting interests and principles.

I began these remarks with the suggestion that political scientists, by and large, want to do good and then went on to argue that a close connection exists between political science and political reform and particularly between political science and one type of political reform, that is, democratization. Let me close by emphasizing again the central lesson the study of politics offers to those who wish to bring about political reform.

The Salvation Army has a motto, "Save the world, one soul at a time." Political science has helped, can help, and should help to save the world by generating understanding of political processes, by illuminating the feasibility and consequences of alternative governmental arrangements, and by enhancing appreciation of the potentialities and the limits of political engineering. The most fundamental lesson of the study of politics, however, is that there are no shortcuts to political salvation. If the world is to be saved and stable democratic institutions created, it will be done through *incremental* political reform undertaken by moderate, realistic men and women in the spirit of one-soul-at-a-time. That is the sobering yet hopeful message of our discipline.

## Notes

This essay is the presidential address presented on September 3, 1987 at the 83rd annual meeting of the American Political Science Association in Chicago.

1. Ariel Dorfman, quoted in *Campus Report*, 1986, 1: 8; Pierre L. van den Berghe, quoted in Adam 1971, 118.

2. Lech Walesa, interview with Neal Conan, 5 February 1985, National Public Radio; Benigno Aquino, speech text, *New York Times*, 22 August 1983; Chief Mangosuthu G. Buthelezi, "Disinvestment is Anti-Black," *Wall Street Journal*, 20 February 1985.

3. See, in general, the comprehensive study by Skidmore n.d., esp. chaps. 6, 7.

4. See, for example, Republic of South Africa, esp. chaps. 3 and 4, and the central role that Arend Lijphart's theory of consociational democracy has played in discussions of possible political solutions in South Africa. Lijphart (1985) applies his theory specifically to that country and also provides a comprehensive bibliography of the relevant literature (pp. 137-71).

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# STRUCTURAL DEPENDENCE OF THE STATE ON CAPITAL

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A central claim of both Marxist and neoclassical political theory is that under capitalism all governments must respect and protect the essential claims of those who own the productive wealth of society. This is the theory of "structural dependence of the state on capital." Using a formal model, the internal logic and the robustness of the theory is examined. We conclude that in a static sense the theory is false: virtually any distribution of consumption between wage earners and owners of capital is compatible with continual private investment once an appropriate set of taxes and transfers is in place. Yet the state may be structurally dependent in a dynamic sense. Policies that, once in place, redistribute income without reducing investment do reduce investment during the period in which they are anticipated but not yet implemented.

Capitalism is a system in which many scarce resources are owned privately, and decisions about allocating them are a private prerogative. Democracy is a system through which people as citizens may express preferences about allocating resources that they do not privately own. Hence the perennial question of political theory and of practical politics concerns the competence of these two systems with regard to each other. Is it possible for governments to control a capitalist economy? In particular, is it possible to steer the economy against the interests and preferences of those who control productive wealth?

The central and only distinctive claim of Marxist political theory is that under capitalism all governments must respect and protect the essential claims of those who own the productive wealth of society. Capitalists are endowed with public power, power which no formal institutions can overcome (Luxemburg 1970;

Pashukanis 1951). People may have political rights, they may vote, and governments may pursue popular mandates. But the effective capacity of any government to attain whatever are its goals is circumscribed by the public power of capital. The nature of political forces that come into office does not alter these limits, it is claimed, for they are structural—a characteristic of the system, not of the occupants of governmental positions nor of the winners of elections.

During the past 20 years Marxists have developed several theories to explain why all governments in capitalist societies are bound to act in the interests of capitalists. In one explanation, state managers internalize the goals of capitalists and use the state as an instrument on their behalf (Miliband 1969). Another explanation emphasizes the generic structural and functional limitations: under capitalism, the state cannot organize production (Offe 1975), mandate investment (Lind-

blom 1977), or command consumption, because these are prerogatives reserved to owners of private property. But the most daring, because the least contingent, theory claims that it does not matter who the state managers are, what they want, and whom they represent. Nor does it matter how the state is organized and what it is legally able or unable to do. Capitalists do not even have to organize and act collectively: it suffices that they blindly pursue narrow, private self-interest to sharply restrict the options of all governments. This is the theory of "structural dependence of the state on capital."

This theory begins with the hypothesis that the entire society depends on the allocation of resources chosen by owners of capital. Investment decisions have public and long-lasting consequences: they determine the future possibilities of production, employment, and consumption for all. Yet they are private decisions. Since every individual and group must consider its future, since future consumption possibilities depend on present investment, and since investment decisions are private, all social groups are constrained in the pursuit of their material interests by the effect of their actions on the willingness of owners of capital to invest, which in turn depends on the profitability of investment. In a capitalist society, the trade-off between present and future consumption for all passes through a trade-off between consumption of those who don't own capital and profits (Przeworski 1985, chap. 5).

Consider this dependence from the point of view of one group, wage earners. At any instant of time, wages and profits are indeed inversely related, as Marx argued in *Wage, Labour, and Capital* (1952a). In a world without a future, wage earners would be best-off consuming the entire product, indeed confiscating the capital stock. But wage earners care about their future as well as present

income, and future wages depend on private investment. If firms respond to wage increases with less investment, wage earners may be best-off moderating their wage demands. Workers' future income depends upon the realization of capitalists' present interests.

While the theory is usually stated with regard to workers' wage demands, to the extent that material means are required to advance their welfare, structural dependence binds all groups: minorities struggling for economic equality, women wanting to transform the division of labor within the household, old people searching for material security, workers striving for safer working conditions, or the military seeking bombs. It is in this sense that capitalism is a class society: not in the sense that there are always two organized classes, but in the sense that the structure of property characteristic of capitalism makes everyone's material conditions dependent upon the private decisions of owners of wealth.

The theory of structural dependence continues with the inference that because the entire society depends on the owners of capital, so must the state. Whether particular governments have interests and goals of their own or they act on behalf of a coalition of groups or a class, the pursuit of any objectives that require material resources places governments in the situation of structural dependence. Politicians seeking reelection must anticipate the impact of their policies on the decisions of firms because these decisions affect employment, inflation, and personal income of voters: vote-seeking politicians are dependent on owners of capital because voters are. Even a government that was a perfect agent of wage earners could not and would not behave much differently from one that represents capitalists. If workers are best-off with a fair dose of wage restraint, a prolabor government will also avoid policies that dramatically alter the distribution of income and

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wealth. The range of actions that governments find best for the interests they represent is narrowly circumscribed, whatever these interests may be.

The reason the state is structurally dependent is that no government can simultaneously reduce profits and increase investment. Firms invest as a function of expected returns; policies that transfer income away from owners of capital reduce the rate of return and thus of investment. Governments face a trade-off between distribution and growth, between equality and efficiency. They can trade a more (or less) egalitarian distribution of income for less (or more) investment but they cannot alter the terms of this trade-off: this is the central thesis of the theory of structural dependence. Governments can and do choose between growth and income distribution; but because material welfare of any constituency depends upon economic growth as well as its share of income and because distribution can be achieved only at the cost of growth, all governments end up pursuing policies with limited redistributive effects.

At this moment the reader may remark that this is the neoliberal theory as well. It is. The Chicago school argues that all transfers of income cause deadweight losses. All support-maximizing governments, it is claimed, are tempered in their zeal for redistribution by the fact that owners of endowments will increasingly withdraw them from productive uses as taxes rise. (Bates and Lien 1985, Becker 1983, and Peltzman 1976 are presentations of formal models.) The difference between the two theories is that neoliberals are "pluralists"; that is, they are agnostic about the groups that have the power of inflicting the losses on the public by withdrawing their endowments. This difference should not obscure, however, the fact that both theories understand in the same way the relation between income distribution and investment.

The belief that under capitalism governments are structurally dependent on capital is widespread. Miliband (1969, 152) portrayed this dependence as follows: "Given the degree of economic power which rests in the 'business community' and the decisive importance of its actions (or its nonactions) for major aspects of economic policy, any government with serious pretensions to radical reform must either seek to appropriate that power or find its room for radical action rigidly circumscribed by the requirements of 'business confidence.'" Block (1977, 15) maintained that "in a capitalist economy the level of economic activity is largely determined by private investment decisions of capitalists. This means that capitalists, in their collective role as investors, have a veto over state policies in that their failure to invest at adequate levels can create major political problems for state managers." Lindblom (1977, 172, 175) observed that "because public functions in the market system rest in the hands of businessmen, it follows that jobs, prices, production, growth, the standard of living, and the economic security of everyone all rest in their hands. . . . In the eyes of government officials, therefore, businessmen do not appear simply as representatives of a special interest. . . . They appear as functionaries performing functions that government officials regard as indispensable." Offe (1975, 234) analyzed the predicament of the state vis-à-vis the private economy: "The political system can only make offers to external, autonomous bodies responsible for decisions: either these offers are not accepted, thus making the attempts at direction in vain, or the offers are so attractive in order to be accepted that the political direction for its part loses its autonomy because it has to internalize the aims of the system to be directed."

The theory of structural dependence can be evaluated in three ways. One

method is to examine differences in policy choices and outcomes under different governments. Unfortunately, while there is much evidence that some policy outcomes covary with partisan orientations of governments, these findings cast little light on the existence of structural constraints that bind all governments. We cannot know whether the observed differences exhaust the realm of possibility. (The classical study was Hibbs 1977. See Cameron 1984, Castles 1982, Maravall 1979, and Shalev 1983 for literature reviews.) The second method is to examine the limiting cases: the experience of governments that came into office with programs of major transformations, in particular, programs of simultaneous nationalization of some industries and redistribution of income (Kolm 1977). All such governments either compromised their original program, including total reversals, like that committed by the French Socialists, or provoked profound economic crises and were overthrown by force, as was the government of the Unidad Popular in Chile. These experiences prompted Rueschemeyer and Evans (1985, 62) to observe that "even in countries where the state appears to be in the strongest position relative to private capital, . . . the state remains dependent on private capital, foreign and domestic, not only to promote accumulation but also to produce a surplus in which the state itself may share. The strict limits under which the state must operate in a dependent capitalist economy are grimly indicated by the severe problems confronted by social democratic regimes such as those of Salvador Allende in Chile and Michael Manley in Jamaica." Yet again, these experiences cannot speak to the issue of limits and possibilities: one can always cite some counterfactual actions that would have perhaps avoided the disasters. The issue concerns possibility and possibilities cannot be determined on the basis of limited historical experience.

Hence, finally, the appropriate method to evaluate the hypothesis of structural dependence is to construct a formal model with which the internal logic of the theory can be explored and the robustness of its conclusions examined.

## The Theory Reconstructed

### Assumptions

To examine the validity of this theory, we need some assumptions. Suppose the society is composed only of wage earners and owners of capital. Wage earners consume all of their incomes and own no capital. There are firms, which receive the income from capital, invest some part of it, and distribute the rest to the shareholders. Let the net national income at time  $\tau$  be  $Y(\tau)$ , let aggregate wages be  $W(\tau)$ , let investment be  $I(\tau)$ , and let profits net of investment be  $P(\tau)$ . Then  $W(\tau) + I(\tau) + P(\tau) = Y(\tau)$ . Let us implicitly define the share of wages in the national product,  $m$ , as  $W(\tau) = mY(\tau)$ . The volume of investment is  $I(\tau) = s(1 - m)Y(\tau)$ , where  $s$  is the rate of investment out of the total return to capital,  $(1 - m)Y$ . The volume of profits net of investment is then  $P(\tau) = (1 - s)(1 - m)Y(\tau)$ .

We make the simplest possible assumption about production: output is a linear function of capital. Expressed in terms of growth,  $Y'(\tau) = \nu I(\tau) = \nu s(1 - m)Y(\tau)$ ,  $\nu > 0$ , where  $\nu$  is the constant productivity of capital. The parameter  $\nu$  represents the state of technology—the amount of additional output that can be produced when an additional unit is added to the capital stock. Thus, output grows at the rate of  $\nu s(1 - m)$  per year. For example, if  $\nu = .25$ ,  $s = .50$  and  $m = .80$ , the economy is growing at an annual rate of 2.5%.<sup>1</sup>

Firms make investment decisions. They are perfect agents of their homogeneous shareholders. Thus, firms maximize the utility that their shareholders derive from

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profits.<sup>2</sup> Shareholders derive their utility from present and discounted future consumption, where  $q$  is the rate at which the future is discounted. For the sake of algebraic simplicity, we assume shareholders' utility in each period is given by a logarithmic utility function.<sup>3</sup> Let  $P^*$  represent the present value of shareholders' welfare: the value they attach today to a particular sequence of present and future consumption. Then  $P^*$  can be written

$$P^* = \int_0^{\infty} e^{-q\tau} \ln[P(\tau)] d\tau, \quad q > 0. \quad (1)$$

Finally, wage earners derive their utility from consumption in the same manner.<sup>4</sup> Wage earners' welfare is given by

$$W^* = \int_0^{\infty} e^{-q\tau} \ln[W(\tau)] d\tau.$$

These assumptions are perhaps the simplest possible ones. Many can be complicated to introduce more descriptive realism without altering the qualitative conclusions. In the hope of remaining accessible to a wider audience, we assert the analytic results without proof in the text. Complete derivations are contained in the Appendix.

### Structural Dependence without the State

Consider the economy without state intervention. The state is there: it enforces contracts, standardizes weights and measures, and the like, but it does not touch income or investment.

Firms choose a rate of investment to maximize  $P^*$  in Equation 1 given that the share of wages in income is  $m$ . The solution to this problem (for  $m < 1$ ), as shown in the Appendix, is

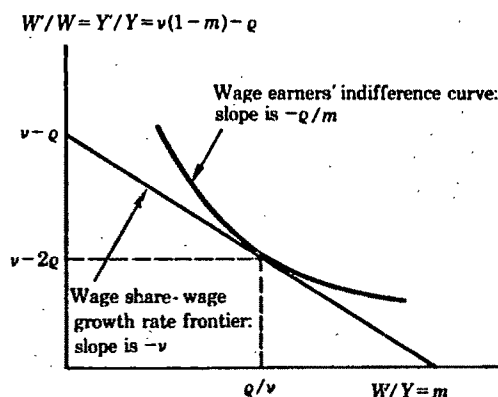
$$s^*(m) = 1 - \frac{q/v}{1-m} = s^M, \quad (2)$$

where  $s^M$  stands for "market rate."<sup>5</sup> This is the investment rate firms will choose in the absence of taxes. Note that the rate of investment falls as the share of wages increases,  $ds^*/dm < 0$ .<sup>6</sup> Note also that the rate of investment is always less than 1, and it may be less than 0. Firms may find it optimal to disinvest. Indeed, as  $m$  approaches 1,  $s^M$  approaches negative infinity. Note, finally, that without taxes, shareholders are consuming the income share  $(1-s^*)(1-m) = q/v$ , and firms are investing the share  $I/Y = 1 - q/v - m$  of national income. If  $q/v > 1$  or if the rate at which shareholders discount the future exceeds the productivity of capital, firms will disinvest, whatever the wage share.

Because shareholders always maintain their current consumption at  $q/v$  of income, any increase in the wage share must be exactly offset by a decline in the share of income going to investment:  $d(I/Y)/d(W/Y) = -1$ . Wage earners thus face a trade-off between the wage share and the rate of growth of national income (which determines the rate of growth of wages) as illustrated in Figure 1.

The straight line represents all possible combinations of the wage share and the

Figure 1. Wage Earners' Trade-off between Wage Share and Wage Growth





rate-of-wage growth as income is transferred from wages to investment and vice versa. If wage earners were organized in a centralized union federation with sufficient power to set wages unilaterally, they would maximize  $W^*$  by choosing the point on the wage share-wage growth rate frontier that is tangent to their indifference curve where the wage share equals  $m^{**} = q/v$ .<sup>7</sup> Regardless of the manner in which wages are determined, the freedom of firms to determine the rate of investment suffices to maintain shareholders' current consumption share constant. In the absence of government intervention, wage earners can only increase wages at the expense of investment and, hence, of future wage increases. Wage earners are structurally dependent.

### Structural Dependence of the State

Now let us consider a government that taxes and transfers income. The government imposes a tax either on income from capital or on wages and transfers the collected revenue to the other group. Taxes,  $T$ , are collected according to  $T = t(1 - m)Y$ , where  $t$  is the tax rate. If the tax rate  $t$  is positive, the tax is imposed on income from capital, and the revenue is transferred to wage earners. If the tax rate  $t$  is negative, the tax is imposed on wages, and the revenue is transferred to firms.<sup>8</sup>

Wage earners' share of national income is now  $(W + T)/Y$ , while the amount of net profits is

$$\begin{aligned} P &= (1 - m)Y - I - T \\ &= (1 - m)(1 - s - t)Y. \end{aligned}$$

The rate of investment firms choose now, given the tax rate and the share of wages in income, is

$$\begin{aligned} s^*(m, t) &= (1 - t) - \frac{q/v}{1 - m} \\ &= s^M - t. \end{aligned}$$

With a tax on income from capital, tax payments come entirely out of investments. With a tax on wages, tax payments go entirely into investments. Shareholders continue to consume the same share of income as before, since  $[1 - t - s^*(m, t)] = (1 - s^M)$ . The share of income that is invested with taxes is  $I/Y = 1 - m - q/v - (1 - m)t$ .

Thus the trade-off faced by the government between redistribution of income through taxation and investment is identical to the trade-off between wages and investment:  $d(I/Y)/d(T/Y) = -1$ . We can relabel the horizontal axis of Figure 1 to read  $(W + T)/Y = m + t(1 - m)$ , and use the same line to describe the possibilities facing the government as it raises or lowers the tax rate. The state is in the same situation as wage earners are.

What will different governments do when confronted with this trade-off? Because shareholders' welfare declines as the tax rate increases,  $dP^*/dt < 0$ , a probusiness government would tax wage earners and transfer the income to firms: it would generate growth financed by workers. A purely procapitalist government, a government which sought to maximize  $P^*$ , would tax wages at the maximum feasible rate. A purely prolabor government, on the other hand, which sought to maximize  $W^*$  would have the same indifference curves as wage earners in Figure 1. Such a government would choose the tax rate such that  $(W + T)/Y = m + t(1 - m) = q/v$ , or  $t = (q/v - m)/(1 - m)$ .<sup>9</sup>

Thus procapital governments favor growth over wage earners' consumption while prolabor governments prefer greater consumption for wage earners at the cost of slower growth. No government, however, can reduce the share of income that owners of capital consume. Any additional income for wage earners, whether it consists of wage gains won at the bargaining table or of transfer payments won through elections, reduces

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total investment, dollar for dollar. As long as  $q/v < 1$ , wage earners will moderate their claims on profits and so will governments representing wage earners.

Given this conclusion, it is not surprising that democracy appears so feeble when faced with the power of capital. "The institutional form of this state," write Offe and Runge (in Offe 1984, 121), "is determined through the rules of democratic and representative government, while the material content of state power is conditioned by the continuous requirements of the accumulation process." "The presumed sovereignty of the democratic citizenry," Bowles and Gintis (1986, 90) state succinctly, "fails in the presence of capital strike." The democratic process may bring governments with radically different bases of support into office but not governments with radically different programs.

### Is the State Structurally Dependent on Capital? Statics

#### Taxes on Consumption out of Income from Capital

These conclusions are based, however, on an assumption that need not be true of capitalism in general. This assumption is that the only policy instrument governments can possibly use is a tax on income. This particular policy instrument leaves the state structurally dependent because utility-maximizing shareholders respond to tax increases with less investment. But governments can adopt policies that alter the trade-off between investment and income distribution. The inference that because society is dependent on capital, so must be the state is false.

In fact, all governments in developed capitalist countries tax, at different rates, the part of income from capital that is in-

vested and the part that is consumed. Tax incentives for investment have been widely used, at least since the end of World War II. Depreciation allowances that differ from the rate at which plants and equipment actually wear out or become obsolete, investment credits, investment grants, special treatment of capital gains, and double taxation of profits distributed as dividends are only some of the most common deviations from a straight income tax. (For recent reviews of investment incentives in different countries, see Bracewell-Milnes 1980; Bracewell-Milnes and Huiskamp 1977; Commission of the European Community 1975; Hogan 1967; King and Fullerton 1984; Kopits 1981; Organization for Economic Cooperation and Development [OECD] 1979, 1983, 1984; Price, Waterhouse & Co. 1981.)

Let us distinguish the two taxes. The tax imposed on all income from capital (gross profits) is  $T_i = t_i(I + P) = t_i(1 - m)Y$ . We will refer to it as the "income tax." This is the tax we analyzed in the preceding section.

The tax imposed on shareholders' consumption is  $T_c = t_cP = t_c(1 - s)(1 - m)Y$ . This is the "consumption tax."<sup>10</sup>

Suppose now that a government adopts a pure consumption tax, that is,  $t_i = 0$  so that  $t_c(1 - s)$  is the tax rate applied to the income from capital  $(1 - m)Y$ . The most important effect of using this instrument is that the rate of investment is not affected by this tax rate, whatever it is, as long as wages are constant (Auerbach 1983). Indeed, as shown in the Appendix, for any rate of taxation of consumption out of profits, firms continue to invest at the market rate given by Equation 2:

$$s^*(m) = 1 - \frac{q/v}{1 - m} = s^M.11$$

We are thus already one step away from structural dependence: with this tax instrument a government can keep investment at its market determined rate and

distribute the remaining income of shareholders to wage earners. A pure prolabor government can set  $t_c$  close to 1, reduce shareholders' share of consumption almost to 0, yet keep the rate of investment unchanged at  $s^M$ .

This much is true when the wage share is fixed. But in some capitalist countries wage earners are highly organized in centralized union federations with the capacity to shape the private distribution of income. If a government uses a pure consumption tax, the union federation will maximize  $W^*$  by choosing the share of income given by  $m^{**}(t_c) = (1 - t_c)q/\nu$ . The willingness of the unions to engage in the political exchange described by Pizzorno (1978), Hibbs (1978), and Korpi and Shalev (1980), in which lower wages are traded for increases in welfare programs is apparent: as the tax rate, and thus transfers of income, increase, the federation reduces its preferred private wage share.

Because consumption taxes do not affect the rate of investment and do reduce the share of wages that centralized unions will demand and because the rate of investment increases as the share of wages declines (Equation 2), increases in the tax on consumption out of profit can increase the rate of investment. The rate of investment resulting from a tax on consumption of income from capital when wages are set unilaterally by centralized unions will be

$$s^*[m^{**}(t_c)] = 1 - \frac{q/\nu}{1 - (1 - t_c)q/\nu},$$

which is a positive function of  $t_c$ ,  $ds^*/dt_c > 0$ .<sup>12</sup> This, then, is the second step: in concertation with the union federation the government can increase investment.

Can it do both: reduce the income share consumed by shareholders and increase investment above market-determined levels? The answer is positive; but

because an even stronger conclusion can be sustained, let us go directly there.

### Workers' Welfare under Socialism and Democratic Capitalism

The strong result is a vindication of the traditional social democratic strategy, formulated perhaps for the first time by Bernstein in 1898, elaborated by Henrik de Man in 1933, adopted by the Swedish Social Democrats in the 1930s, and codified in the German Social Democratic Programme of Bad Godesberg in 1959, namely, that a proworker state can control the capitalist economy in a manner that would bring the material welfare of wage earners to the same level they could reach under socialism.

Under capitalism, owners of capital stock control investment, while wage earners may at most control the distribution of private income. The strategic problem organized workers confront under capitalism is thus to choose the share of income, given that someone else determines investment. Under socialism, wage earners would control both the share of income going to investment and that going to consumption. Since capital stock would be collectively owned, there would no profit incomes; and the society as a whole, through some preference-revealing mechanism, would decide the distribution of income into investment and consumption.<sup>13</sup>

Wage earners' strategic problem under socialism would thus be to choose their share of income, knowing that whatever income they do not consume is invested. Technically, this is the same as setting  $s = 1$  and choosing  $m$  to maximize  $W^*$ , which yields  $m^*(1) = q/\nu$ . The rest of income goes to investment, or  $I/Y = 1 - q/\nu$ . Associated with this allocation of resources is the level of wage earners' welfare under socialism  $W^*(S)$ . Given the technology and the time preferences,  $W^*(S)$  represents the highest possible level

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of wage earners' welfare—the level they could attain if they controlled investment as well as income distribution.

Back to capitalism: consider the case with one big union and a government that chooses a pure consumption tax with the tax rate  $t_c$  close to 1. As  $t_c$  approaches 1,

$$\begin{aligned} (W + T)/Y \\ &= [m^{**} + t_c(1 - s^*)(1 - m^{**})] \\ &\rightarrow q/\nu \end{aligned}$$

and

$$I/Y = s^*(1 - m^{**}) \rightarrow 1 - q/\nu,$$

which is the same as under socialism. As  $t_c$  approaches 1, wage earners' welfare under capitalism approaches the welfare they could obtain under socialism (Wallerstein and Przeworski, n.d.).

Would a government ever choose such a tax rate? To answer the question we need a language to characterize governments. Suppose the government's objective is to maximize  $G^*$ , which depends on the welfare of wage earners and shareholders according to  $G^* = (W^*)^\gamma (P^*)^{1-\gamma}$ ,  $0 < \gamma < 1$ . Thus  $\gamma$  is the weight attached to the welfare of wage earners and  $(1 - \gamma)$  is the weight given the welfare of shareholders. If  $\gamma$  approaches 1, labor's interests predominate; if  $\gamma$  approaches 0, the government exclusively favors capital.

The government's optimal tax on consumption out of income from capital is given implicitly by the equation

$$t_c = \frac{\gamma P^*}{\gamma P^* + (1 - \gamma)W^*}.^{14} \quad (3)$$

Thus, as long as  $0 < \gamma < 1$ , the government will choose to tax consumption out of income from capital at a rate also between 0 and 1. In addition, the greater the weight given to wage earners' welfare, the higher will be  $t_c$ , or  $dt_c/d\gamma > 0$ . Finally, as  $\gamma$  approaches 1,  $t_c$  also approaches 1 and wage earners' welfare approaches that associated with socialism. Note that

the share of national income that shareholders consume approaches 0, but shareholders' consumption in absolute terms may remain substantial. The conclusion is that when all wage earners are organized in one centralized union federation and the government is purely prolabor it will choose a tax on capitalist consumption the effect of which will be to bring wage earners' material welfare almost to the level they could obtain under socialism. Hence, at least when the union federation is willing to engage in a political exchange of private and public income, the state is not structurally dependent on capital.<sup>15</sup>

### Caveats and Preliminary Conclusions

These conclusions are subject to a number of qualifications: (1) the results may depend on restrictive assumptions about workers' and shareholders' preferences; (2) structural dependence may work through mechanisms other than investment; (3) the state may be dependent not on national but foreign investors; (4) interventionist governments may be inherently incapable of serving as perfect agents of any group's interests. Let us consider each in turn.

For the sake of mathematical tractability, we have worked with particularly simple specifications of the production and utility functions. Our conclusion does not depend on the assumption of a linear production function. Auerbach (1983) and Przeworski and Wallerstein (1985) present models of investment-neutral taxes on profits using the standard production function with diminishing returns to investment. Nor is our conclusion restricted to the case of logarithmic utility functions. The same results can be derived with any utility function with a constant elasticity of marginal utility.<sup>16</sup> We do not know, however, the generality of our results within the entire set of quasi-concave utility functions with which economists like to work.

Our results are sensitive to the assumption that firms care only about the present and future consumption of their shareholders. Owners of firms may not like encroachments on their prerogatives or the paying of taxes per se. Yet, the fact that owners of firms do not like something the government is doing, even if it touches them as profoundly as, say, a law increasing workers' rights at the workplace, is not sufficient to affect investment. Suppose firms maximize some more general utility function  $V(P^*, x)$  where  $P^*$  is, as above, the welfare derived from present and future consumption and  $x$  is anything else that causes distress ( $\partial V / \partial x < 0$ ) as the Left comes to power. "Investment strikes" will occur only if  $dx/ds > 0$ . Note that control over the workplace will not do for  $x$ , because it is independent of investment. Dislike of paying taxes will cause firms to increase, not decrease, their rate of investment in the presence of taxes on shareholders' consumption. Thus, likely candidates for  $x$  are not obvious.

Our results are also sensitive to the assumption that workers care only about their consumption. If workers determine the supply of labor by balancing their marginal utility of leisure with their marginal utility of income, the replacement of private wages with transfer payments financed by a tax on consumption out of profits will reduce incentives to work as it increases incentives to invest.

The second objection is that our conclusion is valid at most with regard to a specific mechanism of structural dependence, namely, the dependence of future material welfare on investment. One might postulate other mechanisms that would render the state dependent on capital, for example, the need to finance state activities through borrowing (see Marx 1952b, written in 1850). The mechanism most often suggested by Marxist theoreticians is the following: governments depend on current levels of economic activity, therefore they depend on

investment. This formulation is, however, incoherent: if governments are concerned with current levels of activity, current demand, or current employment, they can and would do many things other than worry about investment. In a Keynesian textbook they can reduce taxes on wages, increase public employment, or print money as well as reduce interest rates to stimulate investment. A concern with the short run would not lead to a preoccupation with investment. Investment matters mostly for the long run.

In general, however, it may be true that the state is structurally dependent through mechanisms other than the one analyzed here. A reasonable short-term model of dependence is yet to be developed, but our conclusion should not be taken to be more general than it is.

Third is the issue of international constraints. A clarification may be needed. In the analysis, export of capital was treated as equivalent to consumption from the point of view of the tax system: whatever is not invested in the particular economy is subject to the tax. Thus all that governments need to do, in order to use the tax system to induce domestic (as distinct from foreign) investment, is to ascertain the actual amount of domestic income and investment. And in spite of transfer pricing and other mechanisms by which firms can shift tax burdens from country to country, there seems to be a general agreement that by and large, governments do monitor international flows of income (Bracewell-Milnes 1980; Hartman 1985; OECD 1983). Governments that seek to attract new foreign investment are in a different and more constrained situation. High taxes on consumption out of profits that have no effect on the investment decisions of domestic firms (or of local subsidiaries of multinational firms) will deter new foreign investors. The most important and unresolved issue, however, concerns the range of choice that the government of any country has when domestic

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producers must trade in international markets in which competitiveness depends to a significant degree on relative labor costs.

Finally, we come to the problem of the limits of governmental competence. Government intervention in the economy creates a potential for vast inefficiencies in the allocation of investment and labor. Any system of investment incentives is vulnerable to inefficiencies due to differing effective rates on different assets (King and Fullerton 1984). Any system in which private consumption depends primarily on transfers rather than wages is vulnerable to inefficiencies due to misallocation of labor. The problem lies not only in the difficulty of designing neutral taxes or transfers but in their implementation. The very fact that the state acquires the capacity to intervene in the economy, that is, to affect material welfare of private actors, turns it into an attractive target of influence by these actors. What has thus developed as the eventual aftermath of the Keynesian revolution is a state that is strong in its capacity to intervene and that is highly vulnerable to private influence (Schmitter 1985, Skidelsky 1977). Diagnoses formulated within ideologically distant perspectives coincide in their anti-statist conclusions: Habermas's (1975) analysis of the crisis facing capitalism is identical to that of Stigler (1975). What may be thus wrong with our analysis is the assumption that interventionist governments can ever serve as perfect agents of the political forces that placed them in office. The economy is sick, the drugs are available, but the doctor may be a hack.

In sum, there may be limits to the ability of the state to redistribute consumption without discouraging private investment, because the economy depends on an inflow of capital from abroad (rather than the reinvestment of profit earned at home) or because private wages cannot be replaced by public income without undermining work incentives or because the

government cannot be trusted with the discretion required by redistributive policies. But all of these reasons are less general than the claims of the theory of structural dependence.

Our analysis reveals a large range of economically feasible policies. Exclusive reliance on income taxes, that is, taxes that are independent of the rate of investment—whether these are imposed on capital or labor—allows owners of capital to choose the share of national income they want to consume and to ward off, by reducing the share of investment, all attempts both by unions and by governments to redistribute consumption. Under exclusive reliance on income taxes, the consumption of all groups other than shareholders, government included, is thus subjected to "market discipline." In contrast, if only consumption out of income from capital is taxed, governments can redistribute consumption without reducing investment. Acting in concert with unions willing and able to trade off wage demands for increased transfer payments, the government can increase investment and reduce capitalists' consumption share simultaneously.

Governments can choose different policies concerning distribution and growth and these policies can be effective. Specifically, governments can increase investment by reducing wage earners' consumption or by reducing the consumption of owners of capital. Since all social groups will seek to shift the costs of investment onto others, the outcome of the democratic process can matter greatly for the welfare of different groups under capitalism. The state is not structurally dependent.

These conclusions may seem banal, but they run against a substantial body of established theory. The Marxist theory presented in the first section is logically persuasive and historically plausible. Moreover, we demonstrated in the second section that as long as governments limit

themselves, or are limited, to using a flat income tax, the state is structurally dependent on capital. And, importantly, Marxists are not the only theoreticians who come to the conclusion that the state is dependent: the language is different but this is the general thrust of theories of the state originating from the neoclassical economic theory, as exemplified by Peltzman (1976), Aumann and Kurz (1977), Busch and Mackay (1977), Becker (1983), Bates and Lien (1985), and innumerable others who conclude that all governments seeking support must anticipate the negative effect of taxes on private investment. Thus, there are good reasons to be theoretically surprised.

### Is the State Structurally Dependent on Capital? Dynamics

Once they are in office, prolabor governments can use tax policies to bring the welfare of wage earners arbitrarily close to the level wage earners could attain if they controlled investment directly: this is the central conclusion we have reached thus far. This conclusion, however, holds only once the prolabor government is in office—more precisely, once the policy is in effect. The question that remains is what happens to investment and wage earners' welfare when firms anticipate that a prolabor party would or might win the next election and institute such tax policies.

Since the mathematical analysis is quite complicated, we present only the framework of analysis and the conclusions. Suppose that at time  $\tau = 0$  a government characterized by  $\gamma_0$  is in office and the tax rate is  $t_c = t_0$ . The government is highly procapitalist and this tax rate is close to 0. An election is to take place and firms expect that this election will be won by a prolabor government characterized by  $\gamma_1 > \gamma_0$ , which will eventually introduce, at

time  $\tau = 1$ , a tax  $t_c = t_1 > t_0$ .<sup>17</sup> What rate of investment will be chosen during the period from  $0 \leq \tau < 1$  if firms expect that tax rate to be higher at and after  $\tau = 1$ .

When firms do not anticipate a change in tax policy, they choose the rate of investment that maximizes  $P^*(t_0)$  between  $\tau = 0$  and infinity. If, however, they anticipate that the tax will increase at  $\tau = 1$  and remain at the new level, they will choose a time path of investment  $s(\tau)$  to maximize

$$\int_0^1 e^{-e\tau} \ln[P(t_0, \tau)] d\tau + \int_1^{\infty} e^{-e\tau} \ln[P(t_1, \tau)] d\tau.$$

The conclusion is the following: firms that anticipate a future increase in taxes will reduce investment in the period  $0 \leq \tau < 1$  even if the tax is a pure consumption tax that will not lower investment in the period  $\tau \geq 1$ . The same holds for firms that are not certain about future tax policy but think taxes will rise with some positive probability. Thus, anticipations of certain or possible increases of taxes lead firms to lower their investment rate. Similarly, expectations of future tax decreases will cause current investment to increase. Pure consumption taxes are no longer investment-neutral when change is anticipated.

Thus "business confidence"—the expectation by firms concerning government policy—has direct consequences for the investment behavior of firms. Yet this conclusion should be distinguished from the view of Kalecki (1966; also Block 1977), who also saw in business confidence the constraint that any government under capitalism is forced to consider. In Kalecki's view, business confidence threatened investment even in a static case, when the government policy was already in place and no changes were anticipated. However, once a government

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has implemented a consumption tax, the investment rate is unaffected by the tax rate. It is the anticipation that a government would introduce or raise such a tax that erodes business confidence and lowers investment.<sup>18</sup>

We suspect that many newly elected proworker governments abandoned or moderated their programs for wrong reasons, having mistaken the cost of anticipation of policies for the costs of policies themselves. The French Socialist party announced in the electoral campaign of 1981 that it would propose a law in favor of renters. Once elected, it prepared such a bill. In the meantime, the vacancy rate went up and so did rents. Having listened to a chorus of right-wing press, the government decided that the policy was too expensive and moderated the provisions of the law. Our analysis suggests that this may have been a mistake. The government never waited to discover the costs of the law. Instead, it panicked over the costs of anticipation which, by then, were sunk.

Ever since Winston Churchill sneered in 1924 that the Labour party was not fit to govern, parties of labor have been making desperate efforts to show their moderation and responsibility in administering the economy once in office. Yet once the decision to adopt a policy has been made and once the government has suffered the effects of anticipation, the government is best-off introducing the original program without any compromises. The time to compromise is not after the costs have already been paid.<sup>19</sup>

Since anticipated transitions to higher taxes are costly for wage earners' welfare, socialists are sometimes tempted to proceed "in one stroke"—to use the phrase of Oskar Lange, who thought that if a government has decided to go ahead with a socialist program at all, "it must go through resolutely . . . at a maximum speed" (1964, 125; also Kautsky 1925). This is not a new idea: Machiavelli

advised, "When a prince takes a new state, he should calculate the sum of all the injuries he will have to do, and do them all at once, so as not to have to do new ones every day; simply by not repeating them, he will thus be able to reassure people, and win them over to his side with benefits" (1977, 28).

Our analysis suggests that socialist governments should never announce more than they intend to do and that they should do what they announced fully and quickly. Leon Blum was wrong to have waited to assume office after the Popular Front won the elections of 1936. Yet we do not know whether one stroke is better than many. Indeed, we cannot even determine whether or not workers' parties should seek to increase the tax on shareholders' consumption. We know that once instituted, an increase in  $t_c$  will increase wage earners' welfare until or unless the socialist government is defeated and the policy reversed. But we also know that anticipation of this moment causes a fall in investment and is costly to wage earners' welfare in the period before the tax increase goes into effect. Thus, the question remains open whether the welfare of wage earners is improved. If workers are better-off when prolabor governments adhere to the status quo, then the state is structurally dependent in dynamic terms: some distributions of consumption are unattainable because the transition costs are too high.

### **Structural Dependence and Socialist Policies**

The capitalist economy, in which owners of wealth and of the capacity to work make decentralized decisions about allocations of their endowments, regularly generates a number of effects that are experienced as profound deprivation by large segments of society. The market does not assure the material security of



anyone who does not own wealth and is not able to earn a living. The market generates drastic inequalities, including inequality of opportunity. Finally, in cases where the private rate of return diverges from the social one, the market allocates resources to uses inefficiently. This has been the traditional socialist view of capitalism.

The perennial question has been what, if anything, can be done about it. Social Democrats in several countries developed a full-fledged program for steering the capitalist economy in the interest of people who depend on income from employment or cannot earn an income sufficient to survive. By controlling the capitalist economy, the state run by socialists could rationalize the economy as a whole and orient it toward the general welfare, the Swedish Social Democratic minister of finance discovered in the 1930s (Wigforss, as summarized by Lewin 1975, 286). Because capitalism became organized, it could be administered by socialists in the interest of the people, argued the eventual German Social Democratic minister of finance (Hilferding 1981). Ironically, it was Keynes who provided the policy instruments for controlling capitalist economies and perhaps the most eloquent statement of the social-democratic position: "It is not the ownership of the instruments of production which it is important for the state to assume. If the state is able to determine the aggregate amount of resources devoted to augmenting the instruments and the basic reward to those who own them, it will have accomplished all that is necessary" (1964, 378).

Even in its heyday, however, the social-democratic strategy was the subject of at least skepticism if not hostility from those who were persuaded that no government, not even one elected by "the people," could control investment and income distribution as long as the instruments of production were privately owned. The thesis that the state is structurally dependent on capital was used by critics of social

democracy to argue that this strategy is unfeasible, since the ability of any government to determine the level of investment and the distribution of income is subject to a trade-off given by the self-interest of owners of the instruments of production.

Our contribution to this debate is as follows: Once in place, a tax on shareholders' consumption does not reduce investment. The state is not structurally dependent on capital in the sense that virtually any distribution of consumption between wage earners and shareholders is compatible with continual private investment. Yet anticipations of future threats to shareholders' consumption do reduce investment and impose costs on wage earners. Whether and when the future benefits to wage earners outweigh the costs incurred during the transition period remains an open question.

## Appendix

### Preliminaries

Given that the economy grows according to  $Y'(\tau) = \nu s(1 - m)Y(\tau)$ , the national income at any time is given by  $Y(\tau) = Y(0)e^{\nu s(1-m)\tau}$ . Wages, profits, investment, and taxes all constitute shares of this income as described in the text. All of our results are derived from the first-order conditions of a series of maximization problems. In all cases, it is easily verified that the second-order conditions for a maximum are satisfied.

### Capitalists' Problem

Firms choose  $s$  to maximize

$$P^* = \int_0^{\infty} e^{-\rho\tau} \ln[P(\tau)] d\tau$$

$$= \frac{1}{\rho} \left[ \ln P(0) + \frac{\nu s(1 - m)}{\rho} \right] \quad (\text{A-1})$$

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Note that  $P^*$  is only well defined when  $P(0) > 0$ . To insure that this condition is met, we assume throughout that  $m < 1$  or  $q/v < 1$ .

Without taxes,  $P(0) = (1 - s)(1 - m)Y(0)$ . Firms thus choose the  $s$  that satisfies

$$\frac{\partial P^*(s, m)}{\partial s} = \frac{1}{q} \left[ \frac{v(1 - m)}{q} - \frac{1}{1 - s} \right] = 0$$

which implies the choice

$$s^*(m) = 1 - \frac{q/v}{(1 - m)} = s^M. \quad (A-2)$$

Capitalists are consuming the share  $(1 - s^M)(1 - m) = q/v$  and investing the share  $s^M(1 - m) = (1 - m - q/v)$ .

With income and consumption taxes,

$$P(0) = [(1 - t_c)(1 - s) - t_i](1 - m)Y(0).$$

The first-order condition is now

$$\frac{\partial P^*}{\partial s} = \frac{1}{q} \left[ \frac{v(1 - m)}{q} - \frac{1 - t_c}{(1 - t_c)(1 - s) - t_i} \right] = 0,$$

which yields

$$s^*(m, t_i, t_c) = 1 - \frac{q/v}{(1 - m)} - \frac{t_i}{(1 - t_c)}. \quad (A-3)$$

In the case with only income taxes,  $t_c = 0$  and Equation A-3 reduces to

$$s^*(m, t_i) = 1 - \frac{q/v}{(1 - m)} - t_i = s^M - t_i. \quad (A-4)$$

Capitalists are consuming the share  $(1 -$

$s^* - t_i)(1 - m) = q/v$  and investing the share  $s^*(1 - m) = [(1 - m)(1 - t_i) - q/v]$ .

In the case with only consumption taxes,  $t_i = 0$  and Equation A-3 reduces to

$$s^*(m, t_c) = 1 - \frac{q/v}{(1 - m)} = s^M. \quad (A-5)$$

Now capitalists are consuming the share  $(1 - t_c)(1 - s^*)(1 - m) = (1 - t_c)q/v$  and investing the share  $s^*(1 - m) = (1 - m - q/v)$ . As  $t_c$  goes to 1, capitalists' consumption share goes to 0.

### Workers' Problem

Unions choose  $m$  to maximize

$$\begin{aligned} W^* &= \int_0^\infty e^{-\alpha r} \ln[W(r) + T(r)] dr \\ &= \frac{1}{q} \left[ \ln[W(0) + T(0)] + \frac{vs(1 - m)}{q} \right]. \end{aligned} \quad (A-6)$$

If there are no taxes and the unions treat  $s$  as exogenous,  $W(0) = mY(0)$  and the solution to Equation A-6 (for  $s > 0$ ) is given by

$$\frac{\partial W^*(m, s)}{\partial m} = (1/q)[(1/m) - (vs/q)] = 0,$$

which implies  $m^*(s) = q/vs$ .

If firms invest according to their best reply  $s^*(m)$  given by Equation A-2,  $vs^*(1 - m)/q = [v(1 - m)/q - 1]$  in Equation A-6 and the solution to the workers' problem becomes

$$\frac{dW^*[m, s^*(m)]}{dm} = (1/q)[(1/m) - (v/q)] = 0$$

with  $m^{**} = q/v$ .

With income and consumption taxes,  $[W(0) + T(0)] = \{m + [t_i + t_c(1 - s)](1 - m)\}Y(0)$ . If unions anticipate capitalists' response  $s^*(m, t_i, t_c)$  as given by Equation A-3, the workers' solution is given by

$$\frac{dW^*}{dm} = \frac{1}{q} \left[ \frac{1 - A}{m + (1 - m)A + t_c q/v} - \frac{1 - A}{q/v} \right] = 0 \quad (\text{A-7})$$

where  $A = t_i/(1 - t_c)$ . Solving for  $m^{**}$ ,

$$m^{**}(t_i, t_c) = \frac{(1 - t_c)^2 q/v - t_i}{1 - t_c - t_i} \quad (\text{A-8})$$

Note that when  $s = s^*(m, t_c, t_i)$ , workers' share  $(W + T)/Y = [m + (1 - m)t_i/(1 - t_c) + t_c q/v]$ . Therefore it follows immediately from Equation A-7 that when workers anticipate capitalists' best response  $s^*$  in choosing  $m$ , workers consume the share  $q/v$  of national income.

In the case with only income taxes,  $t_c = 0$  and Equation A-8 reduces to  $m^{**}(t_i) = (q/v - t_i)/(1 - t_i)$ . In the case with only consumption taxes,  $t_i = 0$  and Equation A-8 reduces to

$$m^{**}(t_c) = (1 - t_c)q/v. \quad (\text{A-9})$$

### The Government's Problem

The government's objective function depends on the welfare of workers and capitalists according to the equation  $G^* = (W^*)^\gamma (P^*)^{1-\gamma}$ ,  $0 < \gamma < 1$ . To find the government's optimal tax, we take the derivative

$$\frac{dG^*}{dt_j} = (W^*)^{\gamma-1} (P^*)^{-\gamma} \left[ \gamma (P^*) \frac{dW^*}{dt_j} + (1 - \gamma) (W^*) \frac{dP^*}{dt_j} \right] = 0 \quad (\text{A-10})$$

where  $t_j$  represents either  $t_i$  or  $t_c$ . Two cases were discussed in the text.

First we consider the income tax that would be chosen by a purely proworker government. Note that if  $\gamma = 1$ , then  $G^* = W^*$ . Thus, we substitute  $s^*(m, t_i)$  (Equation A-4) into  $W^*$  (Equation A-6) and differentiate to obtain

$$\frac{dW^*}{dt_i} = \frac{(1 - m)}{q} \left[ \frac{1}{t_i(1 - m) + m} - \frac{v}{q} \right] = 0,$$

which can be simplified to yield the solution  $t_i = (q/v - m)/(1 - m)$ .

Now consider the case with a pure consumption tax,  $t_i = 0$ , and with unions choosing  $m = m^{**}$ . Substitute  $m^{**}(t_c)$  (Equation A-9) and  $s^*(m^{**}, t_c)$  (Equation A-5) into  $W^*$  (Equation A-6) and take the derivative

$$dW^*/dt_c = 1/q \quad (\text{A-11})$$

Similarly, substitute  $m^{**}(t_c)$  and  $s^*(m^{**}, t_c)$  into  $P^*$  (Equation A-1) and differentiate

$$dP^*/dt_c = -t_c/q(1 - t_c). \quad (\text{A-12})$$

Combining Equations A-11 and A-12 with A-10 and rearranging terms yields  $\gamma(1 - t_c)P^* - (1 - \gamma)t_c W^* = 0$ , or

$$t_c = \frac{\gamma P^*}{\gamma P^* + (1 - \gamma)W^*}, \quad (\text{A-13})$$

which implicitly defines the optimal  $t_c$  (since both  $P^*$  and  $W^*$  contain  $t_c$ ). Thus,  $0 < t_c < 1$  (since  $0 < \gamma < 1$ ). It is readily checked that  $W^*$  does not explode as  $\gamma$  approaches 1. Therefore, as  $\gamma$  approaches 1,  $(1 - \gamma)W^*$  approaches 0 and  $t_c$  approaches 1. Similarly, it is easily checked that  $P^*$  does not explode as  $\gamma$  approaches 0. Thus as  $\gamma$  goes to 0,  $\gamma P^*$  goes to 0 and so does  $t_c$ . To find  $dt_c/d\gamma$ , differentiate Equation A-13 implicitly and rearrange terms to write

$$\frac{dt_c}{d\gamma} = \frac{(1 - t_c)P^* + t_c W^*}{\gamma P^* + (1 - \gamma)W^* + t_c/q} > 0.$$

also argue that these conflicts would have been less had Weber elaborated his liberal commitments in substantially democratic directions rather than the elitist direction he in fact chose.

In contrast to extensive commentary on Weber's philosophy of social science, the philosophical commitments of his social and political thought have not benefited from similar attention. There are a few notable and important exceptions, each of which I rely on here. Karl Löwith (1982) has developed Weber's general concern with individual meaning and responsibility in rationalized societies (cf. Merleau-Ponty 1974, chap. 1). Drawing on Weber's neo-Kantian context, Wolfgang Schluchter (1979, 1981) shows how Weber's social science presupposes an underlying concern with historical conditions that produce or undermine capacities for rational action (cf. Brubaker 1984). And Jürgen Habermas (1984) has raised the level of debate by reconstructing the broad conception of rationality that underwrites Weber's sociology of cultural development. What is missing from the literature, however, is an account of how these dimensions of Weber's thought produce a *political philosophy*; that is, criteria that would permit him to judge the relative merits of political regimes. Nor does the literature provide an account of how Weber's political philosophy relates to his *political theory*, that is, to his views on what kinds of political institutions are possible, given the nature of the contemporary world.

### Pluralist Liberal-Democracy and the Pluralism of Values

What we do find in the literature are attempts by commentators interested in Weber's political thought to extrapolate a political philosophy from his concern for Germany's political future, since it is here one finds his most committed political

arguments. Most strikingly in "Parliament and Government in a Reconstructed Germany" (1978a, app. 2) Weber argues for what we would today call a pluralist-elite model of democracy,<sup>1</sup> both as an appropriate model for Germany and as the best possible political system in mass, industrial societies (cf. Beetham, 1985, 2). Much like Schumpeter later, Weber redefines democracy to fit the facts of mass passivity and high levels of ignorance. He does so by looking at democracy as a mechanism for selecting leaders by institutionalizing competition between elites. From an ethical perspective, liberal-democracy turns out to be a neutral process that leaves out classical liberal-democratic values, values having to do with popular sovereignty and the good of participation. Weber's model is especially distinctive for the importance he placed on strong political leadership with a plebiscitary basis as a means for checking the power of, and providing goals for, increasingly expansive and powerful state bureaucracies (Beetham 1985; Löwenstein 1966, 26-27; Mommsen 1974).

The problem with extracting Weber's political philosophy from this model stems from comparing it to another prominent aspect of his thinking. Commentators are often taken with Weber's account of the impact of secularization on society and politics (Löwith 1982; Merleau-Ponty 1974, chap. 1; Portis 1983). In Weber's view, the breakdown of traditional sources of moral authority meant that there was a greater diversity of values in Western societies. For this reason, he thought that politics would inevitably play a greater role in mediating value conflicts than it had in the past and that political life would be increasingly marked by conflicts of subjective value commitments against backgrounds of coercion.

No doubt because of their prominence in his writings, most commentators take it for granted that Weber's views on the increasing pluralism of values dovetails

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## Weber's Liberalism

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with his pluralistic reformulation of liberal-democratic theory. David Beetham, one of the best commentators on Weber's political thought, writes that in

an age of competing, and finally irreconcilable, value positions, all philosophical basis to political order in a rationally defensible agreed morality had to be abandoned. Its place was taken by a purely formal or procedural justification for liberal order, in its ability to allow expression to the plurality of competing values within society at large. Weber's philosophical and sociological perspectives thus reinforce one another in a pluralist restatement of the liberal tradition. (1985, 4-5)

Other commentators make similar points about the moral function of leadership in Weber's liberalism (Eden 1984; Mommsen 1974; Strauss 1950, chap. 2).

Weber rejected natural rights and utilitarian foundations for liberalism, the former as insupportable metaphysics and the latter as incompatible with his neo-Kantian image of human dignity—a point to which I shall return. Since Weber never explicitly developed the neo-Kantian bases of his liberalism, his rejection of more traditional approaches has left the impression that his political thought consists primarily in his arguments for parliamentary government and strong leadership, combined with an ad hoc preference for individual freedom and initiative. For this reason, Weber's theory of leadership seems to emerge as a replacement for previously accepted natural-right and utilitarian grounds for liberalism. His liberalism thus seems to leave moral questions to be solved by political conflict. This is why commentators can argue that Weber in effect replaces the moral foundations of liberalism with the value decisions of leaders within the context of competitive parliamentary machinery. For conservative commentators like Leo Strauss (1950, chap. 2), Weber reflects the intrinsically nihilistic logic of liberalism. For liberals such as Wolfgang Mommsen (1974, 93), Weber's emphasis on leadership is a

dangerous departure from traditional liberal-democratic ethics and a testimony to his overriding commitment to German nationalism. For social democrats such as David Beetham (1985, 2-7), Weber represents the hollowness of democratic commitments within pluralist-elite theory. These commentators imply that when one chooses to view Weber's liberalism as political *philosophy*—that is, as a defensible set of ethical commitments regarding a desirable political order—rather than simply as a formal theory of leadership in the context of institutional checks and balances, one finds it deeply flawed.

These approaches miss the inner consistency of Weber's political philosophy. For this reason, they also miss crucial and informative ambivalences within his political thought as a whole, especially as they relate to our own political situation today. The problem is that commentators assume that to the extent Weber has a political philosophy, it is fully reflected in his pluralist-elite theory of democracy. The assumption is wrong: as I shall argue, although Weber's philosophical concerns are relevant to his pluralist-elite theory of democracy, they do not, by themselves, account for it.

### Weber's Nietzschean Problematic: Bureaucratic Nihilism

When a thinker's philosophical concerns are inexplicit, we can often reconstruct them by looking at their most fundamental problems. Doing so allows us to see what a thinker would have had to presuppose to see them as problems in the first place. Moreover, it also helps us to know what, in their terms, would have counted as solutions. In Weber's case, these problems are already well developed in the literature (e.g., Löwith 1982; Brubaker 1984; Habermas 1984; Schluchter 1981): they have to do with the progressive rationalization of Western culture,

thought, and institutions. These concerns pervade his writings on religion, law, economics, and politics.

In the case of Weber's political thought, then, we should not be surprised that the classical problems of liberal-democratic theory are of less immediate importance than what he identified as an increasing bureaucratic displacement of politics. He conceived the problem as one of a conflict between the formal (or instrumental) rationality of bureaucracy and the substantive (or value) rationality of politics. In Weber's usage, *instrumental rationality* refers to rationalities of means, the most effective way of achieving a goal. *Value rationality* refers to the rationality of goals themselves, that is, the internal coherence of the interpretive schemas that deal with the intrinsic worthiness of an end. The problem with bureaucracies, for Weber, is that they have no way of embodying value rationality. Bureaucratic rationality is means oriented, merely instrumental. On the other hand, what defines a responsible politics is precisely that political actors take into account the rationality of ends (Weber 1946, chap. 4). Politics ought to make different kinds of value rationalities into objects of discussion, choice, and at some point, power. But where bureaucratic power dominates, responsible politics becomes increasingly impossible (Weber 1978a, app. 2, sec. 2).

Weber's rather Nietzschean way of constructing the issue might be put as follows: in a secular society, a society without values accepted on the authority of faith, bureaucracy is an institutionalized form of nihilism. For Weber, however, it is not—as for Nietzsche—the demise of theological culture and the rise of science in itself that produces a disenchanted, nihilistic universe (Warren 1988, chaps. 1, 7). The disenchantment he identifies in "Science As a Vocation" (1946, chap. 5) is a constant cultural potential, but it is not just this: it is also reproduced and enforced by bureaucratic domination. On

the one hand, the external goals of instrumentally rational institutions are likely to have only the most tenuous relation to the goals that individuals pursue in everyday life, goals intimately related to motivation and meaning. On the other hand, the technical efficiency of a bureaucracy depends on training individuals to separate their individual value commitments from their workplace activities. In so doing, bureaucracies forcibly separate individual and institutional goals and combine this separation with a characteristic mode of domination: individuals are regimented, disciplined, and trained for impersonal devotion to the duties of their offices, which are in turn hierarchically structured according to codified rules and procedures (Weber 1978a, 1148–56).

The logic of Weber's argument suggests that the bureaucratic organization of everyday life destroys what he takes to be the secular basis of meaning in activities that are consciously chosen and guided. The cost of bureaucratic efficiency is life in a world with reduced scope for individual autonomy, responsibility, spontaneity, and initiative. Weber in effect borrows Nietzsche's views on how nihilism can affect individuals, but he interprets the problem politically. Nihilism is a result of institutions that systematically separate means and ends, at least where individual actions are concerned. This suggests—at least in principle—a close relation between the structure of institutions and the abilities of individuals to inhabit meaningful universes. As forms of social organization that systematically sever the rationality of means from that of ends, bureaucracies displace the kind of sovereign, responsible individuality appropriate to secular, liberal-democratic societies. In this sense, bureaucracies destroy the social and psychological soil of classical liberal-democratic values, explaining in part why Weber wrote despairingly of bureaucracy as an iron cage.

Weber's Nietzschean problems presuppose a politically significant commitment to persons as responsible agents, capable of meaningful, goal-directed actions. This ethical commitment provides Weber with a defensible and coherent liberal political philosophy. What keeps these commitments from appearing in Weber's pluralist-elite theory of democracy is that he thought bureaucratic power was inevitable in mass, industrialized societies, and for this reason classical liberal-democratic values could no longer exist, no matter how desirable they might be. The reason Weber's theory of political institutions is such a poor guide to the ethical commitments of his political philosophy is that he thought that contemporary conditions made it impossible to address these commitments politically.

What Weber thought *could* be addressed politically, however, was the closely related problem of organizational drift. Bureaucracies are superior means of achieving a given goal but incapable of determining the rationality of goals themselves. In Weber's terms, bureaucrats are technicians, responsible only for the means of implementing goals that are given and taken for granted. All other things being equal, their experiences and responsibilities train them to disregard the rationality of the organization's goals and the ultimate consequences of these goals. When bureaucratic power displaces political institutions—as they had in post-Bismarck Germany—they make what are in fact irresponsible political decisions under the guise of technical expertise (Weber 1978a, 990–94). In Weber's view, it was precisely such bureaucratic irresponsibility that had allowed Germany to drift into World War I (Weber 1978a, 1385–92, 1407–16). This is why he addressed his leadership theory of democracy to the problem of political leadership in a bureaucratized society while bracketing the deeper problem of bureaucratic nihilism as unsolvable.

### Politics As Value Choice and Politics As Conflict

Implicit, then, in Weber's problematic of rationalization, especially bureaucratic rationalization, is a deep commitment to politics in some positive sense, a notion of how political action ought to relate to persons. It is here that one finds a coherent set of liberal commitments in Weber, one for which he ultimately failed to find a political expression. Viewed negatively, politics is essentially "struggle" (Weber 1978a, 1414), the "striving to share power or striving to influence the distribution of power, either among states or among groups within a state" (Weber 1946, 78). Viewed positively, however, politics involves relating individual value rationalities to group choices, implying that recognition of persons together with processes of discussion, argumentation, and consensus lie behind exercises of power. Political actions are a distinctively human kind of social action: they combine instrumental and value rationality, and thus develop and express what Weber conceives as the human potential for teleological action and self-determination. For this reason, political activities are valuable in and of themselves, apart from outcomes.

In "Politics As a Vocation," for example, Weber makes it clear that politics is a meaningful activity only when it involves a control of destiny; only when, to translate into Marxian terms, politics is a historical activity. Political actors may not agree on value commitments, but positive political activities do require that one have value commitments—what Weber refers to as a *cause* or *calling* (Weber 1946, 117). To define politics only negatively, only in terms of conflict, would miss its distinctive qualities and potentials: the "mere 'power politician' may get strong effects, but actually his work leads nowhere. . . . It is a product of a shoddy and superficially blasé attitude toward the

power (Eden 1984; Strauss 1950, chap. 2). Support for this kind of position comes from Weber's observation that choices between values are inevitably individual and subjective. Rationality can clarify value choices, he argues in "The Meaning of 'Ethical Neutrality,'" but cannot provide universal grounds for making them (Weber 1949, 17-19). Each must choose his or her own god or devil, and this is what it means to live in a secular world (Weber 1946, 151-53; Brubaker 1984, 98-101).

If by *relativism*, however, these commentators mean that Weber sees no ways of distinguishing better and worse actions, then they have misinterpreted his position. Indeed, he protests that to understand his comments in this way would be the "crudest misunderstanding," arguing that they lead instead to a "very special type of ('organic') metaphysics" (1949, 18). Unfortunately, he says little about what this special type of metaphysics might be.

Nonetheless, Weber's position seems to include two interrelated claims. Habermas (1984) has elaborated the first in his *Theory of Communicative Action*. On Habermas's account, Weber does not seek one set of value criteria to ground all others because he sees cultural progress in the differentiation of value spheres, none of which are primary and each of which has its own distinctive kind of rationality and criteria of propriety. This is true for economics, aesthetics, erotic life, language, intellectual life, social life, and so on. Each sphere is necessary for a fully human life, and it would be inappropriate to universalize the standards of one sphere to all others. One does not judge art by logical consistency, love by utility, or righteousness by efficiency. To demand that all value spheres reduce to one would be to empty life of its substance, to flee from life into a world of formal and lifeless standards (Weber 1949, 17), a point that has been revived recently in

ethical philosophy (cf. Williams 1985). At the same time, however, Weber does not suppose that these spheres of value will necessarily coexist in harmony. One is often faced with absolute choices between value rationalities, and this is why one who is fully conscious of his or her fate will know that there are no compromises between God and the Devil (Weber 1949, 18).

Yet Weber's position is not a simple relativism, because it is based on a view of what humans are, such that their lives could be simultaneously rooted in differentiated and autonomous spheres of value. This is presumably where one finds the special kind of organic metaphysics that he alludes to but does not develop. Weber's commitment to a specific conception of human activity becomes clear in a second claim, and this is what ultimately gives ethical content to his notion of political action. In the same paragraphs in which he writes of the inevitable plurality of values, he also argues that humans have an imperative to "choose" between values when they conflict rather than live semiconsciously and muddle through. "The fruit of the tree of knowledge," he writes, "which is distasteful to the complacent but which is, nonetheless, inescapable, consists in the insight that every single important activity and ultimately life as a whole, if it is not to be permitted to run on as an event in nature but is instead to be consciously guided, is a series of ultimate decisions through which the soul—as in Plato—chooses its own fate, that is, the meaning of its activity and existence" (Weber 1949, 18). At one level, of course, Weber's emphasis on decisions seems to support the idea that standards of conduct are merely subjective. At a deeper level, however, his imperative clearly reflects a Kantian conception of persons, a view that human dignity resides in the ability to choose consciously, through which one manifests oneself as a free and rational agent (cf.



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Brubaker 1984, 95–101). This is not surprising, since the academic atmosphere of Weber's Germany was permeated by neo-Kantian ethics and epistemology, a fact often overlooked in Anglo-American commentary.<sup>2</sup> It is in terms of this background that the ethical commitments of Weber's political philosophy begin to emerge.

Kant's approach to freedom was distinctive because of his equation of reason, moral choice, and freedom: freedom is our capacity to use our reason to guide our behavior. As Charles Taylor (1985, 318–21) has pointed out, what proved attractive about Kant's understanding of freedom when compared to earlier liberal thought is that he radically affirmed self-determination, but without either privatizing the self, or interpreting freedom as the victory of impulse over reason. Kant's moral schema thus shifted liberal thinking away from natural-rights approaches, and toward a focus on how value choices constitute humans as rational agents, especially with regard to the way moral choices are manifested in actions. Kant opposed moral schemas that prescribe values—whether by means of religious or metaphysical dogmas or through various kinds of social utilitarianism that ultimately require institutions to distribute values. Such external prescriptions violate the individual's ability to choose freely. In the case of moral values, actions cannot be said to have moral worth unless they result from free choice. Because for Kant humanity is defined by the capacity for moral choice, schemas and dogmas that equate moral action with prescribed behavior violate the personhood of individuals (Kant 1970, 73–86). Weber's concept of personality follows these broadly Kantian lines of reasoning: like Kant, Weber finds an ultimate value in freedom construed as rational agency (or *personality*, in Weber's terms). And like Kant, he construes practical reason (moral choice) as a condition of this kind of freedom

(Weber 1949, 15–18; Löwith 1982, 43; Schluchter 1979).

One might still argue, however, that Weber's position is implausibly relativistic because he believes—unlike Kant—that a commitment to a rational teleology of personhood is, in the end, a matter of faith (Weber 1949, 16–17). But what "faith" means in this context is simply that all arguments involve axiomatic premises that cannot themselves be rationally demonstrated. What separates Kant from Weber is that Kant held that his premises were exhaustive, and that this commits all humans to them insofar as they are potentially rational beings. Weber simply avoids universalizing his claim because he understands that one cannot argue rationally for the axiom that rational capacities define a fully human life. To do so would be to presuppose what one is trying to demonstrate.

Still, one might make the case (I will not here) that while Weber's axioms are a "matter of faith" in that they are (formally) a priori, they have the quality of all good axioms in that they are manifested in the activity of rational argumentation itself. Such an argument would follow Habermas's development and critique of Weber in *The Theory of Communicative Action* (1984) from the perspective of his "discourse ethics." On Habermas's account, the formally binding quality of the premise resides in the *process* of argument about moral commitments and not in a prior agreement about their meaning and content. Thus, speaking implies a commitment to the personality of others qua rational agents. Without this premise, there is nothing more to be said, except to note the empirical fact of different value rationalities. This may be (and almost always is) a political problem, but this should not be confused with the formal structure of Weber's commitment to personality nor with the implied universality of these commitments. It is in this context, then, that one should under-

stand Weber's argument that value choices *as such* and the "belief in the validity of values" remain necessary for the possibility of personality as something developed in and through actions. This is so even if, for Weber, the validity of value choices remains "subjective."

What is important to notice, then, is that what is not subjective for Weber, and hence not relativistic, is the possibility—indeed, the existential and moral necessity—of subjectivity itself. "Certainly the dignity of 'personality,'" he writes, "lies in the fact that for it there exist values about which it organizes its life;—even if those values are in certain cases concentrated exclusively within the sphere of the person's 'individuality,' then 'self-realization' in those interests for which it claims *validity as values*, is the idea with respect to which its whole existence is oriented. Only on the assumption of the belief in the validity of values is the attempt to espouse value-judgments meaningful. However, to *judge the validity* of such values remains a matter of faith" (Weber 1949, 55). What Weber is saying here is that although the specific content of value judgments is "a matter of faith," *as humans* we share an interest in a meaningful existence (cf. Weber, 1946, 275). Realizing a "meaningful" existence depends on orienting actions in terms of value judgments, that is, "organizing one's life" in relation to values. "Personality" is the result of a life so organized. This claim, with its implicit moral teleology, is part of Weber's ontology of social action (or, as he puts it, his "special kind of ['organic'] metaphysics") and part of his understanding of what humans are.

It is this broad claim about the imperatives of value choice that Weber construes in terms of an axiomatic commitment to rationality. Thus his normative definition of personality focuses on the "consistency of its inner relationship to certain ultimate values and meanings of life, which are turned into purposes and thus into teleo-

logically rational action" (Weber 1975, 192; cf. Löwith 1982, 45–46; Portis 1978, 113–20; Schluchter 1979, 73–74; Weber 1946, 151–53). In this way Weber equates meaningful action with what is potentially rational, in the sense that one who is living a meaningful life can, in principle, provide a consistent account of his or her actions in relation to both values and circumstances (Schluchter 1979, 76–92), in this way combining instrumental and value rationality in practices. His concept of rational agency requires not only that individuals make conscious choices between the demands of conflicting value spheres but also have a clear understanding of the means and contingencies of action.<sup>3</sup> The political significance of these points is that they imply that persons' capacities for rational agency are intrinsically valuable and therefore ought to guide and limit political judgments.

Although Weber's ethics develops in the spirit of Kant in these respects, he departs from Kant in another. While Kant holds that freedom implies the universalizability of maxims of conduct, Weber's universal commitment attaches to the inner consistency of values and their integration with empirical conditions of action. His departure from Kant in part stems from his view that there are many possible value rationalities, each closely related to its cultural context. This point reminds us of Hegel's critique of Kant's categorical imperative as implausibly formal. But equally importantly, it represents Weber's emphasis on the consequences of action. There are a multitude of worldly situations, especially in politics, in which there are no unambiguously right courses of action. As Weber's contrast between an "ethics of responsibility" and an "ethics of ultimate ends" in "Politics As a Vocation" suggests, Kant's universality is possible only in intentions, not outcomes (Weber 1946, 118–27). Those who would act according to an ethics of ultimate ends (Kant is only one example)

act only on the basis of the inner consistency of value systems. They consider any action "good" only to the degree that it reflects value-rational maxims of behavior. But value-rational intentions are not sufficient to rational action because one can hold them without regard to the contingencies and consequences of action. When one fails to link intentions to worldly consequences in an instrumentally rational way, good intentions can and often do produce quite contrary results. "Ethics of ultimate ends"—that is, ethics that are strictly intentional and value rational—are not sufficient to the teleologically rational action that defines personality. An "ethic of responsibility," on the other hand, requires that persons strive for long-term consistency between value commitments and outcomes, a consistency that develops and manifests personality.

Karl Jaspers has suggested, correctly I believe, that the effect of Weber's appropriation of Kant was to deemphasize formal rules of conduct and draw out the responsibility for consequences implicit in Kant's formulation of the moral question. He comments that the

opposition between principle and results as a basis of ethics is not tenable. The ethics of principle itself demands that in concrete action guided by the categorical imperative, one should bear the consequences in mind. The true basis of ethics is a third factor, which Kant, who obviously has it in view, does not expressly mention but which Max Weber brings out clearly in his 'ethics of responsibility.' Ethics of responsibility is the true ethics of principle. It does not take mere results or rational principles as its guide but seeks its way in the open arena of possibility, pursuing an absolute that is not manifested through any material content, but only through thought in actions. (Jaspers 1962, 72-73)

Weber in fact makes essentially the same point with respect to Kant in "The Meaning of 'Ethical Neutrality'" (1949, 16-17).

Weber emphasizes, however, that an ethics of responsibility is not a recipe for right action, especially in politics. Actors

will inevitably find themselves confronted with ethical conflicts between means and ends. That the world has amoral qualities means that politics will usually have a tragic dimension. Weber's ideal personality would be conscious of this element of the human condition and accept it without paralysis or cynicism: "It is immensely moving when a *mature* man—no matter whether young or old in years—is aware of a responsibility for the consequences of his conduct and really feels such responsibility with heart and soul. He then acts by following an ethic of responsibility and somewhere reaches a point where he says: 'Here I stand; I can do no other.' That is something genuinely human and moving" (1946, 127).

It is in remarks such as these that one finds Weber's radical reappraisal of politics: no longer is it simply a necessary evil, but rather it is a positive and definitively human activity (cf. Löwith 1982, 47). Politics is the most demanding test of an ethics of responsibility, and at the same time it is the kind of activity through which personality is cultivated and manifested as the expression of responsibility. Insofar as politics has this potential within the human condition, it is valuable in itself. It is this morally significant sense of politics that guides and structures much of Weber's thinking and no doubt accounts for much of his concern with the bureaucratic displacement of politics.

### Weber's Commitment to Persons: Elitist or Democratic?

Yet what is problematic in Weber is whether his politicized, neo-Kantian ethic could, in principle, be expressed in political institutions. In this respect Weber's departure from Kant remains significant. Kant's position on this issue was clear: there is no *necessary* conflict between individual freedom and agreement on uni-

versal standards of conduct, that is, between practical reason and political right. What morality requires of individuals is not agreement on external goods and values but rather agreement that the choices people make not involve treating others only as means. Kant's rule for accomplishing this was, of course, the *categorical imperative*: the notion that the guiding maxim of an act ought to be put to the test of universality. In principle, Kant (1964; 1970, 132-33) considered the respect for persons as free and rational agents expressed by the categorical imperative to be a possible, sufficient, and intrinsically worthy goal for a liberal society.

It is here that one finds a genuine political indeterminacy in Weber's ethical position. In contrast to Kant, Weber simply avoids universalizing his commitment to persons as rational agents, even within societies. For this reason, his commitment to persons produces an elitism rather than the liberal-democratic position that follows from Kant. What one finds in Weber is a heroic interpretation of personality: personality requires a strength, intelligence, commitment, and realism that he believed only a few in society could ever achieve. Combining this heroic interpretation with the notion that hierarchies of domination are inevitable in any society, Weber arrives at what Brubaker (1984, 97-98) correctly describes as an aristocratic morality, one that is only very marginally liberal and not at all democratic. In "The Meaning of 'Ethical Neutrality,'" for example, Weber writes that "only one thing is indisputable: every type of social order, without exception, must, if one wishes to *evaluate* it, be examined with reference to the opportunities which it affords to *certain types of persons* to rise to positions of superiority through the operation of the various objective and subjective factors" (1949, 27).

How fundamental are Weber's claims here? If I am right about the structure of

Weber's ethical commitments, his elitism is one possible elaboration of his commitment to persons. This would mean that the elitism is not fundamental. Instead, it is all that is left of his liberal ethics, with its respect for persons and its ethically significant sense of politics, when he combines it with an analysis of the realities of bureaucratic domination in mass, industrialized society. If so, an originally liberal and substantially democratic political ethos would have been overtaken by Weber's assessment of the realities of power in modern society, producing a marginally liberal elitism.

The point of my argument is not, of course, somehow to save Weber from himself. Rather, it is that looking at Weber in this manner makes him interesting in a way that he would not be if his elitism were simply a matter of taste and personal antipathy for—and perhaps fear of—the masses, as Beetham, for example, suggests (1985, 111-12). This approach shows how Weber's narrowing of his ethical commitments to persons represents what was certainly a political reality in his day and remains so in our own, namely, a stark contrast between liberal-democratic commitments to persons, and the power relations within liberal-democratic societies that undermine the social conditions of these commitments. Reading Weber in this way allows us to learn something about ethical dilemmas of liberal-democracies in mass-administered societies.

In order to define more clearly what is at stake, let us perform the thought experiment of separating Weber's ethics from his analysis of political possibility. What would a good society look like then? A good society would treat politics as something essential to the personhood of its members, assuming we understand politics to include all activities relating to rational agency in social situations. If, therefore, we were to define a good society solely in terms of Weber's ethical com-

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mitments to persons, we would have defined its desirability in terms of its substantially democratic aspects. Only this kind of society would be desirable in a Nietzschean world where there are no transcendental sources of value, because only such a society would allow individuals to give a meaningful durability to the world of their everyday activities. Such a society would not, of course, be devoid of conflict, tragedy, and violence. Nonetheless, as Weber himself points out (1949, 26-27), although social conflict is inevitable, there are many ways for it to be peacefully expressed. The problem for a society that would express Weber's value commitments, then, would be, first, to institutionalize the respect for persons within large-scale institutions without imposing agreement. Second would be to maximize dialogical possibilities of agent formation within small-scale ones (cf. Macpherson 1977, chap. 5; Mansbridge 1981; Habermas 1979, chap. 3). In this way, Weber's problematic of bureaucratic nihilism and his political ethics produce an imperative for substantive democracy quite at odds with the pluralist-elite model of democracy he actually advocated.

### Pluralist-Elite Democracy and Democratic Values

When Bismarck's rule came to an end in 1890, he left, in Weber's words, a nation "*without any political sophistication*" (1978a, 1392; cf. also 1385-91, 1407-16). It was in the context of a post-Bismarck Germany that had drifted into world war that Weber developed his pluralist-elite model of democracy. His outstanding concern was with the bureaucratic displacement of politics and domination of technocrats. The outstanding features of the model were, first, a strong, even Caesaristic political leadership based on

the plebiscite and limited by parliament. Second, Weber expected that this formula for leadership would produce an equilibrium of countervailing forces between political leadership and bureaucratic power, an equilibrium that would ensure a relatively open and dynamic society (Weber 1978a, 1414-17; Beetham 1985, chap. 4).

It is to be expected, then, that commentators on Weber's views of democracy would focus almost entirely on the question of leadership and elite competition. Karl Jaspers, for example, writes that the "primary concern of his political thinking was how to create an active harmony between democracy—which was inevitable anyway—and the authoritative leadership of truly able and responsible statesmen" (1964, 201). Wolfgang Mommsen who, unlike Jaspers, is critical of Weber, notes that he "was an advocate of democracy on the grounds that, under the social and political conditions of a modern bureaucratic society, it offered a maximum of dynamism and leadership. The classical doctrine, however, meant little to him. He did not believe in the theory of the sovereignty of the people" (1974, 87; cf. Kilker 1984). Weber saw the democratic process as a "competitive struggle of various political leaders for the support of the people." In this sense, Mommsen points out, Weber's theory of democracy is clearly the "liberal model of competition" transplanted "from economics to the field of parliamentary mass democracy" (1974, 88; cf. Macpherson 1977, chap. 4). Similarly, David Beetham notes that Weber originated the "theory of competitive elitist democracy that gained such widespread acceptance within political science in the 1950s and 1960s" (1985, 2).

From the perspective of Weber's political ethics, his view of democracy as a mechanism for leadership selection would seem to be quite consistent with his neo-aristocratic view that societies should be judged in terms of the opportunities they

afford for superior leadership (Weber 1949, 27). Weber himself does not even attempt to justify his model in democratic terms. He does so instead in terms of liberal, elitist, and nationalist values (Beetham 1985, chap. 4). Through checks and balances, it would limit the power of government to interfere with individual liberties. By providing an arena of political competition, it would select for the most able leaders. Finally, it would provide the German state with the capacity to assume a place in world politics, allowing it to protect its own national culture as well as those of the weaker nations of central Europe.

It would, in fact, have been much more difficult for Weber to have justified this model in terms of his political ethics of personhood with its substantially democratic implications. That this is so can be seen from Weber's own assessment of the plebiscitary basis of leadership. He insists that if leadership is to have any democratic meaning whatsoever, it would follow from the fact that legitimacy is "formally derived from the will of the governed" (Weber 1978a, 268). Even pluralist-elite democracy rests on the voluntary and autonomous consent to the leadership of particular individuals. That this is important to Weber's model becomes clear when he defines democratic legitimacy in the first part of *Economy and Society* as an antiauthoritarian species of charismatic legitimacy, writing that the "basically authoritarian principle of charismatic legitimation may be subject to an anti-authoritarian interpretation, for the validity of charismatic authority rests entirely on recognition by the ruled, on 'proof' before their eyes. . . . When the charismatic organization undergoes progressive rationalization, it is readily possible that, instead of recognition being treated as a consequence of legitimacy, it is treated as the basis of legitimacy: *democratic legitimacy*" (1978a, 266-67).<sup>4</sup>

Although Weber only intended his

description of democratic legitimacy to elaborate a sociological category, his definition turns on a distinction between authoritarian and democratic charisma that has a normative importance. According to Weber's definition, one should speak of democratic legitimacy only where recognition of a leader *precedes* the relationship of legitimacy. The democratic content of Weber's pluralist-elite model of democracy, then, is in the fact that democratic legitimacy presupposes a more or less considered decision-making process on the part of the governed.

In the context of Weber's broader sociology, however, one finds that even this minimally democratic substance—consent to being ruled by another—would have to exist under very special circumstances if it were not to be merely a formal appearance of democracy. *Voluntary consent, autonomy, and even will*, imply a consciously considered choice. But the status of "choosing" in Weber's model is tenuous. From the perspective of his sociology of mass, industrial societies, it is precisely the existence of individuals as rational agents capable of making choices that is problematic. Weber did not, like many liberals, believe that individuals are necessarily rational utility maximizers. As a sociologist, he observed that most mass followings of leaders involve "purely emotional and irrational" factors (1978a, 1459). Moreover, Weber noted that such followings are especially characteristic of modern mass democracies. It was for this reason that he fully expected plebiscitary leaders to use demagogic techniques to solidify their followings. Very often such leadership evokes mass behavior—something Weber declines to describe as "social action" because of its lack of intentional character (1978a, 23).<sup>5</sup> This is why he thought that the consent elicited by a plebiscitary leader would in fact be "only formal and fictitious" (1978a, 267).

My aim is not, of course, to fault

Weber's descriptions of plebiscitary leadership from a sociological perspective. The point here is that if Weber's sociological considerations are correct, then they blur the limited sense in which it makes sense to describe such political processes as democratic. Surely Weber was aware of this, lending credence to Mommsen's suggestion that for Weber "democratic institutions are a merely functional machinery" in the hands of charismatic leaders, pushing him dangerously close to the "fascist leadership principle" (1974, 93). Although Mommsen may overstate the case, he is right that from the perspective of substantial democracy (as opposed to Weber's own emphasis on the formal mechanisms of democracy) it is very difficult to distinguish between voluntary consent and mass behavior. This is why Mommsen can plausibly argue that Weber fails to distinguish between charismatic leadership and charismatic domination, on which the difference between liberal-democratic leadership and demagogic dictatorship presumably depends. Similar considerations have led Jürgen Habermas to characterize Weber's concept of leadership democracy as a somewhat desperate attempt to break the powers of bureaucratization by means of a "decisionist self-assertion in the midst of a rationalized world" embodied in a strong-willed leader's instinct for power (1971, 65-66).

Clearly there is little conflict between this institutional model and Weber's elitist and nationalist sentiments. But it is also clear that the model gives little or no expression to the ethical commitments of his political philosophy, those I have characterized as based on a commitment to persons and a positive conception of politics, oriented toward bureaucratic nihilism and having substantially democratic implications. Thus it remains to be asked whether Weber's pluralist-elite model was the *only* way he could have institutionalized his political ethics.

### Limits to Democracy: "The Principle of Small Numbers"

As J. J. R. Thomas (1984) has recently argued, it was not for lack of considering possibilities of direct democracy that Weber took the position he did. The fact that he considered and rejected at least some substantially democratic alternatives makes his position all the more challenging for democratic theory. His argument against substantive democracy was based on two claims: the first and most obvious problem is the size of modern societies; the second and more compelling problem has to do with bureaucratic power. But while Weber's "impossibility" arguments are crucially insightful, especially in the latter case, they are not decisive.

With respect to the question of size, Weber held that direct democracy is possible only in small, relatively undifferentiated and simple societies. The problem with democratizing political decisions is that actual political decision making is always governed by the "principle of small numbers," which expresses the "superior political maneuverability of small leading groups" (1978a, 1414). The tendencies toward exclusiveness in political decision making grow as societies become larger and more complex, suggesting that under modern circumstances direct democracy is impossible.

Does this mean that, in Weber's terms, substantive democratic values can have no place in modern societies? Although this limitation may relate to *direct* democracy, it does not so clearly apply to less demanding forms of *participatory* democracy. Weber's positive political ethos would not require that every individual participate in every political decision. What it would require is that individuals participate directly in those decisions that most immediately affect their everyday activities. The reason is that, in Weber's

terms, politics can have positive qualities only where it relates to the "local" development of persons, and to their senses of freedom and self-determination (cf. Weber 1946, 155). Thus, while direct participation in foreign-policy decisions may not be feasible for large numbers of individuals (one of Weber's major concerns in "Parliament and Government in a Reconstructed Germany"), it also has less immediate importance for a positive politics than, say, power in the workplace, neighborhoods, consumer cooperatives, and the like. On the other hand, representative mechanisms would be both feasible and sufficient in those larger-scale decisions that very often determine the contingencies of everyday life, including foreign-policy decisions, investment and production decisions in large-scale industries, decisions on macroeconomic policy, and so on. What Weber's political ethics demands, in other words, is participation appropriate to the level of organization at which decisions are taken. This is the option that he does not consider.

That Weber did not seriously consider these kinds of participatory options shows up in the fact that when he considers socialist principles of economic organization, he looks only at "cartels of producers," and "cartels of consumers," both of which would require centralized state control to be effective (1978b, chap. 12, cf. Weber 1978a, 1454). What Weber fails to consider in this context is democratization of production and other spheres of society in ways that would extend opportunities for participation beyond the spheres of government as they are defined in liberal societies. He does not do so, it seems, because he sees the increasing bureaucratization of all spheres of society as inevitable.

### **Limits to Democracy: Bureaucracy**

Weber's more compelling argument, then, has to do with the inevitability of

bureaucratic power in mass, industrialized societies: "As soon as mass administration is involved, the meaning of democracy changes so radically that it no longer makes sense for the sociologist to ascribe to the term" *democracy* the meaning it has in situations of direct democracy (Weber 1978a, 951). Weber's argument includes three kinds of reasons why bureaucracy limits democracy.

The first has to do with conflict between government by law and democratic control. Weber held that there is an intrinsic tendency toward bureaucratization with the rise of democratic demands for rational and consistent treatment by government (Thomas 1984). Democratic legitimacy minimally involves equality before the law and hence a legal-rational structure of rules. Administering these rules requires that some body of individuals have powers of interpretation and enforcement, which in turn leads to legal-rational domination. Yet "democracy" as such is opposed to the 'rule' of bureaucracy, in spite of and perhaps because of its unavoidable yet unintended promotion of bureaucratization" (Weber 1978a, 991). Weber's insight into the development of formal democratization is that insofar as it involves a "leveling of the governed in the face of the governing and bureaucratically articulated group," the bureaucracy may "occupy a quite autocratic position, both in fact and in form" (1978a, 985; cf. Cohen 1972, 72-77). It is precisely this "passive" democratization that leads to the tension between democracy and bureaucracy. At best, Weber believed the effects of rationalized administration could be stemmed through "political democratization," by which he meant those constitutional mechanisms designed to prevent "the development of a closed status group of officials in the interest of universal accessibility of office," and to minimize "the authority of officialdom in the interest of expanding the sphere of 'public opinion' as far as practicable" (1978a, 985).



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The problem of rule-based administration is compounded by a second problem, namely, the nondemocratic character of expertise. Here, the issue is not, as in the first case, one of an internal contradiction within democracy, but the more familiar one of a trade-off between the advantages of a technological society and the good of self-government. Where "a group of specialists is present," Weber writes, "no matter how strongly the attempt is made to keep them in a dependent position, the seeds of bureaucratization are present. Above all, such persons can neither be appointed nor dismissed according to the procedures appropriate to immediate democracy" (1978a, 290).

Yet in both cases one can imagine ways of arranging complicated societies so that powers of administration and expertise are checked and divided so as to make them less threatening to democracy. The question is not whether authorities should exist, but whether they transgress the boundaries appropriate to the kind of authority they possess. On this principle, I may be subject to the authority of someone else in one sphere of existence (for example, to a policeman's legal authority when I am driving, to a doctor's expert authority when I am sick, and so on). But I might be an authority in other spheres, and an equal in spheres that permit equality. Indeed, the public spheres of liberal-democracies already embody some of these distinctions. The point would be to extend them to bureaucratic organizations throughout society, assuming ways could be found to make them effective. Thus, the overall effect could be a society that is highly differentiated and pluralistic, and yet relatively equal with regard to the values of personhood (cf. Walzer 1983). Certainly Weber's pessimistic image of bureaucracy as an "iron cage" cannot be based on the intrinsic properties of rules and expertise alone.

Rather, what is decisive for the conflict between bureaucracy and democracy is a

third factor: power. Bureaucracies provide technically superior means of control. This quality of bureaucratic organizations—for Weber as much as for Foucault—is the deepest and most intractable problem for substantive democracy. According to Weber's analysis, both economic and political processes cause bureaucratization in modern societies. In capitalist economies, bureaucracy spreads simply because profit is closely related to its technical superiority in production. Where government is concerned, bureaucratization is encouraged by demands for equal treatment and uniform services. The forms of administration that result produce a "passive democratization," combining a formal equality in the delivery of services with higher degrees of control by organizations over both their employees and clientele (Weber 1978a, 949, 985–86).

The superior technical capabilities of bureaucracy, however, not only provide superior means of power, but also result from preexisting relations of power. In such cases, the split between individual and institutional goals does not simply reflect imperatives of efficiency; it also reflects—indeed, presupposes—the power of some individuals to enlist others in the service of their goals (cf. Weber 1978a, 289–92). Weber is quite clear about the fact that bureaucratic domination is fueled in both capitalist and (state) socialist societies by the manner in which groups of individuals monopolize social resources (1978a, 1394). This is why they have the ability to use others as instruments in rationalized schemes of administration and production. In language that evokes Marx, Weber writes that

the hierarchical dependence of the wage worker, the administrative and technical employee, the assistant in the academic institute *as well as* that of the civil servant and the soldier is due to the fact that in their case the means indispensable for the enterprise and for making a living are in the hands of the entrepreneur or the political ruler. . . . The "separation" of the worker from the

material means of production, destruction, administration, academic research, and finance in general is the common basis of the modern state, in its political, cultural, and military sphere, and of the private capitalist economy. In both cases the disposition of these means is in the hands of that power whom the *bureaucratic apparatus* (of judges, officials, officers, supervisors, clerks, and noncommissioned officers) directly obeys or to whom it is available in case of need. This apparatus is nowadays equally typical of all those organizations; its existence and function are inseparably cause and effect of this concentration of the means of operation. (Weber 1978a, 1394; cf. Weber 1978b, 253)

Weber's comments here are important, for they suggest that he was quite aware of what critics such as Marcuse (1968, chap. 6) accuse him of overlooking, namely, that instrumental rationalization—whether in capitalist or state-socialist societies—is supported by power relations stemming from control over social resources. Bureaucratization is not, then, simply an effect of the technological requirements of a complex society, nor even less an inevitable social expression of "rationality." What Weber is claiming is that bureaucracies both presuppose power relations based on control over means of existence, and provide superior means of increasing control. His pessimism regarding the march of bureaucracy stems from the fact that bureaucratic organizations are superior means of power, and this makes it less likely that concentrated control can be successfully challenged. The key to criticizing Weber on this point, as Habermas has pointed out (1984, 220–22, 241–42), is to notice that Weber equated the rationalization of society with bureaucratization and did not entertain the possibility of any other *institutional* forms of rationalization. This made it difficult for him to locate rational alternatives. This is so in spite of the fact that Weber's conception of rationality in social life is very much broader than the instrumental rationality of bureaucracy.

Nonetheless, the importance of this

point is that it shows the *political* nature of Weber's problem; that the discontinuity between his political ethics and his pluralist-elite model of democracy reflects his assessment of political reality, and not just problems of principle. It suggests that if solutions exist to the problems of bureaucratization, they would have to be political in nature and address the distribution of control over social resources throughout the institutions of society. In terms of Weber's logic, only this kind of redistribution would remove the sustaining cause of bureaucratization. This would, of course, involve a substantial democratization of society, consistent with Weber's positive political ethic. This would be the clearest imaginable solution to his politicized "Nietzsche problem," bureaucratic nihilism. Yet any workable solution requires a political agent. Weber saw only a few courageous individuals, but no political movements likely to challenge bureaucratic domination. This is presumably why he opted for a position that could remain intact politically, if not as a liberal-democratic political philosophy.

## Conclusion

Weber does have a coherent political philosophy, then, one concerned with individual capacities to construct meaningful lives in an increasingly secularized and bureaucratized world. Yet he is inexplicit about the values of his political philosophy, in large part because he sees little or no possibility for their institutional expression. This leaves the underlying problems of his political philosophy unsolved, as we see in the conflict between his political ethics and what he took to be a possible politics—his pluralist-elite theory of democracy. This conflict in Weber between an ethically significant sense of politics and its institutional possibilities is compelling because it

is also our conflict: it reflects a continuing divergence between the promise and performance of liberal democracies. At the same time, framing Weber's problems in these terms allows us to identify solutions that remain obscure in Weber because he does not always distinguish between problems of principle and those of power in his analysis of bureaucracy. Identifying the power relations that sustain bureaucratic domination suggests that Weber's problem of a meaningful politics in a secularized world, could, at least in principle, be addressed through substantive forms of democracy now lacking in liberal-democratic societies.

### Notes

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1. I intend the term *pluralist-elite theory of democracy* to refer to the model explained and developed by C. B. Macpherson (1977, chap. 4). Macpherson refers to this model as "equilibrium democracy," because of its emphasis on marketlike equilibrium outcomes of elite competition.

2. Some aspects of Weber's Kantian legacy are, of course, better known than others. He subscribes to the Kantian distinction between moral choice and empirical existence, for example, when he insists that values are subjective commitments that cannot be scientifically (that is, empirically) validated. Likewise, Weber follows Kant in pointing to the non-scientific character of value judgments, while also—like Kant and against the positivists—holding that values have a rational meaning. Much of Weber's writing on the philosophy of social science was aimed at saving value choices from pseudoscientific claims, a central point of "The Meaning of 'Ethical Neutrality'" (1949, chap. 1).

3. Indeed, for Weber, the ethical significance of social science is its utility in clarifying conditions of rational agency. His choice of objects (social action) as well as his choice of methods (interpretive reconstruction of subjective orientations) both follow from his interest in personhood. His social science is appropriate to the study of humans insofar as they are conceived as agents, rather than, say, organisms manifesting behavior. Wolfgang Schluchter alludes to this point when he notes that for Weber "the conceptually presumed consistency of personality is a

kind of transcendental precondition of interpretive sociology" (1979, 73).

4. In Weber's schema, the plebiscitary leader is "the most important transitional type" between authoritarian charismatic domination and government where choice has become rationalized in an "elected officialdom" (Weber 1978a, 266–67). Thus, although Weber would not consider a plebiscitary democracy to be a mature democracy, the rationalization of this mode of legitimacy adds nothing to its consensual basis, and can be treated in the same way in regard to this issue.

5. Much like contemporary pluralists, Weber concluded that to counter the "direct and irrational mob rule typical of purely plebiscitary peoples," it is desirable that individuals be represented by means of secondary groups, such as clubs and unions as well as political parties (1978a, 1460).

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can then be used to pick out and discuss the concept with consistency" (Skinner 1978, 2:352).

The Anglo-American vocabulary of "political science" or the "science of politics" emerges as a staple of political discourse in the second half of the eighteenth century, roughly from 1740 to 1790. The principle characters behind these newly articulated efforts were Scotsmen: David Hume, Adam Smith, and Adam Ferguson. These thinkers—and others, too, including William Robertson, John Millar, Lord Kames, and Dugald Stewart (at a somewhat later period)—were subsequently judged so illuminating and so strikingly different in the context of Calvinist Scotland that they were credited with having ushered in a Scottish Enlightenment. The science of politics may be understood, then, as one product of this Enlightenment. And indeed, unlike the term *enlightenment*, the term *political science* was expressly used by the new scientists of politics to characterize their joint enterprise. Hume's own hopes—to "introduce the experimental method into moral subjects" (1978, subtitle) so that, among other things, "politics may be reduced to a science" (1748, 20)—were shared by his Scottish cohorts, however much they differed about details or about how these hopes might best be realized in practice. The sense of novelty and urgency was palpable to them all.

These enlightened Scots wrote a new chapter in what they took to be the history of the progress of the human understanding. The study of politics was, of course, an ancient and venerable enterprise of the human understanding, judging by the Anglo-American tradition alone. Calling theirs a "science" of politics, then, was of no small note. Neither Bacon, nor Hobbes, nor Locke, for example, sustained any effort to create what they were willing to repeatedly denominate as a "science of politics," though of course they used the constituent terms

often enough. Mentioning Locke in this context is of particular importance, for "Hume's and Smith's moral thinking was initially much preoccupied with remedying what they saw as the defects of Locke's thought" (Dunn 1983, 121). Less remedially, they carried forward and transformed one aspect of the political intent behind Locke's thought, namely the critique of enthusiasm—a subject to which I turn in the next section. Thus Locke marks an important point of departure from which to understand the conceptual changes involved in the formation of a science of politics.

The Scots shared a number of things with Locke: a recognition of the profound limits of the human understanding; an attempt to prescribe general rules to govern the understanding so that it might perform at its humble best; an acknowledgment that "science" named—and the optics and mechanics of Sir Isaac Newton best revealed—the highest exemplification of those general rules; a hope to advance a "moral science" that would hold implications for a right understanding of politics. But these shared commitments conceal as much as they reveal. In particular, they conceal conceptual changes that allowed the "science of politics" to be counted amongst the "moral sciences," properly so called.

Three changes are particularly salient in this stretch of intellectual history from Locke to the Scottish Enlightenment relating to (1) the strength of the cognitive claims for "science"; (2) the nature and range of "moral" phenomena; and (3) the idealization of Sir Isaac Newton as a methodological exemplar.

For Locke, *science* was an honorific term reserved almost exclusively to describe those achievements of the human understanding that laid cognitive claim to *certainty*. Certainty was identified with knowledge, strictly speaking, and was guaranteed either by intuition or demonstration. Intuition directly perceived the

truth in any relation of ideas, as for example in logical truths. Demonstration required intervening ideas, as for example in mathematical proofs. Any form of cognition that fell beneath certainty—which was to say, most forms—was characterized as a *probability*. Probability characterized a relation of ideas whose command over our considered belief was mitigated in varying degrees by such things as conflicting observations, the testimony of others, or hypotheses into matters beyond our senses (Farr 1987). In short, probability characterized the sort of understanding about which there could be any doubt whatsoever, and this meant any form of what we would now call *empirical inquiry*. Thus, even though probability consumes “the greatest part of our Concernment” here on “this our Pilgrimage,” a science it does not make (Locke 1975, 652). With the distinction between certainty and probability firmly in place, Locke could conclude that logic and mathematics were sciences, whereas mechanics and natural philosophy were not. And no amount of human labor or clever experiment could change the score: “How far soever humane Industry may advance useful and experimental Philosophy in physical Things, *scientific* will still be out of reach” (p. 556).

Human industry may never transform physics into a science, but in the matter of morality it expends itself on a preordained science. However odd this sounds to modern ears so attuned to a debilitating if honest relativism, Locke believed that “the *Idea* of a supreme Being . . . whose Workmanship we are, and on whom we depend; and the *Idea* of our selves . . . afford such Foundations of our Duty and Rules of Action, as might place *Morality amongst the Sciences capable of Demonstration*” (p. 549). Locke went on to say that “Indifferency and Attention”—the nonenthusiastic attitude of moderation and rationality required for this science—will make out “the measures of right and

wrong” to be “as incontestable as those in *Mathematicks*” (p. 549). Locke dramatically reveals here the theocentrism upon which his epistemology was founded. But even though God has underwritten a moral science, so very much remains to be done by his creatures to specify the content of its rules. Locke knew this painfully well, not only to judge by the epistemological difficulties he acknowledges in the *Essay*, but by his failure to draw up an actual list of demonstratively certain moral duties—despite the persistent prodings and requests from his closest friends, especially William Molyneux (Locke 1976–85, nos. 1838, 2030, 2059). On this score Locke left his readers only a few threadbare examples, like “No Government allows Absolute Liberty” and “Where there is no Property, there is no Injustice” (1975, 549). Had Locke ever chosen to characterize any aspect of his enterprise as a “science of politics,” this would have been the time to do so. As a matter of public record, however, this he never did.

Hume, Smith, and their Scottish cohorts in fact did characterize their labors in precisely these terms half a century and more later. This was an important conceptual innovation in itself. But behind it were conceptual changes in the meaning of *science*, and of *moral science* in particular. To measure the profundity of these changes, consider Hume’s criticism of one of Locke’s threadbare examples just cited: “To convince us of this proposition, that *where there is no property, there can be no injustice*, it is only necessary to define the terms, and explain injustice to be a violation of property. This proposition is, indeed, nothing but a more imperfect definition” (1975, 163). But Locke was guilty not only of a verbal sleight of hand; he had been trying to do the impossible, namely, to demonstrate morality: “The only proper objects of . . . demonstration,” Hume went on to say, were “the sciences of quantity and num-

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ber" (p. 163). "Morals" (in Locke's sense of *rules of conduct*) "were not so properly objects of the understanding as of taste and sentiment" (p. 165; cf. 1978, 456–70).

This did not mean that a "moral science" was impossible. Quite the contrary. But its very possibility depended upon abandoning a theocentric epistemology and expanding the "moral" domain capable of "science." Indeed the changes may be succinctly characterized as a move "from applied theology to social analysis" (Dunn 1983). In his own time, Adam Ferguson put the point more metaphorically in his *Principles of Moral and Political Science*: "In a period of many pretensions to science" it is necessary "to recall the attention of mankind from the heavens to the earth" (1792, 1:4). When Hume advertised the *Treatise of Human Nature* by subtitling it as an "attempt to introduce the experimental method into moral subjects," those moral subjects were expanded to include virtually all earthly things social or human, including, in principle, deviations from the rules of right conduct, especially rational Christian conduct. Implicit in this expansion was an increasing sensitivity to the relativity of cultural and moral forms and a certain moderate—or "mitigated"—skepticism that invariably attends such sensitivity. Hume not only said this but along with Smith, Ferguson, Robertson, and Millar showed it in practice by moving beyond theocentrism and its "moral" limitations. They produced comparative studies of forms of government, histories of the progress of civil society, discourses on natural religion, and inquiries into political economy, especially those detailing the nature and causes of the wealth of nations.

These various enterprises in social analysis were factual and causal inquiries that no one scrupled to disallow as "science." Thus the demonstrative sciences like logic and mathematics were now joined by other sciences, including "moral

reasonings . . . either concerning particular or general facts." History and astronomy were examples of sciences of particular facts. "The sciences which treat of general facts are politics, natural philosophy, physick, chemistry, etc. where the qualities, causes and effects of a whole species of objects are enquired into" (Hume 1975, 165).

Calling politics a *science*, and ranking it alongside physics, depended upon two further developments. The first was an increasingly evident tendency to liberalize what could in principle pass muster as "science." Hume, along with Smith and others of his generation, promoted into the ranks of science those forms of factual inquiry that laid cognitive claim to *probability*—especially probability in its strong form as *proof*, something about which Hume (wrongly) thought Locke oblivious (1975, 56n). Hume did, of course, place great strictures upon how much confidence we might place in our attributions of probability, whether in matters of evidence or of causality. And he unleashed his most astringent comments on those who believed they could go beyond these strictures, especially those who believed in miracles. For Hume, the only miracle was that anyone could believe in one (p. 131). All of this was a consequence of his mitigated skepticism and his recognition of the profound limits of the human understanding. But probabilistic inquiries were still in principle sciences. Though liberal, this did not signal an attitude of "anything goes" when it came to demarcating a science. In his more inquisitorial moods Hume was willing to "commit . . . to the flames" such cryptocognitive practices as "metaphysics" since it was "not properly a science" (p. 165). Smith was only slightly less ungenerous, facetiously calling metaphysics a "cobweb science" (1976, 2:293).

In practice, of course, the science of politics had considerable distance to travel to justly deserve its place alongside

physics.<sup>2</sup> Hume's own particular program—inspired in part by Francis Hutcheson (1755) and joined by Smith, Ferguson, and others—to ensure the methodological success of the science of politics depended upon a certain architecture of social theory. "Politics," he averred, "consider men as united in society, and dependent on each other" (1978, xv). Mutual dependence and social unity required sympathy and other moral sentiments about which theories must be developed. Underneath this superstructure Hume laid his foundation in a theory of the human understanding whose hallmark was the association of ideas. This foundational theory had its visionary potential in Hume's eyes: "By this means, we may perhaps, attain a new microscope or species of optics . . . in the moral sciences" (1975, 62).

The very mention of microscopes and optics in the moral sciences intimates the other notable development that made possible Hume's juxtaposition of politics and physics: namely, the incredible power of the image of a moral science modeled on Sir Isaac Newton's physics. Following Newton's lead, Hume envisioned (at least in his more prophetic and positivistic moods) an experimental and law-governed methodology for science. It was to be one in which, as Ferguson put it, "the *Regulae Philosophicae* [of Newton's *Principia*] . . . must be sustained" (1792, 2:118). The Scots had even greater hopes to mobilize Newton's mechanical principles in their own substantive theories. "ATTRACTION," Hume shouted in bold letters, characterized a principle of the association of ideas and of sympathy (1978, 12–13). Action and reaction was "a maxim of the science of mechanics" good for the moral and political sciences, Ferguson noted (1792, 1:11). The imaginative fixation on Newton was so compelling that Hume hoped to be named the Newton of the moral sciences. John Millar later complicated that particular hope,

but underscored its power. He paid that tribute to the author of the *Wealth of Nations* (quoted in Smith 1980, 275n).

Here, too, is the third major change that informs our narrative. "The incomparable Mr. Newton" was of course well known to Locke. Among other things, Locke modestly hoped to be his "Underlabourer" (1975, 10). But Locke marveled at the complexity of Newton, a complexity all but lost on Hume, Smith, and countless generations since. Locke was privy to Newton's enormous range of interests not only in natural philosophy (which Locke understood only in part) but in religious hypotheses, biblical prophecy, numerology, and other "mystical fancies," which Newton shared with him in correspondence (Locke 1976–85, no. 1357). Among Locke's contemporaries, the latitudinarian clergy promoted their gospel of tolerant Christian rationalism partly by appealing to Newton's own vision of the harmony of "the world natural and the world politic" (quoted in Jacobs 1976, preface). In short, the imaginative possibilities that Newton symbolized for numerous intellectuals and clergymen at the turn of the eighteenth century had changed dramatically by the time of the Scottish Enlightenment. Newton had become a uniquely methodological and scientific figure, though one of imperial proportions (Smith 1980, 104). The moral sciences were potential colonies, and could only take their place in the sun by following the lead of Newtonian mechanics. It was this view that Dugald Stewart was to call "the peculiar glory of the latter half of the eighteenth century" (1854, 1:70).

In sum, by the time the Scots furthered the "moral sciences" and expressly individuated a "science of politics" out of them, the conceptual materials that Locke structured had been radically transformed. No longer need the moral sciences bear the burden Locke placed upon them. No longer, that is, need they pro-

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vide a demonstratively certain understanding of the prescriptive moral duties binding the consciences and conduct of rational Christians. Rather, the moral sciences could move forward as probabilistic forms of empirical and causal inquiry into social or human matters that took methodological inspiration from the natural sciences, especially Newtonian mechanics. Here at the veritable coining of *political science* we recognize a glimmer of the modern conception of the nature and limits of what have come to be called the social, human, or behavioral sciences, even though an endless series of debates were to govern the constitution, specification, and transformation of these sciences, even to the present time.

### An Antidote to Enthusiasm

Setting methodological standards did not constitute an insular academic exercise for the Scottish founders of the science of politics. Indeed, in and through setting methodological standards, the Scots were at the same time thinking and acting politically. Their science was to be an instrument of political education and criticism in the interests of moderation. "Nothing," Smith declared, "tends so much to promote public spirit as the study of politics" (1969, 307). Hume agreed: "Those who employ their pens on political subjects, free from Party-Rage and Party-Prejudices, cultivate a science, which, of all others, contributes most . . . to public utility" (1748, 127). The accent on *rage* and *prejudice* put some teeth into these otherwise innocuous-sounding gestures of public-mindedness. Such excessively immoderate expressions as *rage* and *prejudice* were symptoms of political passions and attitudes that in an era of faction, disorder, and rebellion were best likened, as Smith likened them, to "poisons."

This heightened and even hyperbolic

language suggests the stakes for the Scots, as well as their fervor for science. These days we might be tempted to call it an enthusiasm for political science. But this is precisely what our eighteenth century Scottish Enlighteners could not have called it. In their terms, "enthusiasm"—as well as "superstition"—posed two of the greatest threats to the mind and the body politic. The Scots found enthusiasm and superstition to be such virulent strains of poison largely because they diagnosed them so broadly as to encompass all manner of immoderation in religion and politics—especially but not invariably amongst the lower orders. Enthusiastic and superstitious claims about the human understanding—about what could be known or justifiably believed, especially in matters informing action and conduct—lay at the foundation of this immoderation. This naturally drew the attention and the ire of the Scottish scientists of politics. The full story of the political formation of the science of politics requires equal attention to both of these poisons—as well as to yet other political intentions and projects. But this cannot be done here, and it is certainly the case that superstition is a relatively well worked theme in studies of Hume, Smith, and the Enlightenment generally (Gay 1966). Perhaps, then, we might mention it only briefly here, so as to press on to its understudied twin.

Superstition—on the telling of its countless enemies, at least—manifested itself in any number of irrationalities harbored by the understanding. But it especially emerged in "fear of harm and disorder from invisible powers" (Robertson 1777, 1:305), which, as Hume noted, became institutionalized in "Ceremonies, Observances, Mortifications, Sacrifices, Presents, or in any Practice, however absurd or frivolous, which either Folly or Knavery recommends to a blind and terrified Credulity" (1748, 106). Although ancient pagans, Indians, Highlanders,



high Anglicans, and Jews were not exempt from its charms, "the grossest delusions of superstition" (Smith 1976, 2:325) particularly infected Roman Catholicism. "Priestcraft" and "priestly power" exercised an authoritarian hold over the minds, if not the souls, of benighted papists. The critique of superstition in this its Romish guise had long figured in the polemical battles of the English and Scottish Reformations. The Scottish Enlightenmenters carried on in this task. In the *Principles of Moral and Political Science*, Ferguson epistemologically indicted the superstition of priests and popes for having "set reason aside" and so become "an engine to work up the minds of other men to their purpose" (1792, 1:147). In this he agreed with Hume who had already drawn a political conclusion about this engine. Superstition was "an Enemy to Civil Liberty," for it "renders Men tame, and abject, and fits them for Slavery" (1748, 108, 111).

Enthusiasm threatened the minds of men in different but equally poisonous ways. The Scots' diagnosis of it was not utterly unique in the Anglo-American tradition. To appreciate both continuity and change we may briefly trace its history, a history told largely by its critics. The idea of enthusiasm entered the English language in the late sixteenth century, bringing with it the Greek notion of divine inspiration, of "God possessing Man." Very shortly in Reformation Britain, however, it took on the decidedly more suspicious connotation of *delusions* of inspiration or of *false* claims to having certain and immediate knowledge of God. In this guise, it generally became a convenient epithet of abuse employed by countless writers who sought to discredit the zealotry of the fringe sects thrown up by the Reformation and the English Revolution of the 1640s. Quakers, Baptists, Antinomians, Diggers, Ranters, Familists, Fifth Monarchists, and other sectaries were excoriated for their beliefs

and for their appeals to (an alleged) divine inspiration as the source of these beliefs. Hobbes, predictably, defined as "madmen Prophets" those who "pretended to Prophecy by possession of a Spirit." He heard in "such Enthusiasme" another loud note amidst "the Seditious roaring of a troubled Nation" (1968, 141, 143). Treatises poured out, under titles that invoked the specter of *Enthusiasticus Triumphatus* or that, conversely, announced with hope of relief *The Spirit of Enthusiasm Exorcised*. By the late seventeenth century, the Anglican clergy could weigh up all the enthusiastic sects as "vast Loads of Filth" (quoted in Jacobs 1976, 193).

Locke, too, lodged warnings and complaints against "the Extravagancy of Enthusiasm" (1975, 667), although he was neither Hobbist nor friend to the Anglican clergy (Goldie 1983). Indeed, the topic was of such importance to him that he added a wholly new chapter, "Of Enthusiasm," to the fourth edition of the *Essay Concerning Human Understanding*. His analysis of it was tied to his theocentrism, as well as to questions of "method"—a point he makes clear in the juxtaposition of entries in his commonplace books (see Farr 1987, appendix). Locke proceeds to explain "the rise" of enthusiasm in the individual mind, to undermine the force of its claims, and to reaffirm the role of reason in the human and Christian understanding.

"Men, in whom Melancholy has mixed with devotion," Locke explains, readily tire of "the tedious and not always successful Labour of strict Reasoning." They are thus "very apt to pretend to Revelation": "Their Minds being thus prepared, whatever groundless Opinion comes to settle it self strongly upon their Fancies, is an Illumination from the Spirit of GOD, and presently of divine Authority: And whatsoever odd Action they find in themselves a Strong Inclination to do, that impulse is concluded to be a call or direction from Heaven, and must be obeyed;

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'tis a Commission from above, and they cannot err in executing it" (1975, 699). Using his own very strict language of science, Locke castigates this as an unwarranted claim to "Certainty without proof, and without examination" (p. 700). It confuses genuine understanding with seeing and feeling, especially because of the oft-abused simile of "a light within." It also substitutes the immediacy and strength of belief for rational argument or scriptural evidence. Locke's training as a physician allowed him to find not revelation in all this enthusiastic exultation of "certainty in themselves, and demonstration to others" but only the "Conceits of a warmed or over-weening Brain," which fuel the "untractable Zealots in different and opposite Parties" (pp. 699-700, 703). After having identified enthusiasm in this way, Locke begged off writing "an historical account of the various ravings [that] men have embraced for religion" because it was "besides my purpose, and enough to make a huge volume" (1976-85, no. 1887).

Doubtless, Locke felt with considerable discomfort the challenge that enthusiasm posed; for whatever its extravagance, enthusiasm still held out something of Locke's own hope that moral knowledge could be shown to be certain knowledge. Moreover, Locke could not and never tried to disallow revelation and miracles as grounds for moral belief: "I am far from denying, that GOD can, or doth sometimes enlighten Mens Minds in the apprehending certain Truths, or excite them to Good Actions by the immediate influence and assistance of the Holy Spirit" (1975, 705). Locke settled, as he had to, for hedging round revelation and miracles with reason and Scripture read aright. And he had to settle for arguing negatively against enthusiasm because, as mentioned above, he failed in the constructive task of demonstrating morality and so showing it to be, on his own terms, "scientific." For these reasons, perhaps,

Locke never clearly tells his readers what to *do* about enthusiasm (though he was ready to put priests to a "test" and atheists to much worse). On pain of intolerance, it would appear that Locke himself in the *Essay* sets the example for the only acceptable action against enthusiasm: arguing rationally against it.

The Scottish scientists of politics had a slightly better formed idea of what must be done to combat enthusiasm publicly. But this was itself a consequence of changes in their understanding of science and in their diagnosis of the *social mechanisms* by which enthusiasm was propagated. Here the Scots followed the lead of Anthony Ashley Cooper, the third earl of Shaftesbury, a boyhood pupil of Locke's and the grandson of Locke's radical Whig patron and employer. (Locke's own radicalism, only now being fully documented [Ashcraft 1986], might be credited in good measure to the *first* earl's influence.)

In *A Letter Concerning Enthusiasm*, occasioned by the arrival in London of the so-called French Prophets, Shaftesbury espied the same "Specious Pretext of moral Certainty" (1727, 1:44) which, alongside an alleged divine inspiration, defined enthusiasm. Yet Shaftesbury pressed on in two new directions. One was to allow for an occasional "noble enthusiasm" whose appeals were different from but consistent with the demands of reason (1:54). The other was to emphasize the spread of (the more common, vulgar forms of) enthusiasm, using the socio-medical vocabulary of *effluvia*, *transpiration*, *contagion*, *infection*, *fury*, and *panic* (see Schwartz 1978, 41-56). The general causal mechanism of "sympathy" was at work here, a mechanism later to become a hallmark of Scottish philosophy after Hutcheson (1755). In the hands of Hume (1978, bk. 2) and Smith (1969), sympathy became the principal form of communication that made society possible. But on occasion it endangered it, too. Shaftesbury explained that "one may with good

reason call every Passion Panick which is rais'd in a Multitude, and convey'd by . . . Contact or Sympathy. . . . And thus is Religion also Panick, when Enthusiasm of any Kind gets up" (1727, 1:15-16). The magistrate must combat this enthusiastic panic not with violence but with "cheerful means" and a more impartial sympathy: "The only way to save Mens Sense, or preserve Wit at all in the World, is to give Liberty to Wit" (1:17, 19). And so Shaftesbury concludes his *Letter* with words that in all likelihood inspired Adam Smith's line a half century later: "By this means we prepare ourselves with some Antidote against Enthusiasm. And this is what I have dar'd affirm is best perform'd by keeping to GOOD HUMOUR. For otherwise the Remedy itself may turn to the Disease" (Shaftesbury 1727, 1:54-55).

Shaftesbury's cure may have seemed a trifle lighthearted, given the disease. But in rough outline the Scots shared his general diagnosis and developed it in accord with their own post-Lockean views of philosophy and science. Enthusiasm was identified by its unscientific extravagance: its boasts of a certainty that "shuts up the mind against the conviction of error" (Ferguson 1792, 1:145); its failure to respect facts, natural causes, and the bounds of probability in matters moral; its unwillingness to correct its particular judgments in accordance with general rules (Hume 1792, 631); and its conviction of divine inspiration—"the Summit of Enthusiasm" (Hume 1748, 106). Furthermore, as Ferguson noted in his *Political Science*, enthusiasm spread like the "contagion" of a social disease (1792, 1:143), especially amongst the uneducated and witless general public. Like cowardice, which, too, affected the health of mind and body politic, enthusiasm scarcely seemed different from "leprosy or any other loathsome and offensive disease" (Smith 1796, 2:308).

The Scots went beyond Shaftesbury's diagnosis to discover and describe all

manner of enthusiasm in history. The contrast with the general health and progress of modern commercial societies was thereby made all the more dramatic. Ferguson found it amongst ancient Greek republicans and Indians of the sub-continent (1767, 127; 1792, 2:141); Robertson found it amongst early Highlander nationalists and Indians of America (1777, 1:390; 1844, 11); Smith found it amongst the Independents during the English Revolution and "the dissenters and the methodists" of his own day (1796, 2:310). Hume found it virtually everywhere, amongst "the Anabaptists in Germany, the Camisars in France, the Levelers and other Fanatics in England, and the Covenanters in Scotland" (1748, 110; also see his *History of England* [1754], especially the infamous volume on the Stuarts and the Commonwealth). No noble enthusiasm inspired these various sectaries. However, in Shaftesbury's spirit, Hume did allow that in political matters enthusiasm was "naturally accompanied by a Spirit of Liberty" (1748, 111)—a point that influenced Gibbon in his otherwise critical account of ancient enthusiasm (Pocock 1985, chap. 8). Against the authoritarian and enslaving doctrines of superstition, this was to enthusiasm's credit. However, liberty was fraught with perils when too enthused. Thus from the history of "dangerous enthusiasts," Hume confirmed the recurring voices for "leveling" and "a community of goods" (1792, 186, 193); Smith, "the interested complaints of faction and sedition" (1796, 2:309); and Ferguson, the contagious communications of "revolutionists in government" (1792, 1:135).

For fear of political excesses such as these, government had to actively oppose enthusiasm. Science was a partisan in this opposition. Scientists and philosophers were themselves trustworthy because "there is no enthusiasm among philosophers" (Hume 1792, 147). "Nothing but philosophy," Hume went on in his essay,

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"Of Enthusiasm and Superstition," is "able to conquer entirely these unaccountable Terrors" (1748, 108). The engagement implied by these words suggest that philosophy and science represented not only enthusiasm's *opposite* but—in Smith's striking phrase—its *antidote*. The matter may be put somewhat differently by developing Ferguson's observation that "the affairs of society require the light of science" (1792, 1:269). Not only was science to shed light on the affairs of society, as it were; it was—to lift a line from the enthusiasts themselves—to shine like a light *within* society.

Adam Smith was the most forthcoming in presenting a basic strategy for the state's campaign against enthusiasm, though concurring sentiments can be found amongst his countrymen. As was his wont, our "professor of moral philosophy" did not construct a *heads of proposals*, much less a *manifesto*. But he made his views sufficiently clear in the *Wealth of Nations*, especially in the articles on education and religious instruction in book 4, part 3, "Of the Expence of public Works and public Institutions." State violence was to play no part. Neither would one state church nor would many legal edicts. Rather, the first plank of his program was religious toleration. Here he followed Locke and Shaftesbury and a whole host of eighteenth century tolerationists. But Smith's reasons included something of a hidden-hand argument above and beyond tolerationist considerations respecting doctrinal differences or individual moral development: "Where the society is divided into two or three hundred, or perhaps as many thousand small sects . . . no one could be considerable enough to disturb the public tranquility. The teachers of each sect, seeing themselves surrounded on all sides with more adversaries than friends, would be obliged to learn . . . candour and moderation" (1776, 2:314).

For obvious reasons, however, a policy

of toleration was in itself insufficient to prevent the public disorders attendant to enthusiasm. More direct action by way of education—that enlightenment gamble—was required by the state. First came direct general education of the young, especially those of the laboring poor. Their virtues and understandings were constantly threatened by the division of labor and other unavoidable corruptions of a commercial society. The stability of government was palpably at stake.

The State derives no inconsiderable advantage from their instruction. The more they are instructed, the less liable they are to the delusions of enthusiasm and superstition. . . . An instructed and intelligent people besides, are always more decent and orderly than an ignorant and stupid one. . . . They are more disposed to examine, and more capable of seeing through, the interested complaints of faction and sedition, and they are, upon that account, less apt to be misled into any wanton or unnecessary opposition to the measures of government. (2:309)

Beyond general education, Smith counseled further "Institutions for the Instruction of People of all Ages," the chief of which was to be for "religious instruction" in the broadest sense imaginable (2:309). After lengthy analysis and a harangue against "stupid and ignorant enthusiasts," Smith advises the "wise legislator" in the two principal remedies that were noted at the outset of this essay. One, we may recall, provided for "public diversions" in the form of "dramatic representations and exhibitions" in order to dissipate gloom and melancholy. This rendered in a more publicly spectatorial way Shaftesbury's design for "gaiety and good humour . . . and public ridicule," as Smith himself put it (2:318). The circuses of the ancient world had their place in modern commercial society, after all.

The other remedy provided for "the study of science and philosophy, which the state might render almost universal among all people of middling, or more than middling rank and fortune." Here the state could take the low road of insist-



ing only upon certain standards that a candidate for "any liberal profession . . . or any honourable office" might meet. This would force them, marketwise, to find better teachers than the state could provide directly. But the end gave purpose to these means; for, however achieved, a scientific education was necessary for the health and order of the people: "Science is the great antidote to the poison of enthusiasm and superstition, and where all the superiour rank of people were secured from it, the inferior ranks could not be much exposed to it" (2:318).

Protected from the poisons of enthusiasm and superstition, both mind and body politic may then attempt to govern themselves as an "impartial spectator" might. Impartiality of the spectatorial sort required "good temper and moderation" (2:297, 315). Thus, in advancing the cause of a science of politics, the figures of the Scottish Enlightenment *at the same time* furthered the cause of a politics of moderation and its attendant virtues—impartiality, candor, and coolness of temper. This was not, in their eyes, simply a matter of choice. Rather, it was *required* by the structure of their political argument, which conceptually connected philosophy, science, moderation, and the absence of enthusiasm. Thus, when Hume opened his famous essay, "That Politics May Be Reduced to a Science," he presented himself as he felt he had to—as a "friend of *Moderation*" (1748, 20).

Moderation secured for its Scottish friends a political position with considerable rhetorical opportunities, to say the least. It characterized a calm and detached mode of political argument more than it picked out a particular agenda. Among moderation's rhetorical virtues, it allowed the Scots to be—or at least to claim to be—both detached and committed, both scientific and political. It also earned them the subsequent charge of conservatism. Given the animus that Hume showed towards Levelers or that Smith showed

towards Dissenters, the charge is not utterly undeserved. A more generous reading is possible: "In its full extent and in its historical circumstances, Hume's science of politics was constructive, forward-looking, a programme of modernization, an education for backward-looking men" (Forbes 1977, 39). In any case, if *conservative* aptly describes the "Lesson of Moderation" that Hume and Smith and Ferguson drew time and time again, then surely it is "the result of an analysis derived from a philosophical position rather than a mere attitude of conservatism" (Winch 1978, 171). The lesson of moderation *had* to be drawn from the very nature of the science of politics.

### Ideology and the History of Political Science

Curiosity naturally presses a narrative beyond the historical limits it sets itself. One wants to know what, if anything, came of the past? What, in this case, came of the Scottish Enlighteners' expressly articulated intention to create a "science of politics" that would advance both a natural scientific methodology whose inspiration was Newtonian and a politics of moderation whose tasks included the critique of enthusiasm? This is a difficult question to answer, in part because of the sheer mass of historical materials needed to satisfy curiosity and in part because of the evident fact that so very much has changed. The Scottish Enlightenment does not immediately spring to mind when contemporary political scientists set themselves to solve theoretical, methodological, or political problems of current concern. Or, to use Marx's haunting metaphor, the Scottish Enlightenment does not, in political scientists' waking—or sleeping—hours, "weigh like a nightmare on the brain of the living" (1963, 15) (though I venture to predict some night-

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mares now that political scientists are bringing political economy and the state back in). Let me conclude this essay with some further historical and theoretical reflections that might begin to alleviate the curiosity behind the above question and to do so fully cognizant of change. I hope, thereby, to intimate the ongoing relevance of the history of political science for our disciplinary self-conceptions.

The call for a science of politics newly modeled on the natural sciences in Newtonian and probabilistic form continues to be heard well after Dugald Stewart praised this "peculiar glory" of Hume, Smith, and Ferguson. One hears it in Comte, Mill, Spencer, Bryce, Merriam, Lasswell, Easton, and not a few behavioral revolutionaries. So time honored has this call become that, as far as the history of political science is concerned, one need only highlight the faint irony that attends it. One of the oldest things about political science is its methodological promise to be new—and Newtonian! Though this view has met more than one critic, it has generally set the terms of debates over the *methodological* identity of political science. And so the echoes of the Scottish Enlightenment can be heard in our own debates today.

The science of politics continues to be pressed into political debates, as well. For a time, these political debates continued to be rhetorically heightened by the charges of enthusiasm. These charges wane soon enough, however, as the meaning of *enthusiasm* changes, and one ideology or another—indeed, *ideology* as such—comes to replace it. But the *political* identity of a science of politics committed to moderation lingers on well after "enthusiasm" receives the rather unintended antidote of conceptual change.

Consider one episode of interest and importance to students of U.S. politics: the debates in 1787-88 over the yet-unratified Constitution. The mock Romans on both sides—Cato, Brutus,

Publius—claim to speak on the authority of the "science of politics," or the related sciences of government, legislation, and statesmanship (for anti-Federalists, see Storing 1981, 2.7.3 on the juxtaposition of "the science of government" with "that frenzy of enthusiasm"; for Federalists, see Wood 1969, chaps. 1 and 15). In the case of Publius (Hamilton, Madison, and Jay 1961), the Scottish connection is strikingly evident (Adair 1957; Wills 1981).

In defending the Constitution's plan for an extended compound federal republic, Publius opens the *Federalist* with a "lesson of moderation"; and closes it by citing Hume's essay, "Of the Rise and Progress of Arts and Sciences." "These reflections contain a lesson of moderation to all the sincere lovers of the Union" (Hamilton, Madison, and Jay 1961, 34, 514). In the course of the 85 papers—most evidently in numbers 9, 31, 37, and 47—the "science of politics" takes a stand as a methodological and political ally of the Federalist rhetoric of moderation and union. Eschewing pretences to "the same degree of certainty with those of the mathematics," the science of politics nonetheless has "better claims" than ordinary opinion (p. 194). Thus it counsels "caution and investigation" as "necessary armor against error and imposition" (p. 194). Without this armor a nation comes to suffer "moral diseases," "poisons," and "the infection of violent passions" (pp. 99, 147, 385). Such maladies attend a zealous and factious politics of the sort Publius fears is everywhere evident in the state governments under the Articles of Confederation.

But the science of politics understands the republican mechanisms necessary to alleviate these ills. Consider the hidden hand of national size at work against a familiar source of trouble: "A religious sect may degenerate into a political faction in a part of the Confederacy; but the variety of sects dispersed over the entire

face of it [the nation] must secure the national councils against any danger from that source" (p. 84). (The comparisons with Smith are too striking to miss; see Smith 1976, 2:314.) So it is that the science of politics plays its rather more visible hand for "the necessity of Union," since union alone is "the proper antidote for the diseases of faction" (Hamilton, Madison, and Jay 1961, 99).

Given the Scottish intellectual sources and, even more, given the history of religious enthusiasm in the colonies (Lovejoy 1985), it is not surprising that *enthusiasm* makes its appearance in these debates. In the very first paper—and with strong Shaftesburian and Humean overtones—Hamilton warns his readers against those zealots who in the name of liberty cry out against union. For such "noble enthusiasm of liberty is apt to be infected with a spirit of narrow and illiberal distrust" (Hamilton, Madison, and Jay 1961, 35). Later, in cataloging the problems created by the bevy of state governments under the Articles, Madison reminds his postrevolutionary readers that such governments were formed in the midst of "an enthusiastic confidence of the people in their patriotic leaders, which stifled the ordinary diversity of opinion in great national questions" (p. 315).<sup>3</sup> This echoed Madison's prerevolutionary sentiments, in which he contrasted "enthusiasm" with "sober public opinion" (quoted in Lovejoy 1985, 220). But in great national questions there should be sobriety and diversity of opinion. And surely, one "ought not to assume an infallibility" (Hamilton, Madison, and Jay 1961, 226), for none is to be had, not even from God: "When the Almighty himself condescends to address mankind in their own language, his meaning, luminous as it must be, is rendered dim and doubtful by the cloudy medium through which it is communicated" (p. 229). Science, in short, represents the best citizens can hope for by way of understanding politics and con-

stituting a new polity (Ball 1983). It serves the interests not of the enthusiastic but only of the "enlightened friends of liberty" (Hamilton, Madison, and Jay 1961, 72).

Social, political, and conceptual processes quickened the pace of change inaugurated by the great revolutions in America and France. Reflecting increasing patterns of secularization and recording an attempt to resolve the contradiction between moderation and the practical requirements of boldly meeting novel problems, *enthusiasm* started to lose much of its sectarian identification and its connotation of divine inspiration. It even came to have its champions. The twice revolutionary Tom Paine, who had spoken for *Common Sense* in the American Revolution, reminded the French Convention in July 1795 that it was not "cold-indifference" but "the enthusiasm and energy which have hitherto been the life and soul of the Revolution" (quoted in Tucker 1972, 100).

The French Revolution continued to divide opinion well after the blades of the law quit falling. "Enthusiasm" and the "science of politics" were parties to these divisions. Macaulay's encomium to "that noble Science of Politics" was contrasted to "the barren theories of utilitarian sophists," whose political commitments, he thought, branded them as a "Republican sect . . . enthusiastically attached to its ends . . . as the French Jacobins themselves" (quoted in Collini, Winch, and Burrow 1983, 101). The Scottish connection was again in evidence, though by now Hume's legacy seemed to bifurcate: historical inductivism for Macaulay's "noble science," utilitarian deductivism for James Mill's "science of government." John Stuart Mill later reflected on this methodological and political episode with some pain, trying in his own way to walk a *via media* between Macaulay and his father, whom he immortalized as "the last survivor of this great school" of Scottish

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Enlightenment philosophers (1963, 566). The younger Mill helped advance the cause of the "moral sciences," especially in book 6 of his *Logic*. In addition to others, Comte had been instrumental in forming Mill's scientific views. Without hint of disgrace,<sup>4</sup> Mill remembered Comte as having "rekindled all my enthusiasm" (1924, 147), even though Mill had by then disassociated himself from the religious extravagance (not to say *enthusiasm*) of Comte's positivism.

Mill's ability to use *enthusiasm* in this way, and without opposition to *moral science*, marks considerable conceptual change. In spite of occasional archaic uses and reminders, *enthusiasm* was by then assuming its contemporary meaning. The term now denotes intense interest, excitement, or eagerness in any sort of pursuit. It has certainly lost its sectarian reference, pejorative connotation, and opposition to science. Little trace of those seventeenth and eighteenth century debates can be hard these days, what with references in scientific contexts to "the enthusiasm for developing the orthodox position" (Kitcher 1982, 119). With some historical irony, we even hear from two of his excellent editors and commentators that "[Adam] Smith wrote of the Newtonian system with real enthusiasm" (Skinner and Raphael quoted in Smith 1980, 32).

Ideology replaces enthusiasm, literally and functionally. That is, the concept of *ideology* is the direct descendant of *enthusiasm*—as well as of the other ancestral extravagances found in "superstition" and "priestcraft" (see Goldie 1988). *Ideology* takes the place of *enthusiasm* as the opposite of science and the standing danger to reasonableness and moderation in mind and body politic. The history of *ideology* is not without its own fairly dramatic conceptual changes, but the moral remains much the same (Goldie 1988, Lichtheim 1967). *Ideology* was coined by Destutt de Tracy in the post-revolutionary France of 1796. By it, he

meant simply the "science of ideas," in Locke's sense of *ideas* and Newton's sense of *science*. This science conceived of its political task as one of enlightenment, rather as did the science of politics to the Scots. It was to liberate human ideas from illusion, predicated on the diagnosis of human ignorance, dogma, and prejudice. But by the late 1840s—thanks practically to Napoleon's expressed disdain for *les ideologues* and theoretically to Marx's no less disdainful criticism of Feuerbach and the Young Hegelians—*ideology* changed its meaning from a science against illusion to illusion itself. Religion—and for Marx all religion suffers from enthusiasm, superstition, and worse—proves to be the veritable exemplar of those illusions. Marx's "method of approach," so he argues, sharply contrasts "religion, metaphysics, [and] all the rest of ideology" with "real, positive science" (Marx and Engels 1970, 47–48).

On this score, at least, twentieth century political scientists have largely agreed with Marx—even if, to many, Marxism itself has become virtually synonymous with ideology. While there are surely some neutral uses, the sense of *ideology* that has been of greatest interest and dispute has referred generally to any systematic set of ideas that is methodologically impervious to rational and empirical inquiry and is politically extremist in the face of moderate alternatives. Ideology, in short, is the antinomy of science both in matters of method and politics. And science not only identifies itself as the opposite of ideology, it promises an end of ideology. Thereby, the scientific antidote to enthusiasm retains its potency even when the poison becomes ideology.

Saying so doesn't make it so, of course; and political science can be, and has been, criticized for failing to live up to its political promises, whether in the matter of moderation, democracy, or progressive reform (Euben 1970; Ricci 1984; Seidel-



man and Harpham 1985). It has even been criticized for having capitulated to an ideology of the end-of-ideology (MacIntyre 1971, chap. 1). But this only underscores the more fundamental point. The identity of political science is as much a political as a methodological one; and these days this is rhetorically bound up with debates implicating ideology, as earlier it was in debates implicating enthusiasm and superstition. One does not, therefore, have to strain to make the general case that there are ideological implications of political science. The history of political science quite literally records them.

Many of the debates over the ideological implications of political science *in fact* have been occasioned by published histories of political science. This suggests just how relevant such histories can be to the identity of political science, and indeed even to political discourse in the public arena. Episodes from the history of U.S. political science as recounted in the narratives of Beard (1927), Merriam (1931), Easton (1953), Crick (1959), Storing and associates (1962), Ricci (1984), and Seidelman and Harpham (1985) make this abundantly—and sometimes heatedly—obvious. The history of political science, in short, provides a forum for remembrance, reflection, and critique that contributes as directly as one could imagine to our disciplinary identity, our educational practices, and occasionally our public posture. (For history, political science, and education, see Jacobson 1963 and Sabia 1984.) We try to understand ourselves in light of our history, and often in the process we change our conceptions of our history and of our current condition and future prospects.

Debates over Locke and Smith—to choose from the larger cast with which we began—may appear to be different from, say, debates over Beard, Bentley, Merriam, or Lasswell by their temporal remove, if nothing else. But these thinkers

are not intellectual antiques. They even enliven public debate in the United States at present. Apart from numberless political scientists, a chief justice of the Supreme Court, an attorney general, several senators, the head of the Office of Management and Budget, and two Nobel economists have appropriated or castigated the doctrines of Locke and Smith in public in recent times. Debates over property rights, international tariffs, legal reform, and even postal delivery have alluded to Locke and Smith—or, at least, to what some say Locke and Smith said and how it is or is not applicable to our current problems. In a politically charged environment like this one, very much depends upon the rectitude of the interpretation of the meaning of these doctrines. *And so back we go, the better to understand ourselves now and in the future.* I have already given more than a few hints how I think we should understand Locke and Smith, above and beyond their contribution to the emergence of political science and the establishment of a rhetorical framework for subsequent debates about its state and discipline. Locke can be better understood as a theocentric radical than as a liberal. Smith can be better understood as a skeptical statist than as a free-marketeer. These interpretations are admittedly less comforting and self-congratulating than are the usual ones we hear these days. They may or may not be able to bear the ideological load some have placed upon them. But they are no less relevant for that.

## Notes

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environment. Subsequent versions of the essay benefitted from conversations with Léon Epstein, Peter Euben, Terence Ball, and especially Mary G. Dietz. Thanks are due to them all; of course, they are not responsible for the errors that remain.

1. In an essay of this size and scope, much must be assumed or neglected. Innumerable nuances must be passed over in silence, and otherwise important contrasts between thinkers must be suppressed for the sake of the broader historical sketch. Thankfully, however, much of that historical sketch has been narrated or instructively debated by scholars of Locke (Dunn 1969, 1983; Farr 1987; Goldie 1983; Leary 1980; Yolton 1970), of Hume (Farr 1978, 1982; Forbes 1975, 1977; Livingston 1984; Moore 1977; Norton 1982; Whelan 1985), of Smith (Flew 1985; Haakonsen 1981; Harpham 1984; Skinner 1979; Skinner and Wilson 1975; Winch 1978); of enthusiasm (Knox 1950; Lovejoy 1985; Tucker 1972), of other figures in the Scottish Enlightenment in general (Camic 1983; Hont and Ignatieff 1983; Kettler 1965, 1977; Pocock 1975, chap. 14), and of yet other figures of decided importance for this stretch of intellectual history, especially Montesquieu (Richter 1977). This essay also aims to contribute to the resurgence of interest in the history of political science as an intellectual and political practice (see Collini, Winch, and Burrow 1983; Ricci 1984; Seidelman and Harpham 1985).

2. There are a number of reasons why, on Hume's extended account of "moral science," it could never succeed. For some nonpositivistic readings of Hume, see Farr 1978, 1982 and Livingston 1984.

3. Compare the sentiments Madison expressed to Jefferson in a letter of 24 October 1787 (his general subject is how religion does not constrain interest): "When indeed Religion is kindled into enthusiasm, its force like that of other passions is increased by the sympathy of a multitude. . . . Even in its coolest state, it has been much oftener a motive to oppression than a restraint from it" (quoted in Kammen 1986, 72).

4. Mill may have been influenced by the romantics—Coleridge in particular. For his part, when putting in a good word for enthusiasm, Coleridge casts a glance back at Hume: "Histories incomparably more authentic than Mr. Hume's (nay, spite of himself, even his own history) confirm by irrefragable evidence the aphorism of ancient wisdom that nothing was ever achieved without enthusiasm" (quoted in Tucker 1972, 48).

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# AMERICAN POLITICAL SCIENCE, LIBERALISM, AND THE INVENTION OF POLITICAL THEORY

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*The contemporary estrangement of political theory from political science is in large measure the product of a quarrel that originated in the challenge to the values of U.S. political science initiated by émigré scholars during the 1940s. The behavioral revolution was in an important respect a conservative rebellion in defense of the values of liberalism and related notions of science, relativism, and historical progress that had traditionally informed the discipline. This controversy in the context of political science fundamentally structured the discourse of academic political theory and the contemporary constitution of the field both as a division of political science and as a wider interdisciplinary enterprise.*

Only that which has no history is defineable.

—Nietzsche

This essay, most narrowly specified, is part of an account of the discursive history of academic political theory in the United States. It can, however, for at least two reasons, be construed as a study in the history of political science. First, the period under consideration, 1940–50, was a crucial one in the development of this discipline, and the issues that arose in the subfield of political theory were determinative with respect to its subsequent evolution. Second, the discourse of political theory was also the basic vehicle for reflection on the state of the discipline, its past, and its future prospects. But the relationship between political science and political theory has, since the beginning of the period in question, been an uneasy one. The principal purpose of this essay is to explain that relationship and to explore the origins of a controversy that funda-

mentally shaped the structure and content of contemporary academic political theory.

Political theory today has little to say to or about its parent field, and much of political theory is of marginal interest and intelligibility to many political scientists. This estrangement cannot be explained merely in terms of normal trends in professional differentiation. It is in part the legacy of an old quarrel that was one of the principal factors in the emergence of the independent interdisciplinary field of political theory. Since the early 1970s, the subfield of political theory in political science, once understood to be the core of the discipline, has tended to reflect concerns generated within the wider, more autonomous field that has evolved its own institutional structure, issue nexus, and self-image.

This situation has sometimes been perceived as a problem, but it has seldom occasioned great concern among either political theorists or political scientists as

a whole. In some quarters, both in political theory and mainstream political science, there is a sense of relief that the tension between field and subfield that had characterized the behavioral era has been ameliorated by intellectual distance. There are narrow but weighty professional pressures that tend to lead to the validation of separatism but that are rationalized in terms of notions of intellectual pluralism. Often the problem is depreciated by noting that it is more apparent than real if one takes into account the manner in which the policy turn of the postbehavioral era has produced a convergence between normative and empirical research. But the propensity to confront the issue on a superficial level conceals both the historical source of the problem and some very basic difficulties that cannot easily be dismissed.

A readily accessible and familiar explanation of the alienation of political theory from political science locates the cause in the conflict that arose in the course of the behavioral revolution and its attack on the study of the history of political theory as antiquarian and inimical to the development of empirical theory and a scientific study of politics. This explanation is not incorrect, but it is incomplete. It is necessary to reach a deeper historical sense of the genealogy of political theory and to recover some of the more fundamental issues that occasioned the controversy we associate with behavioralism and its aftermath. By the early 1950s some of these issues had already been submerged in the rhetoric and legitimating philosophies that characterized the debate between "traditional" and "scientific" theory. A pivotal transformation in the discourse of political theory, which occurred during the 1940s, was obscured. An understanding of that transformation is also important for exploring the general problem of the relationship between academic political inquiry and politics—a problem that was

at the heart of the controversy about political theory as it originally developed.

### **The Real History of Political Theory**

The basic images of political theory of and by which we are now possessed, images of theory as both a product and activity and as both a subject matter and mode of inquiry, have been generated within, and have little meaning outside, the language of political theory as an academic field. Attempts to endow political theory with world-historical significance by seeking its past in a great tradition from Plato to the present, by categorically defining it as the reflective and critical dimension of political life, by understanding it as a tributary of theory in the natural sciences, or by creating other putative identities that would enhance the authority of this professional field and provide foundations for its claim to knowledge, cannot withstand much analytical scrutiny (Gunnell 1986). Although we have become accustomed to thinking of the history of political theory as the chronologically ordered canon of classic texts, such history is in fact largely a reified analytical construct. What we might call the "real history of political theory" is the history of the academic field that created this image as its subject matter and projected it as its past. Furthermore, despite the current distance between political theory and political science, these images, and the field of political theory itself, were largely an invention of U.S. political science.

Although it is important to investigate the early development of the concept of political theory as well as the academic practice that gave rise to it (Gunnell 1983), the current species of both essentially came into existence after 1940 in the wake

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of the emigration of refugee scholars from Germany. Although the émigrés did not, for the most part, come to the United States understanding themselves as political theorists, this eventually became their identity. The adoption, appropriation, or discovery of this identity and the propagation of their image of, and concerns about, liberalism, as well as a related conceptual cluster including positivism and relativism, precipitated a scholarly and ideological conflict within political science that was manifest in a debate about political theory. Although the behavioral revolution was hardly one-dimensional, it was in an important respect initially a conservative rebellion catalyzed by, and directed against, the encroachment of a vision that was hostile to the traditional values of U.S. political science.

Notions of theory as the history of political ideas and as part of empirical social-scientific inquiry had, through the 1930s, been consistently understood as either complementary or even merely diachronic and synchronic modes of the same endeavor, which involved explaining politics by linking behavior and ideas. Charles Merriam, despite his emphasis on jettisoning or surpassing history as a form of inquiry, neither disparaged the paradigmatic studies of the history of political theories associated with his mentor, William Dunning, nor perceived any serious discontinuity between such studies—in which he himself participated significantly—and the advancement of social scientific theory. But a more significant factor in explaining the lack of tension was an underlying intellectual consensus that transcended various scholarly and ideological divisions between empiricism and idealism, history and science, and statism and laissez-faire. The history of political theory was understood by all as the history of the development of scientific knowledge about politics and as a story of the progress of the symbiotic relationship between such knowledge and the

evolution of liberal democratic thought and institutions.

The notions of political theory and the disciplinary practices that developed between the late 1800s and the 1930s were, to be sure, the receptacle for the discourse that emerged in the 1940s. The arguments, for example, of someone such as Leo Strauss about the rise and decline of the great tradition would not have taken hold if the basic idea of the tradition had not already gained conventional acceptance in the work of George Sabine and his predecessors, and there was nothing in the behavioral credo that had not been already articulated as an ideal to which political science should aspire. But the transformation was dramatic. The dominant consensus was frontally challenged for the first time, and political theory, as the focus of controversy, became an essentially contested concept.

The great change began almost unnoticed, or at least uncomprehended, as the claims of Strauss, Hannah Arendt, Hans Morgenthau, Theodor Adorno, Eric Voegelin, Franz Neumann, Arnold Brecht, Max Horkheimer, Herbert Marcuse, and others, in varying ways and degrees but inexorably, reshaped the discourse of political theory. Despite some very great differences in ideological and philosophical perspective, they looked much alike to the successors of Merriam as they uniformly and on very similar bases challenged the liberal, scientific, relativistic, historicist perspective that dominated political theory and political science. What might be taken as the politically conservative arguments of individuals such as Voegelin were initially more professionally visible than those on the Left associated with the Institute for Social Research, but they all propagated the thesis that liberalism, either inherently or because of its degenerate condition, was at the core of a modern crisis and implicated in the rise of totalitarianism.

It would be far too simplistic to suggest

that the behavioral revolution should be understood exclusively as a reaction to this challenge, but we have lost sight of the degree to which its arguments were originally formed in response to it. The attack on liberalism and science and the rejection of the progressive-pragmatic vision of history were too basic to allow any syncretic resolution. The sense of liberal givenness, which characteristically allowed U.S. theorists to embrace relativism and the idea of the separation of facts and values while remaining totally committed to definite political ideals, was incompatible with the transcendental speculation and philosophies of history, whether of the Marxist or natural-law variety, that marked the new literature. Since at least the turn of the century, such ideas had traditionally been specifically what most U.S. political theorists had understood themselves as rejecting.

I seek to specify as precisely as possible how the controversy commenced and how it shaped the discourse of political theory. The situation as it first emerged was not clear to many of the participants. The new ideas had often not taken distinct and published form when the conflict began, and some of those involved in the controversy were at first truly perplexed. Individuals such as Sabine, whose influential analysis of political theory was not significantly different from that of those devoted to empirical political science, found himself the target of those whose ideas he largely shared simply because he represented the history of political theory. Similarly, protobehavioralists and the founders of the behavioral movement, such as David Easton, saw not only a growing tendency toward historical and evaluative analysis that was pointedly at the expense of the idea of a scientific study of politics and liberal values but one that was being mounted within the genre of political theory, which had heretofore reflected the constitutive conventions of the discipline of political science.

### The Transformation of Political Theory

Apart from textbooks, there was at the beginning of the 1940s, a dearth of literature distinctly understood as political theory, and institutionally, as a subfield of political science, it had all but disappeared. The revitalization of discourse could be construed as beginning with the publication of Sabine's 1939 article, "What is Political Theory?" in the first issue of the *Journal of Politics* and with the appearance, in the same year, of the *Review of Politics* (which became a principle vehicle for the work of the émigrés). The resumption of discussion about political theory and its place in political science was in part prompted by an increasing concern about the status of the scientific study of politics which, despite the work of Harold Lasswell and others connected with the Chicago school during the 1930s, had received limited attention after Merriam's enthusiastic claims in the 1920s. More than a decade before the behavioral revolution, Benjamin Lippincott (1940) argued that in practice the discipline of political science was much as it had been at the turn of the century and that the greatest deficiency was theoretical. What passed as political theory had been devoted to the recounting of past ideas, and even among those committed to science, there was an "aversion" and "hostility" to theory (p. 130). But most important was the renewed reflection on liberal political values and the beginning of the impact of newly arrived European scholars who severely questioned the scientific faith and liberalism as well as the historical vision that united them.

A pointed "challenge" to political scientists was advanced by William Foote Whyte in an article published in 1943. This piece (quite accidentally) served as the principal catalyst for the incipient controversy. Whyte was a social anthro-



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pologist who had been studying political organization in a slum district, and there is no indication that he had any notion of the nerve endings that would be agitated by his comments. He suggested that the war had occasioned a concern about democratic values that had led political scientists to "write political philosophy and ethics" and neglect the study of "plain politics." He argued that "a scientific study of politics" required "the discovery of certain uniformities or laws" and that political scientists should direct their attention to "the description and analysis of political behavior" (pp. 692-93).

The most immediate response to Whyte was by John Hallowell (1944a) whose work would become one of the principal conduits through which the ideas of the émigrés entered political theory and whose voice would come to represent the new mood in the field. The response, however, moved the discussion to a very different context. Whyte, much like Lippincott, was writing from the perspective of traditional American social science, but Hallowell identified this critique, as well as the general commitment to science in the discipline, with an intellectual position that was not only alien but unfamiliar to most U.S. political scientists. Hallowell suggested that Whyte's views reflected "increasingly positivistic" trends that threatened to undermine "all belief in transcendental truth and value" that could serve as a barrier to intellectual and political "nihilism" (pp. 642-43).

The political-theory research committee of the American Political Science Association had been largely inactive in the early years of the war, but in 1943 it met, under the chairmanship of Francis G. Wilson, and attempted to sort out the issues that were beginning to surface. Wilson noted that there was now a "deep cleavage among political theorists in the area of primary ideas" on an "ultimate issue" (Wilson et al. 1944, 726-27). The exact nature of this issue and cleavage was

something that he had some difficulty in specifying. One facet of the issue, or one way of stating it, involved a conflict between those who, like the "great political thinkers," took metaphysics seriously and those who believed that philosophy was relevant only as "logical thought." Was scientific "detachment" from traditional philosophy a sign of "progress" or "ineptitude"? Another definition of the problem was in terms of a clash between the "theological approach" and the "empirical" or "'positivistic,' scientific, or liberal technique of social study." A theological perspective was considered by some as necessary for understanding both the past and contemporary society, but there was a question of what constituted such a perspective and the manner in which it might conflict with the "approaches of idealistic and rationalist liberalism" that were more characteristic of political science (p. 727).

There was also a recognition of a conflict between those who stressed "value-free discussions in political science" and those who believed that there was "more in politics than simply clinical observation." All agreed that it was important to "formulate and criticize values" and that there should be "a frontal attack" on the problem of "value in American political society," and many believed that in some sense it was necessary and possible to arrive at "valid social and political principles." There was much the same kind of discussion about knowledge of regularities regarding "political behavior." Most were willing to compromise at some point and acknowledge that both "utopia" and the "facts" should be given consideration (pp. 727-28, 730), but it was clear that the consensus in political theory was breaking down.

Some believed, Wilson reported, that the fundamental cleavage was manifest in the differences between the thought of the Middle Ages and that of the modern age, between natural law and natural rights.

There was also a division between those who argued that the United States needed a consciousness of history and that an examination of our philosophy of history was in order and those who wanted to concentrate on practical questions of political choice and the "ends-means relationship." There was also a general, but maybe less than enthusiastic, agreement that there was a need for a greater availability of the "texts of the great thinkers" and that "the political tradition of the West must be subjected to close scrutiny." The "ancients" should be studied as really "modern" and "timeless" because we belong to the same tradition but maybe also because there is an "essential nature of moral man or the moral universe (or even Satanic man and the Satanic phase of the universe)" to be discovered (pp. 730-31).

Wilson, who was a specialist in American political thought and a consistent spokesman for a U.S. brand of conservatism, did his best to report and make sense of the various and sometimes novel notions of theory that surfaced in these discussions and to reconcile them. Neither task was easy. The issues being formulated were radically different from those that had been characteristic of American political science and political theory.

In the symposium, Benjamin Wright (who had written on the American tradition of natural law) claimed that the greatest need of the period was for a statement of objectives in terms of ideals, since there was no "clear conception of what we are fighting for, what goals we should seek to attain, even in this country, after the war." We had "been too inclined to hide timidly behind the excuse of objectivity" (pp. 739-40). In this respect, he maintained, interpretations of the history of political thought and analyses of current doctrine could make great contributions to the future of democracy in the United States. Ernest S. Griffith, on the other hand, argued that "research in political

theory hitherto has been largely synonymous with searches for the origin, growth, and decline in ideas, principles, and doctrines," while what was required, if there was to be "precision" in the field, was more attention to "the basic concepts that underlie all theory" and to the definition of these concepts. Such research, he cautioned, should not be understood as a search for "correct" concepts, since theorists have too long looked for absolute principles and failed to recognize that principles are subjective and historically relative to various institutional arrangements (pp. 740-42).

The claims of Wright and Griffith could easily be situated within the characteristic discourse of the field and did not necessarily represent sharply conflicting attitudes. Wright's notion of natural law, for example, was largely functional or instrumental. The increasing tension between such positions, however, indicated a different kind of influence. Voegelin, who had fled to the United States in 1938, was beginning to make his mark on professional discussions of political theory during this period, and in this symposium he presented at some length the basic research scheme and thesis regarding the derailment of modern political thought that would inform his *New Science of Politics* (1952)—a book that would epitomize one of the poles of political theory during the 1950s and 1960s.

Voegelin suggested that the study of the history of political ideas had been represented with distinction by individuals like Dunning and Sabine and was of particular importance to U.S. scholars who had done the most to develop this field, over which they had held a "monopoly." He claimed, however, that it was now necessary to rethink the field in light of the vast new historical knowledge available and in terms of the general philosophy of history. Dunning, he argued, had made the first real step beyond earlier work by focusing on "political theory" as a special

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category, but his work was somewhat distorted by his emphasis on the notion of "progress." Sabine had taken the further step of making political theory distinctly subordinate to the structure of political history, and it now remained, Voegelin suggested, to pursue this task in a much more "philosophical" manner (pp. 746-51).

The increasingly pivotal issue of relativism was, at this time, grounded in a concrete concern about the defense of liberal democratic principles. The war and the domestic political crises of the 1930s that preceded it had prompted an uneasiness about the lack of an articulate and philosophically grounded democratic ideology. And the work of individuals like Charles Beard and John Dewey had already incited a controversy about historicism and relativism as a threat to both political values and scientific objectivity. The concern about relativism, then, was not without precedent. But as the issue entered the literature of political theory, it took on a reconstituted identity based on new claims about the origins of totalitarianism. The assessment advanced by émigré scholars, and by Hallowell, was that the problem of totalitarianism was at root a problem of science, liberalism, and relativism and that it was susceptible in large part to a philosophical or religious solution.

If we want to understand fully the behavioral animus toward "traditional" political theory, it is necessary to take account of the extent to which what had actually been traditional in American political theory was being fundamentally challenged. The unhappiness with liberalism and science that would mark much of the major work in the history of political theory by the 1960s (Strauss, Voegelin, Arendt, and others) was already taking shape. And it propagated the image of a crisis and decline in Western political history and thought. Hallowell, in reviewing Brecht's account of the decline of the Ger-

man republic, argued (1944b) that Brecht did not sufficiently stress the degree to which that decline was "a direct consequence of the liberal's lack of conviction in his own philosophy." The solution, for Hallowell, was a renewal of what he believed were the religious and spiritual foundations of liberal democracy.

Hallowell had developed this thesis in an earlier article (1942) and book (1943) in which he argued that the decline of liberalism as an ideology and the rise of totalitarianism were directly related to positivism and historicism and that a reconstruction of liberalism could be effected by a grounding in modern theology as characterized by the work of Niebuhr, Tillich, and Berdyaev. Voegelin reviewed the book sympathetically—suggesting only that Hallowell might seek the roots of the decline of liberalism further back than the nineteenth century. Voegelin was at this time working on a general (but later temporarily abandoned) history of political ideas that attributed the decline of the West, which culminated in Marxism, to the secularization of Christian history during the Enlightenment (1975). Where the tradition of political philosophy went wrong and led to the decadence of modern politics and political thought would become the principal concern of a new wave of historians of political theory.

Hallowell's attack on liberalism became strident (1947), and the substance of his argument was hardly congenial to American political science. Merriam, for example, had stressed the danger of theology to liberalism while Hallowell was suggesting that secular liberalism, with the disintegration of religious conscience, led to anarchy and tyranny. Much of the tradition of U.S. social science had grown out of an attempt to replace religion as a cohesive social force with a science of social control and public policy that would realize liberal values, but now "political theorists" were claiming that

liberalism and "the liberal science of politics" were a failure and the breeding ground of totalitarianism.

It would be a mistake to assume that political scientists committed to the scientific study of politics were unconcerned with values—either as an object of study or as premises and goals of empirical inquiry. As Herman Finer put it (1945), the need was for an ideology that would accomplish for democracy what Marxism had done for Soviet Communism" (p. 239). These claims would be overshadowed by the renewed commitment to scientific inquiry by the end of the decade, but the "value-free" stance of behavioralism did not entail a rejection or neglect of what it understood as liberal democratic values. Gabriel Almond, for instance, who had studied with Merriam and Lasswell, spoke out strongly at this time for the ethical involvement of political scientists and against Whyte's suggestion that values should be the province of the philosopher. Almond even argued that the "chief challenge" of the day was to seek "a valid theory of natural law" (Almond et al. 1946). The problem was not that the new influences in political theory stressed values but that behavioralism could not countenance the idea that science and liberalism exemplified social and intellectual decline.

The symposium, "Politics and Ethics," in which Almond participated was conceived as an attempt to explore further the issues that had been raised by Whyte. But, for the most part, the participants talked past one another, and the arguments were by now far removed from Whyte's actual concern and understanding. Whyte claimed that he did not recognize the target of Hallowell's criticism—that he had never even met anyone who represented the position attributed to him. He stated that he was not interested in the "philosophy of science" and "positivism" and that Hallowell had gone off on a "tangent" and created a "straw man"

(p. 301). Hallowell was caught up in a realm of ideas and issues that were distinctly European and closely related to the arguments of individuals such as Brecht. Brecht's work constitutes a particularly interesting and illuminating case. Although he posed the problem in a paradigmatic form, his solution was not quite consonant with the claims of Voegelin and Hallowell.

Brecht had been a political actor and administrator who did not enter academia, or what he understood as the pursuit of "science," until he came to the United States and the New School of Social Research in 1933 after a period of Nazi harassment. Around the turn of the decade, he published several articles dealing with the question of relativism, absolutism, and science that eventually became the basis of his book, *Political Theory: The Foundations of Twentieth-Century Political Thought* (1959). Brecht did not directly attribute the rise of national socialism to a philosophical context, but, like many others, he was concerned about the influence of legal positivism and relativism on judges and lawyers who applied laws that were morally reprehensible. He believed, however, that it was impossible to seek a form of "scientific" proof or some rationalistic equivalent for moral principles and natural law. Moral judgment was a matter of human volition even though no less lacking in validity. Brecht argued that scientific value relativism, as expounded by Max Weber, Hans Kelsen, Gustav Radbruch, and Lasswell, had once seemed a liberating philosophy, but in the face of Nazism it became a paralyzing one. In a search for intellectual authority to combat totalitarianism, intellectuals, however mistakenly, "turned from being political scientists to becoming ethical philosophers or theologians (if not simply bad logicians) and they called this science" (1970, 434, 490). For Brecht, the great dilemma of modern political theory was

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that there was no scientific knowledge of values. His solution was to teach "the limits of science" and urge other ways to bridge the *is* and *ought* (p. 494). He was an intensely religious Lutheran, particularly in his later years, and in his own way he attempted to ground political theory in faith and religious values.

In 1947, Brecht reported on a roundtable ("Beyond Relativism in Political Theory") that included Francis Coker, Roland Pennock, Lippincott, Wright, Voegelin, Hallowell, and Almond. Kelsen was unable to attend, but he sent a message indicating that despite the growing unpopularity of value relativism in the United States, he was still an adherent and remained a critic of moral "absolutism." Kelsen's legal positivism was a prime target of those such as Hallowell who wished to ground political theory in religion and natural law, but for Kelsen, as well as for most U.S. political scientists and theorists, value skepticism and relativism were understood as the foundations of liberal democratic theory. Only on this basis, they believed, could liberal democracy be justified. Moral absolutism signaled political absolutism for émigrés like Kelsen, while for Kelsen's erstwhile student, Voegelin, relativism abetted and reflected the decline of the West.

Brecht suggested that the paradox of modernity was that "modern science and modern scientific methods, with all their splendor of achievement, have led to an ethical vacuum, a religious vacuum, and a philosophical vacuum." He argued that the situation had come to a point where social science found it impossible to distinguish between right and wrong, good and evil, and justice and injustice. Political science was particularly affected by this dilemma, because it dealt most directly with the phenomena of nazism, fascism, and communism, which had "settled down" in this vacuum. Brecht claimed that "no political theorist can honestly avoid the issue" posed by "scien-

tific relativism" (pp. 470-71). Most participants in the symposium offered little in the way of a concrete recommendation for getting "beyond relativism." Hallowell made his characteristic case for Christianity as the basis of political theory, but Wright and Lippincott, speaking for the more traditional conception in American political science, warned against any such retreat to commitment.

There has not been sufficient recognition of the degree to which the transcendental urge and historical pessimism manifest in the émigré literature affected political theory. It contributed to the split between political theory and political science during the behavioral era and to the eventual constitution of political theory as an independent field. Although the behavioral revolution, or counterrevolution, in many ways transformed the mainstream practice of the discipline, it was, at least theoretically, a reaffirmation of past ideals—ideals that did not rest easily alongside the arguments that characterized the new literature of political theory. Although the behavioralists singled out the study of the history of political theory as requiring displacement, transformation, or supercession, this was more because it was the genre to which most of the offending arguments happened to belong than because the literature characteristic of the field before 1940 actually embodied the problems with which behavioralists were concerned.

By 1950, there was an antiscientific, antiliberal sentiment in political theory even though neither the medium nor the message were always entirely clear. Although, for example, Arendt had published widely during the 1940s in a variety of journals, her first major work in English, *The Origins of Totalitarianism*, did not appear until 1951. Much of Strauss's *Natural Right and History* was originally presented as Walgren lectures at the University of Chicago in 1949, a year after his appointment, but the book

was not published until 1953. Strauss had already announced in his work on Hobbes (1936) some of his distinctive themes, including the depreciation of both liberalism and the new science of politics, but the book received little attention and surely not much comprehension. The same could be said of Herbert Marcuse's *Reason and Revolution* (1941) and Horkheimer's *Eclipse of Reason* (1947). In an article-length review of Strauss's book, Carl Friedrich, himself an earlier émigré, praised the effort but clearly did not understand the project. He wished that Strauss had been more forthcoming about his own views, but he believed that it was safe to conclude that Strauss was a "historical relativist" (1938).

Although these attacks on science, liberalism, and the legitimacy of the modern age had not yet coalesced into a definite oppositional force in political science, they were at least engendering an alien mood that had a considerable professional impact. There were all sorts of reasons to reassert the scientific image of political science in the postwar years, and the new voices in political theory were not only failing to do so but were inimical to such an image. The need for scientific theory was the theme of William Anderson's survey of political science in 1949. What was required, he argued, was an "established body of tested propositions concerning the political nature and activities of man that are applicable throughout the world and presumably at all times" (p. 309). It was, he claimed, necessary to distinguish between politics and science and pursue the latter, and the way to do this was to study individual "human political behavior" or the "political atom" and produce knowledge in the "field of scientific method" (pp. 312-14).

Behavioralists, beginning in the 1950s, would increasingly emphasize their dedication to a vision of pure science, but the underlying concern at the end of the decade was still the articulation and reali-

zation of a science of *liberal* politics. In two articles, on Bagehot (1949) and Lasswell (1950), that closely preceded his analysis of the "decline of political theory" (1951), Easton made clear that the pursuit of science was a means and not an end and that the belief in the complementarity of scientific realism and liberal democracy was still alive. The ultimate purpose remained political rationality, but the immediate goal was to create a theoretically grounded science of politics equal to this task. The priority for the moment was an emphasis on what Lasswell and Abraham Kaplan termed the "contemplative" as opposed to the "manipulative" dimension of science (1950).

The individuals most responsible for affecting and sustaining the behavioral revolution had almost without exception been primarily trained in the field of historical and normative political theory, as traditionally conceived. While the debate within the discipline would eventually often be understood as a conflict between political *scientists* and political *theorists*, it really signified a split between different conceptions of theory. Few of the early behavioralists understood themselves as antitheoretical, and probably very few initially understood their concern with scientific political theory as a rejection of their earlier education. To some extent, what was involved was a lag between the graduate educational establishment and the new literature and mood of political theory. The theorists trained in the 1930s and 1940s had, for the most part, not been exposed to the arguments of the new wave of European thought that was appearing in political theory, and they felt a good deal more at home with the traditional goals of the field than with the perspective that characterized this literature. Ultimately they felt compelled to choose between political theory and political science, and the choice was as much ideological as professional. But by the mid-

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1950s, the behavioral critique of political theory was forcing the same polarizing choice on theorists concerned with historical and normative issues.

In 1950, Lippincott attempted, as he had a decade earlier, to make sense of what constituted political theory in the United States and to provide a critical analysis of the field. His discussion did not gain the attention received by some of the testaments of the behavioral movement that would be advanced during the next few years. But it indicates the degree to which the basic tenets of behavioralism had already been formulated and enunciated by individuals like Lippincott, who were hardly enemies of "traditional" political theory but who were clearly worried about the direction that the enterprise was taking and about its relationship to political science.

Lippincott suggested that political theory was at least potentially—and as correctly understood—the "most scientific branch" of political science. It was, however, an area in which political scientists had "produced little," if theory were defined, as he believed it should be, "as the systematic analysis of political relations." It should, he argued, be concerned with "general" claims about the "actions" and behavior of men" and with particular events and institutions only as objects for testing its principles (pp. 208–9). Up to this point, Lippincott argued, "the greatest effort has been devoted to writing the history of political ideas" and "defining and classifying terms and principles of politics." The methods in political theory had been basically historical, and the "emphasis placed on the history of political ideas has meant very largely the abandonment of the aim of science" (pp. 209, 211, 214).

The call for science was increasingly accompanied by an attack on the existing practice of political theory. Theory was often described as historical, teleological, utopian, moralistic, ethical, and generally

obscurantist. There was more going on in these critiques than was readily visible, and the exact target of the criticism was often not clearly or easily specified. By 1950, however, Hallowell had brought together his arguments in a comprehensive work (*The Main Currents of Modern Political Thought*), and to understand fully the image of political theory that behavioralism would invoke, this work must be examined. It reflected and summarized claims that had emerged in the discourse of political theory during the 1940s, and it initiated a genre that would find its classic expression in the work of Strauss, Voegelin, and others during the next decade. Even a work such as Sheldon Wolin's *Politics and Vision* (1960), which ideologically and philosophically was far removed from the positions of Hallowell, Voegelin, and Strauss, was still a story of liberalism and the decline of modern politics. The imprint of the 1940s was fundamental for the future of political theory.

Although Hallowell's book was about "modern political thought," a large portion was devoted to a synoptic reconstruction of the development of political ideas from ancient times up through the emergence of what he designated as "integral liberalism," which he claimed was an outgrowth of the ideas of individuals like Grotius and Locke. The image projected was one of a seamless web of history that moved from the classical world through the evolution of modern science, the Renaissance, and Hobbes to the rise of liberalism. However, the story of modern political thought was now a story of decline—a decline as organic as the upward slope of the past and one that was offered as an explanation of modern political problems and "the crisis of our times" exemplified in socialism, Marxism, the Soviet Union, fascism, and the theory and practice of totalitarianism in general. But the most basic problem was the decadence of contemporary liberalism and its inability to defend and maintain itself.

In Hallowell's saga, the main villain was positivism, which in turn he viewed as a direct outgrowth of the Enlightenment, utilitarianism, and German idealism. His notion of positivism was the very general one of an "attempt to transfer to the study of social and human phenomena the methods and concepts of the natural sciences," and he believed that it was represented by the work of Comte, J. S. Mill, Herbert Spencer, Frank Lester Ward, and Gumplowicz. Positivism, with its attendant separation of fact and value, and relativism undermined liberalism and allowed the rise of antiliberal political movements. For Hallowell, the answer to the modern crisis was once again Christianity and the reconstitution of decadent liberalism on the basis of religious values. This particular work, although reasonably influential in political theory, was seldom the specific focus of behavioral criticism, but it represented the kind of claims that in large part evoked that criticism.

Some émigrés like Kelsen (1948) and Felix Oppenheim (1950) would continue to argue, like U.S. liberals, that relativism and empiricism were more conducive to democracy than moral absolutism. They claimed that there was a root connection between democracy and the spirit embodied in the scientific method. The imminent debate between "scientific" and "traditional" political theory would be far from simply a debate about method and political inquiry or even about an emphasis on values as opposed to facts. It was, in the end, a debate about liberalism, its foundation, and its fate—and about the relationship between political theory and politics.

By the 1950s, the dispute in political theory was projected on the wider screen of academic philosophy and issues about the nature of science. This was already quite evident in the 1950 volume of the *American Political Science Review*. J. J. Spengler, speaking from the perspective

of an economist, claimed that it was time that political science was "transformed into a full-bodied social science" (p. 375). What was required, T. I. Cook argued in a discussion of this thesis, was the development of a general unified science of theories and postulates similar to that "developed in natural science before, and first completed by, Newton" (p. 388). Wilson, commenting on these claims, observed that U.S. social science had now come to share a common conception of theory, knowledge, and social reality that was based on scientism, empiricism, pragmatism, instrumentalism, positivism, relativism, and operationalism.

In a symposium on "The Semantics of the Social Sciences," which was basically a statement of the philosophical and social-scientific position of the Chicago school, Charner Perry, a Chicago philosopher and long-time editor of *Ethics*, argued that natural science had made its great advances by going beyond the lore and common sense embedded in "anthropomorphic and teleological" language. In political science, however, there still was "no important contribution in the field resulting from the application of scientific method." This could be attributed to the "theological, normative, or even moralistic terms" of "political theory" which "belong to subjective or fictitious universes of discourse quite inappropriate to a general science of society" (1950, 397-99).

There was an increasingly extravagant dimension to the rhetoric on both sides of the debate. Actual research and publication in the field hardly matched the picture of "positivism" that was painted by the critics of scientism, and the image of moralistic political theory on which the protobehavioralists focused was equally unreflective of the literature. To claim, as Perry did, that political science was "mostly history and ethics" was simply counterfactual, and to argue that "the propositions of political theory have a



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character of 'unreality' and futility that bars out any serious interest in their discussion" (pp. 399, 401) was clearly to speak generically on the basis of a narrow and unspecified body of literature. But there was, ironically, a sense in which each faction was now beginning to fulfill the image conjured up by its opponents.

Herbert Simon, George A. Lundberg, and Lasswell participated in the discussion and pressed, as they would in many forums, for making the language of social science scientific. Although they did not specify their sources, they were clearly no longer strangers to the positivist philosophy of science. Simon enunciated a model of science as a system of predictive-cum-explanatory general propositions from which could be deduced other propositions about concrete observables that were therefore testable. Science was conceived methodologically as a unity, and human action as an object of inquiry was viewed as requiring no departure from the methods of natural science.

For Simon, it was essential that political science adopt what he considered to be the more advanced methods of the other social sciences, and he insisted that central to this task was a *transformation and redefinition of political theory* that would entail "consistent distinctions between political theory (i.e., scientific statements about the phenomena of politics) and the history of political thought (i.e., statements about what people have said about political theory and political ethics)" (p. 411). Lundberg, a sociologist, who was a major advocate of positivism in the social sciences during this period, confirmed Simon's image of science and stressed the need to "adopt the orientation of modern natural science," which would involve as a first step finding out "what the methods of natural science are" and thus provide a means for getting away from the a priori philosophy characteristic of the humanities (p. 421).

Lasswell warned against getting too

involved in the question of what "science" means and neglecting the practical end toward which scientific inquiry was ultimately directed, that is, "to decrease the indeterminacy of important political judgments," but he advocated "more attention to the construction of theoretical models" that would guide research (pp. 423, 425). Exactly what was entailed by the commitment, or recommitment, to science was not very clearly defined, but there definitely was a new philosophic dimension to the argument. Lasswell's collaboration with Kaplan, who had been a student of Carnap's, was one obvious point of intersection. And they had stated in the introduction to their book (1950) that it reflected a new scientific outlook informed by "a thorough-going empiricist philosophy of the sciences" based on "logical positivism, operationalism, instrumentalism" (pp. xiii-xiv). Maybe their position was actually more justified than informed by this philosophy, but in the face of new philosophical challenges, political scientists attempted more self-consciously to ground and articulate their scientific faith.

By 1950, some mainstream political scientists were becoming uneasy about the implications of the new scientism. Paul Appleby (1950), speculating on the direction of political science during the next 25 years, argued that a much broader value-oriented vision was required than the one that was then developing in the discipline, and for many the concern was still to link science to problems of practical politics (White 1950). Easton's call for the reconstruction of political theory has quite reasonably often been construed as the first shot fired in the behavioral revolution. But although *The Political System* (1953) was, to be sure, a critique of the study of the history of political theory and a plea for the creation of scientific theory, it is sometimes forgotten that it was an explicit challenge to the new "mood" in political theory and an attempt

to find a basis for the reconciliation of political science and political theory that would at the same time sustain and advance liberal democratic values (chap. 1). By mid-decade such mediative integrating efforts had largely given way to the alienating forces that pushed political theory and political science along different paths. And in an important sense the language and agenda of political theory, for at least a generation, had already been set.

### Conclusion

The literature of political theory is, and since the late 1930s has been, saturated with discussions about liberalism and its tradition, rise and decline, faith, dangers, limits, collapse, challenges, agony, paradox, irony, spirit, development, end, poverty, and crisis and its relation to innumerable things, individuals, and other political concepts. Even when the concept is not specifically a focus of discussion, the concern about liberalism significantly structures the discursive universe of political theory. An understanding of this situation requires a grasp of those initiating issues that informed the invention of political theory and that fundamentally shaped its real history. The enthusiastic reception of European thought in recent years, such as that associated with Habermas, is directly related to its perpetuation of the critique of liberalism, and surely some of the literature that has had the greatest impact on the field has been devoted to attempts, such as that of John Rawls, to provide new foundations for liberal theory.

Although the tension between political science and political theory was, and in large part has remained, the product of a disagreement about liberalism, the controversy quite early on became detached from concrete political issues. From the beginning, there was a tendency for the

debates about liberalism to escape the political concerns that had given rise to them. For the émigrés, the experience of Weimar and totalitarianism was determinative and came close to confirming the idea that in Germany, as in Athens and Rome, liberalism was the manifestation of political decline and the threshold of tyranny. No matter how much individuals like Strauss and Marcuse might disagree on various grounds, they were at one in extending the analogy to contemporary politics in the United States. And even their specific critiques of liberalism were not all that different. For U.S. political scientists also, the rise of totalitarianism and its attendant ideologies during the 1930s, as well as the domestic problem of rationalizing the conflict between government and the economic enterprise, indicated a need to rearticulate and ground the principles of liberal democracy. If a sense of crisis regarding liberalism, in theory and practice, had not been part of the world of political science, the arguments of the émigrés would likely not have carried the weight that they did.

On both sides of the controversy, however, there continued to be an alienating displacement of the issues. In the case of the émigrés, there was a philosophization of their experience and a projection of the analysis onto recalcitrant political circumstances in the United States as well as the ambiguous screen of world history. This produced an estrangement between academic political theory and politics that has plagued the subsequent evolution of the field. The defense of liberalism among political scientists also became oblique. Either it became the validation of an image that was not easily related to political realities, or it took the form of a philosophy of science, conceptual frameworks, and empirical findings that often seemed to confirm the existence and efficacy of liberal society.

The debate about relativism was also originally grounded in a practical issue

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that was eventually obscured by the philosophical formulation of the problem. This was the problem of the relationship between academic and public discourse or between philosophy, political theory, and political science on the one hand and politics on the other. This concern about the authority of political inquiry and the intellectuals who engaged in it was equally prominent in the work of both the émigrés and the practitioners of traditional American political science, but the responses were different.

Although relativism, as a philosophical problem, may arise in reaction to crises in substantive practices such as science and politics, it more basically and frequently reflects a crisis in the understanding of the relationship between first- and second-order discourses, that is, between, for example, academic political theory and politics. It is a problem in part because second-order inquiry conceives its mission as the proprietary one of discovering, validating, or explicating the grounds of judgment in the activity it has appropriated as its subject matter. But the problem is often only tangentially one of the possibility and integrity of practical judgment. The pursuit of a solution to the problem of relativism is a quest for a comprehensive answer to the infinitely complex and contextually diverse questions attending the nexus between theory and practice. It most essentially reflects a crisis in the self-image of political theory and the foundations of its claims to knowledge.

The world of the émigrés was one of not only political but intellectual and professional insecurity, and the problem of relativism was a philosophical expression of their anxiety. The question of the authority of academic inquiry was an important concern for political science, but there were several reasons why the discipline tended not to perceive relativism as a threat. Easton's remedy for the decline of political theory, for example,

called for the reconstitution of value theory without jettisoning the liberal belief in the relativity of values and their emotive nontranscendental basis. One reason that political science was able to absorb the issue of relativism was because it was an essential part of the pragmatic liberal creed of fallibility and progress in both scientific inquiry and politics. But the liberal science of politics had its own grounds of certainty.

There was, first of all, the belief that history validated liberalism and that liberal values were embedded in the basic institutions of U.S. politics. Relativism never was taken as implying that one value was as good as another but only that values reflected contexts and perspectives. Furthermore, certainty was to be achieved through science. Although science could not yield values as such, it seemed to select, confirm, and discredit them in various ways, and, again, there was the belief that the logic and procedures of science and liberal democracy were inherently mutually supportive and that both rested on a pragmatic notion of truth and the eschewal of absolutist speculation. Finally, political science was, much more than the emerging autonomous enterprise of political theory, an established disciplinary practice with its substantive criteria of judgment and, despite continuing concern about its public role and possibilities, a sense of practical efficacy and professional identity. It was, consequently, less daunted by the philosophical phantom of relativism that haunted émigré political theorists.

The discipline of political science constituted a context that profoundly and singularly shaped the institutional form and ideational content of political theory. Political theory, in turn, was deeply implicated in some of the most fundamental changes that took place in political science, including both the behavioral and postbehavioral revolutions. Today, political theory has largely retreated from an

involvement with, let alone critique of, political science, and political science has, for the most part, afforded political theory the ultimate disdain of pure tolerance. This situation has allowed many important issues, both explicit and implicit, to slide into obscurity or to become, as in the case of liberalism and relativism, detached from the practical concerns that originally generated them. Professional pressures, under the guise of intellectual principle, continue to push in the direction of separatism, but the results are debilitating. Both political science and political theory have been diminished. The former has lost its most important critical and reflective dimension, and the latter has lost its congenital and maybe most authentic field of action.

### Note

This essay is based on a manuscript (in progress) devoted to a study of the history of political theory as an academic field and conceived as a case study of the relationship between academic and public discourse in the United States. Earlier versions were delivered at the Yankee Conference for the Study of Political Thought (Amherst, MA, 1984), the annual meeting of the American Political Science Association (New Orleans, LA, 1985), and the Joint Finnish-International Political Science Association Symposium on the Development and Institutionalization of Political Science (Helsinki 1985). A longer version will be published in a volume edited by Dag Anckar and Erkki Berndtson.

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# POLITICAL JURISPRUDENCE, THE "NEW INSTITUTIONALISM," AND THE FUTURE OF PUBLIC LAW

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*For the last quarter century public-law studies have been dominated by "political jurisprudence," which tries to understand law as a product of political forces. Critics claim this outlook, as now articulated, has generated fragmented empirical work disconnected from larger normative issues. This essay uses recent "institutionalist" or "structuralist" perspectives, based on critiques of pluralist political science and both Marxist and functionalist sociology, to propose a political approach to public law studies that can avoid such criticisms. If both empirical and normative public-law scholars took as their central concern the "dialectic of meaningful actions and structural determinants" and recast their research in several specified ways, they might be better able to describe the role of normative ideas in law and to achieve a broader empirical agenda that could ground and inform normative debates.*

In a 1984 symposium, several leading political scientists specializing in public law considered what directions their subfield might take in the future. They generally agreed that a "vision of political jurisprudence," calling for analysis of law and courts as aspects of broader political processes, dominated work in the field. But they perceived a plurality of approaches under this label, some in sharp tension with others. Studies focusing on the interplay of courts and interest groups; the politics of appointive processes; courts as small-group decision makers; the implementation and consequences of judicial rulings; the socioeconomic makeup of bench and bar; statistical-attitudinal analyses of judicial voting behavior; litigative versus non-litigative means of conflict resolution; comparative studies of legal systems; and much more were all identified as currents flowing within the broad river of political jurisprudence (Danelski 1984; O'Brien

1984; Sarat 1984; Shapiro 1984; Stumpf 1984).

Reflecting on this diversity, David O'Brien (1984, 561) argued that despite its hegemony, political jurisprudence had proven unable truly to unify the subfield into an influential intellectual discipline. He traced this inability to the approach's failure to link "normative jurisprudence and positive political analysis" adequately. Empirical work had splintered and sometimes faltered due to the absence of any theoretical structure that could draw on normative concerns to define an appropriate agenda for political jurisprudence's "descriptive enterprise."

In contrast, Martin Shapiro (1984, 543-44) opposed seeking such a synthesis. He insisted that political jurisprudence had succeeded in becoming the modern orthodoxy even in law schools. Much progress would be lost, he feared, if careful empirical work should give way to a more old-fashioned "jurisprudence of

values" that would be "really a branch of normative political philosophy." Yet he acknowledged regretfully that many younger scholars were moving in precisely this direction.

The symposium's portrait of the subfield suggests a multiplicity of approaches that threatens to break out into renewed clashes between "behavioralist" and "normative" public-law scholars, now perhaps compounded by intergenerational divisions. This essay argues that any revival of these longstanding feuds is pointless and avoidable. Recent directions in research in a number of fields, of a sort dubbed the "new institutionalism" by James March and Johan Olsen (1984), suggest how public-law scholarship might be recast to unify many of its longstanding descriptive and normative concerns. By explicitly designing their studies as explorations of what Theda Skocpol (1984, 4) has termed "the dialectic of meaningful actions and structural determinants," political scientists with many different interests might pursue them so as to facilitate the communication and comparison of their results not only among each other but with the work of empirical political scientists and normative theorists more generally. That possibility, to be sure, is speculative, but it is worth considering the potential of these recent trends to provide more common ground for public-law scholars.

### Two Roads to the New Institutionalism

It is only fair to acknowledge at the outset that much of my own previous work falls squarely within what Shapiro labels, rightly, the *jurisprudence of values*. Even so, I have come to share his concern that public-law scholarship will not flourish if all scholars focus simply on spinning out their own normative legal theories. While the subfield cannot be vital without vigor-

ous normative debates, the ratio of new insights to reworkings of established views will be disturbingly low if disputes over values are all that is on our agenda.

But many recent efforts to reinvigorate qualitative inquiries into norms, values, and ideologies within public-law scholarship do not really represent efforts by legal scholars to be political or moral philosophers. Rather, they are attempts to integrate the study of ideas in law with *descriptive* studies of the historical evolution of political institutions and behavior. These authors regard qualitative studies of the patterns of reasoning characteristic of various strains of legal discourse as investigations into one dimension of actual political conduct—a dimension that needs to be assessed like any other if we are to build up a comprehensive empirical portrait of political life (Gordon 1984, 57–125; Smith 1985, 5–9).

It is true that this focus on values and ideologies derives from several objections to how political and legal evolution have previously been depicted, including reductionist treatments of normative issues. And while many recent writings aim at better empirical accounts of legal ideas, it is also true that they can be quite valuable for normative debates. Much of this sort of descriptive work turns out, incidentally but felicitously, to dovetail nicely with emerging modes of moral argument. Even so, the claim that qualitative studies of the historical role of legal ideas can contribute to empirical political science as well as to moral philosophy deserves to be taken seriously.

A major reason for granting credibility to this claim is that there are two distinguishable roads leading to the study of ideas in law as part of the new, developmentally focused "institutional" analyses March and Olsen describe. These roads take as their points of departure two distinct and important academic strains, one in contemporary political science and one in sociology. Their convergence suggests

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that "new institutionalist" accounts are indeed responding to real weaknesses in current perspectives.

The road March and Olsen stress is a path of reaction against the treatment of legal and political institutions simply as epiphenomena of self-interested individual and group behavior—a treatment dominant in mainstream U.S. political science since the 1950s and visible in much of what Shapiro terms *political jurisprudence*. The other road is one trafficked mostly by neo-Marxist comparative scholars and somewhat like-minded members of the Conference on Critical Legal Studies. These writers are reacting against traditional Marxist and sociological reductions of such institutions to epiphenomena of economic relations or the functional needs of social systems. Instead, they stress the "relative autonomy" of political and legal organizations, including recurring organizations of legal ideas, from all such socioeconomic "deep structures."

As suggested above, the convergence point of both reactions is on the importance of the interrelationship between human "institutions" or "structures" and the decisions and actions of political actors. In these approaches to the study of politics, institutions are expected to shape the interests, resources, and ultimately the conduct of political actors, such as judges, governmental officials generally, party or interest-group leaders, and other identifiable persons. The actions of such persons are in turn expected to reshape those institutions more or less extensively. Ideally, then, a full account of an important political event would consider both the ways the context of "background" institutions influenced the political actions in question, and the ways in which those actions altered relevant contextual structures or institutions.

The term *institutions* as used here has a quite inclusive meaning but no more inclusive than Samuel P. Huntington's

definition of institutions as "stable, valued, recurring patterns of behavior" (Huntington 1968, 12). Indeed, a focus on the "interplay of meaningful actions and structural contexts" means that political scientists might plausibly narrow Huntington's definition somewhat (Skocpol 1984, 1). They would normally attend only to relatively enduring patterns of behavior that (1) have arguable importance for human decisions that significantly shape social development and (2) appear subject to meaningful modification through such choices and conflicts. Nonetheless, this definition is broad enough to include not only fairly concrete organizations, such as governmental agencies, but also cognitive structures, such as the patterns of rhetorical legitimation characteristic of certain traditions of political discourse or the sorts of associated values found in popular "belief systems."

Thus these definitions can be useful in analyzing the elements and tendencies of legal and political outlooks or ideologies as well as behavioral regularities more narrowly defined. They also preserve at least the possibility that concrete political choices will prove to have important consequences, intended and unintended, that result not from any shaping "structural context" but from the actors' own creativity. It is their attention to the role of ideas and their assumption of the potential meaningfulness of political decisions that make these approaches well suited to speak to normative as well as empirical concerns. They can provide descriptive materials for the sorts of pragmatist moral arguments increasingly advocated by a diverse range of political theorists. Richard Rorty (1979, 1986), Alasdair MacIntyre (1981), and Michael Walzer (1983), for example, all agree that philosophers can best assist prudential deliberations on current issues by identifying and assessing our constitutive moral traditions and their historic role in political life.



Such accounts are precisely what descriptions of legal ideologies as political "institutions" or "structures" provide. Attracted by the promise of such benefits, then, let us begin appraising the arguments for these approaches by examining the two roads to the new institutionalism in more detail.

### **The Road from Mainstream Political Science**

As March and Olsen argue, since the behavioral revolution of the 1950s, much U.S. political science has analyzed politics chiefly in terms of the conduct of individuals or organized groups. Their behavior is normally portrayed as the result of rational calculations designed to advance the individual's or group's self-interest. The "behavior" of political institutions, in turn, is treated as simply the product of complexly interacting individual and group choices and actions. Such analyses tend, moreover, to regard the interests and resources of political actors, and hence much of the content and consequences of political action, as ultimately reflective of deeper social and economic forces which shape politics much more than politics shapes them. Thus the preferences and powers of political actors are often treated as exogenous "givens" in political analysis. (Although many desires may of course be described as internal to persons, those impulses are regarded as determining forces that are relatively immune to political action—so their roots are not analyzed when politics is studied.) And while such preferences or interests might be defined quite diversely, much modern political science presumes that concerns to enhance one's economic condition and political power are usually decisive (March and Olsen 1984, 735–38).

Numerous writers also assume that the political choices born of rationally self-interested calculations usually result in outcomes that are "efficient" or "func-

tional," at least for the dominant elements in the prevailing social and political structures (March and Olsen 1984, 737). As March and Olsen fail to note, however, many critics have assaulted the political science of the 1950s for assuming too complacently that all possible "groups" latent in a society can develop organizations to protect their interests effectively. These writers hold that many significant interests are frequently left inarticulate, excluded, or systematically slighted (Connolly 1969, 13–19; Lowi 1979, 57–63; McConnell 1966, 7–8). Thus they believe the apparent systemic "efficiency" of the decisions of ruling groups is often accompanied by frustration for others, who may ultimately produce significant social ruptures. But after making these important criticisms, only a few of these analysts have gone on to challenge the prevailing picture of politics as largely a matter of self-interested individual and group behavior.

According to the view still dominant in political science, then, politics is comprehended as kind of drama at sea. It occurs on the surface, where individuals and groups sail along, rationally navigating in pursuit of booty, frequently clashing with each other in the process. Their fates, however, frequently depend on the dynamics of the largely uncontrollable economic, technological, and social forces that surround them. The student of politics is mostly concerned with making sense of the calculations and actions of the pilots and crews, but the analysis often rests on showing how they could not safely act otherwise.

In some important respects, this image of political life has only been polished by the development of rational-choice theory into a more formal paradigm for virtually all political and social, as well as economic, analyses, although certain significant departures are visible as well. Leading works in this mode have stressed the constraints that information costs impose

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on decision making, giving this portrait of human conduct more empirical plausibility. They have then explored the interconnections between calculations of individual interest, particularly those of political "entrepreneurs," and the creation of organized self-interested groups. Much attention has also been given to how institutions try (and often fail) to aggregate individual choices coherently under various decision rules, and how these complications affect political calculations in groups small and large. And rational-choice theorists continue to encompass more and more political behavior within such analytical frameworks (Buchanan and Tullock 1962; March and Simon 1958; Mueller 1979; Olson 1965; Riker 1962; Simon 1957).

Many of these analysts do break significantly with more traditional studies of individual and group behavior in at least two respects. They are often unconcerned about whether rational-choice models really *describe* the actual decision processes of human beings in any very precise way. For example, the economist-lawyer Richard Posner (1977, 12-14) follows Milton Friedman in maintaining that all truly scientific theories are abstractions that are "necessarily 'unrealistic' when compared directly to actual conditions." The question should be not whether a theory captures "the full complexity, richness, and confusion" of human behavior, but whether it proves to have "utility in predicting" conduct, so that it can be relied on in designing policies and political strategies. In Posner's view, rational-choice models used by economists and other public-choice theorists have been shown to have "surprising" predictive power, far more than the available alternatives; so they offer the most promising direction for social-scientific research.

Partly as a result of this diminished concern to describe social reality in all its complexity, many rational-choice analysts have also not been greatly concerned

to explore how far the array of effective, organized groups in political life mirrors the "actual" structure of group interests in society. Because their models suggest that nearly all groups are products of self-interested political organizers and subsidizers offering incentives that generate group membership and activity, it is the presence, skill, and opportunities of such leaders and patrons that determine whether a potent interest group will emerge. Thus while the disadvantaged clearly face special burdens in gaining organized advocacy, there seems little point in trying to determine what groups, organized and unorganized, might reflect the "real" structure of interests in society. We can best answer questions about why certain interests are and are not represented by keeping our focus on the calculations and resources of these leading political actors (Hansen 1985, 93-95; Salisbury 1969, 11, 23-24; Walker 1983, 402-4).

These differences, however, only take to an extreme certain tendencies in earlier mainstream political science, particularly the inclination to use self-interested calculations of wealth and power maximization as the basic model of political behavior. Thus rational-choice models are not likely to satisfy those who believe that this perspective recognizes at best a narrow subset of possible human standards, and they are even more frustrating to those who believe much political science is methodologically insensitive to the claims of the oppressed. Yet for many, the rapid growth of rational-choice analyses in several disciplines suggests that this general approach to politics will eventually be able to provide more theoretically rigorous and empirically falsifiable accounts of virtually all of political and social life.

Hence we should note that William Riker (1980, 444-45) attempted a particularly relevant expansion of the rational-choice paradigm when he called for a

"return to the study of institutions," out of recognition that they systematically exclude or include certain "tastes or values." He cited pioneering studies by Kenneth Shepsle (1979) on how a congressional committee system may achieve "structural equilibrium"—a condition in which it is possible to pass motions that genuinely cannot be defeated by any other alternatives—if committee jurisdictions compel the disaggregation of multidimensional issues and if certain other requirements for members' preferences are met. More generally, Riker argued that we must use the tools of rational-choice theory to explore how institutional arrangements, themselves the products of past political choices, act as "congealed tastes" that influence the kinds of values that are "feasible and likely" outcomes of current decision processes.

While such extensions of the rational-choice paradigm to include institutional analyses are important, they do not alter the approach decisively. These writers still contend that one should build up a picture of institutional structures by stressing the individual and group decisions and actions that led to their creation; and they continue to treat tastes and preferences "as given." These tastes are impulses that may be blocked or advanced by different institutional arrangements, but they are otherwise exogenous to institutions, products of other forces that in the long run "cannot be denied" (Riker 1980, 444; Riker 1982, 190). These writers also still conceive of "tastes or values" in utilitarian fashion, as preferences—usually for wealth or power—to be maximized. Hence their focus remains on individual and group calculations of how to achieve values that are thought of as externally determined and described in rather reductionist terms.

It would be more tedious than difficult to show that much scholarship in "political jurisprudence" falls within the broader behavioral approach that since

the 1950s has led, among other things, to the proliferation of rational-choice analyses. A few leading examples should suffice. None could be more appropriate than the seminal exposition of that jurisprudence by Martin Shapiro (1964, 7–8), where he called for examination of institutions in terms of "the behavior of their personnel, and their places in the various decision processes." And he identified David Truman's classic effort "to analyze all government in terms of the influence and interactions of interest groups" as a chief "catalyst for the new jurisprudence." Shapiro's subsequent work consistently analyzes judicial decisions as responses to constituent claims and as expressions of power-seeking political actors, as does, for example, Stuart Scheingold's influential essay on the "politics of rights" and the growing literature linking patterns of judicial decisions with patterns of party realignment (Adamany 1980; Funston 1975; Lasser 1985; Scheingold 1974; Shapiro 1978, 1981, 1986). The many empirical analyses that connect judges' decisions to attributes such as their socioeconomic, educational, or professional backgrounds or to "values" treated as external "givens," rest on a similar picture of political actors as calculators advancing preferences or interests that are more the results of powerful sociological forces than their own deliberations (Gibson 1978; Goldman 1982; Rohde and Spaeth 1976; Schubert 1975; Tate 1981).

Recently, drawing on the newer rational-choice mode of analysis, Jeffrey Segal (1984) has employed the contemporary recognition of bounded rationality to describe how judges simplify their choice problems by relying on cues, such as certain facts about each case. At varying levels of formality, other political scientists have used the game-theoretic-style calculations involved in negotiation among multiple members of a decision-making group in the problems of aggregating choices, to clarify judicial strate-

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gies and to determine likelihood of winning coalitions in decision-making processes (Murphy 1964; Rohde 1972).

All this work is valuable, the image of politics that it elaborates is in many ways compelling, and it is at best premature to dismiss the hope that these endeavors will lead us closer to more truly scientific political studies. Nonetheless, the limitations of these approaches suggest that their underlying conceptions of politics and their paradigms for political analysis must be significantly if not fundamentally modified to be fully adequate to human experience. While March and Olsen and others canvass many objections, the root problem is that all these fairly diverse writers treat the resources, the institutional environment, and especially the very values and interests of political actors as exogenous, as determinants of political-choice situations that shape events while remaining more or less impervious to conscious human direction themselves.

As March and Olsen note, experience seriously challenges this picture. Political institutions appear to be "more than simply mirrors of social forces." They are themselves created by past human political decisions that were in some measure discretionary, and to some degree they are alterable by future ones. They also have a kind of life of their own. They influence the self-conception of those who occupy roles defined by them in ways that can give those persons distinctively "institutional" perspectives. Hence such institutions can play a part in affecting the political behavior that reshapes them in turn—making them appropriate as units of analysis in their own right.

The role of institutions, moreover, goes well beyond providing the rules governing decision-making situations in the manner Riker stresses. It influences the relative resources and the senses of purpose and principle that political actors possess. And sometimes, at least, those

purposes and principles may be better described as conceptions of duty or inherently meaningful action than as egoistic preferences. Correspondingly, the behavior they alter may serve other values than economic or systemic functionality (March and Olsen 1984, 738–42).

These criticisms can be made more concrete by placing them in the context of public-law research. It is unquestionably of some value to know that, for example, the votes of moderate justices in search-and-seizure cases are significantly correlated with facts such as the place of the seizure, the extent of the search, and the presence or absence of a warrant, and that judicial votes on civil-liberties and economic issues can to a large degree be linked to judicial attributes such as age, educational and professional background, and partisanship (Segal 1986, 942; Tate 1981, 355, 359–63). But we might learn more about the crucial factors in judicial politics by also asking if there are established police practices, or inherited values, that lead justices to think searches in certain places are more problematic; or if we identify the content and sources of the typical experiences influencing judicial attitudes that attributes like educational and professional background signal; or if we study the institutional constraints on the sorts of justices that are likely to be sitting on the bench at a given time. We might also wish to consider whether judicial voting patterns affect the types of searches police conduct; whether prevailing popular notions about the privacy of certain locations do so; whether judicial decisions on various economic and civil-liberties issues assist or alter the institutions that shape and select justices with certain attitudes; and other questions of this order.

Obviously, such issues are so complex as to be largely beyond the scope of any rigorous, essay-length study of judicial decision making. But the "new institutionalist" argument is that we must not there-

fore displace them from the discipline's research agenda, declining even to suggest how narrower findings might have *implications* for these broader questions, not to speak of pursuing the latter directly. When limited inquiries dominate the field of research, too many of the decisive elements in politics are left unexplored. Yet in political life, many economic currents and even political actors' own purposeful commitments are affected by relatively enduring legacies of past political choices. If these elements are left permanently at the margins of analysis, the result can be a restricted, atemporal view of politics that says little about the factors it holds itself to be most potent in political life. Political action, such as judicial decision making, then inevitably seems a tedious, crassly self-interested, and rather ineffectual game among programmed players.

Adherents of rational-choice perspectives might reply that this "new institutionalism" stresses the importance of background structures and apparently inefficient historical processes over the intelligible free choices of identifiable political actors, thereby raising the specters of determinism, blind chaos, or both. Yet ironically, the writers who have followed the second road to the new institutionalism have done so precisely because they believe it finds more room for meaningful political decisions than the deterministic outlooks with which they began. Before further describing and assessing the implications of the new institutionalism for public law, let us trace this second road.

### **The Road from Historical and Sociological Determinisms**

The contention that interest-group theories of politics and later rational-

choice models all neglect how resources and values arise historically is, of course, not new. Numerous schools of thought endorse these accusations. The perspectives that have most influenced political science are probably Marxist historical analysis and the non-Marxist "structural-functionalist" sociology elaborated by the U.S. students of Max Weber, Talcott Parsons, and Robert Merton, as adapted by leading political scientists, notably David Easton and Gabriel Almond. Common to both these schools is an effort to go beyond the surface of individual and group conduct to identification and analysis of the deeper social and economic forces that—interest-group analysts acknowledge—seem to drive human political life. Marxist and non-Marxist sociologies identify these forces differently, however.

In its most bare-bones versions, Marxist historicism provides the famous argument that relationships to the means of production are the root determinants of the prevailing modes of social and political organization and the processes of conflict and change they display. Political actors can be placed into classes according to where they stand in relation to the dominant productive forms, and their conduct (and that of the institutions they create to further their interests) will ultimately be explicable in terms of the economically based imperatives facing those thus situated (Tucker 1978, 473–83, 487).

In contrast, Parsonian "systems analysis" or "structural-functionalist" refuses to give such utter preeminence to economic relations. It argues instead that human life should be analyzed in terms of the multiple, interlocking social, cultural, and psychic "systems" that organize activities in ways that prove to be more or less enduring. On this view, all systems with any prospect of survival share certain formal characteristics and behaviors, various functional necessities and modes

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of adaptation to meet those necessities. Among these are certainly functions of resource production but also functions of socialization (or "pattern maintenance"), coordination (or "integration"), and "goal attainment" more generally. While studies of individual and group behavior can be included within such a framework, these types of systemic structures and functional needs become the focal points of sociological analysis. Within political science, David Easton (1953) influentially modified Parsonian systems theory by laying greater stress on the distinctive importance of the "political system" and on "input-output" analysis, while Gabriel Almond (1960) emphasized the adaptive features of systems in order to elaborate a less static, more developmental version of structural-functionalism. But all these writers shared a concern to provide theories that would explain behavior in terms of the needs of deeper social structures without deriving all behavior from the prevailing mechanisms of economic production (Barbrook 1975, 40-67; Parsons 1951; Skocpol 1984, 2-4).

Despite that difference, recent scholars with historical, sociological, and often Marxist sympathies have rejected both these older Marxist and non-Marxist alternatives to individual and group behavioral analysis. Again their criticisms are many, but the basic objection here is that these positions go too far, treating history, politics, ideologies, and institutional behavior as *entirely* dictated by economic or systemic necessities. Instead, many contemporary writers, like March and Olsen, stress the "relative autonomy" of at least some aspects of political action from such deeper structures, and they perceive many fairly long-lived social systems and processes of historical change as tragically inefficient, nonfunctional, indeed oppressive.

Different scholars make these points in different ways and to different degrees.

Within political sociology and political science, Theda Skocpol (1979) and Stephen Skowronek (1982) have provided influential analyses of how *state* institutions have played a relatively independent role in shaping political development during the French, Russian, and Chinese revolutions in Skocpol's account and during the more peaceful and piecemeal institutional adaptation to industrialization in the United States described by Skowronek. Inspired in part by Antonio Gramsci, many historically minded members of the Conference on Critical Legal Studies have instead emphasized how legal ideologies, like political ideologies conceived more broadly, are "relatively autonomous" from economic and systemic "necessities." Hence, legal doctrines merit careful study. In the words of Mark Tushnet (1981, 30-31), they form "groupings of ideas connected by repeated association," which may be burdened by "internal tensions." Those tensions can generate some processes of adjustment and change that are largely independent of outside forces. Any adjustments made may in turn shape the choices and behavior of, at least, legal actors within the political system. People, it is assumed, are psychically driven to "interpret the material conditions of their existence in ways that make their experience coherent," and they may at times seek to alter those conditions in order to make coherent interpretations more possible.

Critical legal scholars disagree on just how "autonomous" legal ideologies are. Tushnet (1981, 30) remains close to traditional Marxist perspectives by arguing that "we can still expect the law to embrace positions that are required by the interests of the ruling class as a whole, even if they are inconsistent with the interests of individual members of the class. The law remains linked to the relations of production directly through the political perception of advanced segments

of the ruling class and indirectly through the political principles that are ultimately rooted in those relations."

But Robert Gordon (1984, 101) breaks more fully with Marxism, as indicated by his influential essay, "Critical Legal Histories," which attacks both Marxist and Parsonian forms of "evolutionary functionalism" in legal history precisely as contemporary political scientists and sociologists attack its counterparts in their disciplines. Gordon insists that "causal relations between changes in legal and social forms" are "radically underdetermined." Legal forms and practices therefore cannot be adequately grasped as "objective" responses to "objective" historical processes, not even evolving relations of production. Law is indeed a "product of political conflict," but it is not simply a mirror or reflex of that conflict. Again, legal ideologies are "relatively autonomous" structures with their own peculiar internal character, so that they sometimes act as "independent variables" that transcend and actually help "shape the content of the immediate self-interest of social groups." Judges may decide in part out of concern to mitigate internal tensions in legal doctrines; parties to a case may be influenced by what the law suggests their legitimate claims are. Partly as a result, there can be no confident expectation that the decisions of legal actors or institutions will always be "functional" for their material interests, or even for their own survival: the quest for ideological consistency can lead to behavior that is counterproductive by these standards. Thus these legal and sociological critics of functionalist accounts arrive, like March and Olsen, at a belief in the importance of various relatively enduring political and intellectual institutions in human life beyond economic relations and social "systems"; but they do so as much to break free of rigid historical determinisms as to identify overlooked constraints on political choices.

### **The Lessons for Public Law: An Appraisal of the "New Institutionalism"**

#### **Strengths**

While not every protest against dominant perspectives deserves to be sustained, it is not likely that many scholars would feel obliged to move in the same direction unless there were something to their feelings. I believe the basic critiques advanced by those on each road to the new institutionalism are correct, whether or not they really point toward that destination. Those reacting to individual and group behavioralism and rational-choice theory are right to insist that analyses of politics should explore how relatively enduring structures of human conduct have shaped the existing array of resources, rules, and values, instead of simply taking that array as given.

As just suggested, this point is strikingly supported by the study of public law—for controversial, politically charged judicial decisions in the past may later determine the types of litigants that can get *into* court at all, as well as the very types of claims or rights persons believe they are entitled to assert, morally as well as legally (Orren 1976; Gordon 1984, 109–13). Obviously, no group is likely greatly to influence an institution that will not attend to its voice. And many groups' sense of their nature and purposes may be significantly affected by how far the legal system legitimates, for example, the permissibility of religious dissent, or of unregulated production and exchange. Thus, accounts of self-interested rational calculations and the behavioral regularities they are thought to generate will have limited explanatory power if they are not sensitive to how the legacies of past decisions lead people to think their interests should be so defined. Neglect of these factors may also prevent us from seeing how social definitions of interests appear much

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better tailored to enrich the normative debates in the field, and those debates might be more attentive to the problems of the empirical generalizations they rely upon tacitly. To see how, we must identify further the recent philosophic developments that suggest the relevance of "new institutionalist" descriptions of the legal system.

### The New Institutionalism and Normative Debates

After a period in which rather abstract and hypothetically oriented moral and political theory was in vogue, many philosophers have recently been urging the adoption of more concrete, particularistic, and historically infused modes of moral and political argument. Writers who describe themselves as subscribing to quite different substantive outlooks, including neo-Aristotelianism, Enlightenment liberalism, and democratic socialism, have agreed with Alasdair MacIntyre (1981, 119) that "morality is always to some degree tied to the socially local and particular." Therefore, as Richard Rorty (1986, 13) argues, following John Dewey, moral philosophy is properly concerned with the "precious values embedded in social traditions," and with the "conflict of inherited institutions with incompatible contemporary tendencies." Consequently, we are now frequently urged to begin not with states of nature, past, future, or hypothetical, but "with the values we hold and the political world we inhabit," examining the record of the diverse inherited values in our past and present social existences in order to deliberate on what prospective courses seem more or less promising (Herzog 1985, 225).

This recent turn (or return) in moral and political philosophy, which may be called a move away from "ideal theory" to more empirically oriented "prag-

matist" approaches, seems to me largely sound. It does raise legitimate fears that we will be too wedded to past values or else inclined to doubt our values if we see them as merely our own. Pragmatism must indeed prove unsatisfactory if we use it to conceal from ourselves the necessity to criticize the current array of normative dispositions and to make reasoned choices among them. Those choices may have to be defended in turn by appeals to what seem to us to be more lasting aspects of our condition, in the manner of natural-law theorists.

Yet it simply seems true that our moral outlooks are largely the products of past traditions, whether we are conscious of that fact or not. So we are more likely to be liberated from the inadequacies of our inherited principles if we recognize their historical roots and attend carefully to the role—great or small—they have played politically, attempting to judge their characteristic tendencies, strengths, and weaknesses in the crucible of social life. Furthermore, recognition that our values have been carried to us via particular traditions does not preclude faith in their transcendental validity, much less their propriety for us. And if it makes us somewhat more dubious about making universalistic claims, generally that may be to the good.

It is fair to say, however, that so far the call to begin with the empirical and historical realities of our moral traditions remains more a program than an achievement. While Rorty, MacIntyre, Walzer and others have offered useful historical analyses of certain grand philosophic traditions, few of these writers have paid any detailed attention to the other varieties of political and moral discourse that have played such a prominent part in shaping our current societies and selves. Clearly, if the "new institutionalism" is understood in the manner proposed here, as encompassing among other things descriptions of the influence of enduring



structures of legal ideas, it is well suited to help fill this gap. Research that identifies actual patterns in legal and political discourse and their consequences, testing their significance versus that of other structural contexts, should enable public-law scholars to argue more powerfully about the values U.S. law has really embodied historically, about the ways those values have shaped, and been shaped by, political conflicts, and about the results they have furthered or forestalled.

Those studies would be of great relevance for many contemporary normative debates, such as, for example, whether more "republican" or "liberal" conceptions of civic life have been dominant in U.S. public acts in particular eras, and whether "republican" conceptions have most assisted democratic reforms or reactionary localism (see, e.g., Bellah et al. 1985). Often, I fear, theorists would have to acknowledge that the historic traditions they favor, whether "republican" or "liberal" or both, have often been relatively ineffective in furthering the ends they admire, and have even proven conducive to quite unattractive results. If so, many current normative arguments would be greatly altered by such increased historical awareness. And since I believe debates over the values that should guide the U.S. legal system in the future must always remain a prominent part of the public-law agenda, this contribution of "new institutionalist" studies of historical ideological structures is a vital one indeed.

At the same time, analysts engaged in more quantitative empirical research might benefit from an increased awareness of how their studies are relevant to ongoing normative controversies. When a quantitative scholar can see quite easily how prevalent moral disputes rest in part on empirical claims about the historical operations of the legal system, he or she is more likely to choose to research topics

that speak sharply to those disputes. For example, much of the current discussion of "law and economics" takes place on an abstract normative level. Scholars ask, Is it desirable to think of the legal system's goals in this way? But there are attendant empirical questions. For instance, Is it adequately realistic, or historically plausible? To convince us that it is, Posner, among others, has advanced strong claims that much of nineteenth-century common law is explicable in terms of economic reasoning (Posner 1977, 13-14, 18; Posner 1981, 5, 106, 114). Those claims are susceptible to much fuller empirical and historical investigation than they have heretofore received from either the advocates or critics of this viewpoint. (Even when adherents of this school do not claim that economic reasoning was consciously employed, they generally assert that their models capture the "deep structure" of common-law decision making, a claim that is empirical on its face and that is in any case dependent on arguments about what actually was economically efficient in the historical periods in question.) The results of such investigations are likely to affect the credibility of such economic reasoning as a normative model for contemporary jurisprudence. More generally, a public-law subfield that explicitly took part of its empirical agenda from normative debates and that produced descriptions relevant to current normative claims would go far toward advancing the unity of descriptive and prescriptive concerns that some participants in the 1984 symposium found lacking.

All this may be too optimistic. At present we can be more confident of the critiques of current efforts than of the potential of the suggestions offered here. But I would insist that at a minimum, these recent trends mean first, that empirical investigators should agree that the impact of structures of ideas forms a part of their enterprise; second, that qualitative analysts of the history of legal ideas and

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normative advocates should recognize that their endeavors will seem too abstract if they do not study carefully the role that the values they appeal to have played in the actual judicial decisions that constitute so much of our legal traditions; and, I would hope, third, that both quantitative and qualitative public-law scholars might consider exploring further how we might realize this suggested recasting of their work in terms of a common focus on the interplay of specified structures and decisions. If the methods and conceptions of the "new institutionalism" can help promote such shared awareness among the diverse scholars at work in the field, these recent developments will indeed prove to have both importance and promise for students of public law, as well as for political scientists in general.

### Note

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# INTEREST GROUP POLITICS AND ECONOMIC GROWTH IN THE U.S. STATES

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**M**ancur Olson's model of economic growth has attracted great attention as a theoretical account of how interest groups influence the rate of economic growth over time. Moreover, the model appears to have received strong empirical support in Olson's tests employing U.S. state data. However, the specification of the Olson construct in these tests is insufficiently attentive to the complex causal chain implicit in Olson's argument, inadequately accounts for precisely how interest groups matter, and employs a static research design that obfuscates the cause-and-effect relationships posited by the model. We review these issues and develop a more complete specification of the Olson model. The respecified model is then tested using U.S. state data for the period of the late 1970s and early 1980s using new measures of interest-group influence.

**M**ancur Olson has provided a compelling and broad ranging theoretical account of the manner in which interest groups influence economic growth rates. Building on his analysis of the mechanics of interest-group organization in *The Logic of Collective Action* (1965), he suggests in *The Rise and Decline of Nations* (1982) that over time, the number and influence of groups interested in retarding economic growth accumulate and are incrementally successful in having antigrowth policies adopted and implemented. Unfortunately, one of the long-term consequences of such policy adoptions is a decline in economic growth rates as nations move temporally further from the periods of invasion or domestic upheaval that serve to upset patterns of interest-group organization and influence. The Olson construct has, not surprisingly, attracted a great deal of theoretical comment (Mueller 1983; Willis 1986) and has been the subject of some degree of empir-

ical evaluation (Brace 1985; Brace and Dudley 1985; Choi 1979; Dye 1980). Olson (1982) himself tested his model on both cross-national and U.S. state data in a truly comparative study. In this paper, we confine our attention to the Olson model as applied to the 50 U.S. states.

While much of the U.S. empirical evidence supports Olson's model, it remains suspect for several reasons. First, the specification of the Olson construct upon which this evidence is based is at best incomplete; little attention was given by Olson to specifying the complex set of relationships that lie between the initial observation of time since invasion or upheaval and changes in the rate of economic growth. Second, the specification of the precise manner in which interest-group power is influenced by the passage of time and, in turn, influences policies affecting growth rates is ignored in Olson's tests of the construct. Instead, Olson and Choi simply examined time

and growth rates, something that is surprising given that the model is a theory *about* interest groups. And third, most tests based on such inadequate specifications of the Olson construct have examined time periods that are too long to allow for valid inferences about cause and effect and only address indirectly "changes" in the concepts identified in the model. In short, most existing tests of the Olson construct are inadequate. Recent research by Brace and his associates (Brace 1985; Brace and Bauman 1986; Brace and Dudley 1985) is the chief exception.

We develop and test a more complete specification of the Olson model. The model is reviewed in the first section, where we develop a series of equations that are more consistent with the complex set of relationships posited by Olson. The model is then partially tested in the second section with U.S. state data for the period of the late 1970s and early 1980s. The test results are discussed in the final section.

### Evaluating the Theoretical and Empirical Foundations of the Olson Model

Extant theoretical specifications and empirical evaluations of the Olson hypothesis are characterized by at least three major problems. These problems concern, respectively, (1) the theoretical specification of the causal chain underlying the model, (2) the specification of just how interest groups matter, and (3) the appropriate research design to assess validly the cause-and-effect relationships posited by the construct.

#### The Problem of Specifying the Causal Chain

Olson's hypothesis appears deceptively simple. As Olson (1982, 77) has stated it,

"The logic of the argument implies that countries [or states] that have had democratic freedom of organization without upheaval or invasion the longest will suffer the most from growth-repressing organizations and combinations." In this form, the model has only two variables: time since invasion or upheaval and the rate of economic growth. And this is certainly the form of the argument that has received the most empirical scrutiny. The empirical tests of Olson (1982, 94-117), Dye (1980, 1099-1100), and Choi (1979) all use U.S. state data on the relationship between time since admission to the Union (or since the Civil War for southern states) and economic growth rates. In this usual form, then, Olson's model is tested with the following equation:

$$\text{GROWTH}_i = a_1 + b_1 \text{TIME}_i + e_i \quad (1)$$

where GROWTH is the economic growth rate and TIME is time since major domestic upheaval or war. Moreover, these tests have generally provided very strong support for this specification of the Olson model, with the estimated coefficients being negative and significant as expected.

But while the model starts with time and ends with the hypothesized growth effects, there are several other causal steps that must necessarily come between, linkages that remain untested with the specification presented above. These untested linkages between the two variables cited in Olson and Choi's presentations and tests are at least implicit, and in several cases quite explicit, in more complete presentations of the model. Specifically, four steps are implied in the hypothesized relationship between time and growth.

First, length of time since major upheaval or invasion is hypothesized to be associated with growth in the "number" of groups organized to pursue particularized benefits for their membership. As we will see later, time may also be related to other characteristics of interest-group

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influence. And while most of the empirical tests of the Olson model have focused on the more indirect relationship between time and growth rates, there has been some attention to this more specific hypothesis in evidence linking the "size" of the unionized population to time since statehood (Dye 1980, 1103-4; Olson 1982, 106-7). However, this evidence is at best indirect since the Olson hypothesis speaks to *number* of groups in the political system, while the test addresses the membership *size* of one kind of group. Moreover, even this relatively limited effort to assess a key linkage of the model empirically is missing when the other linkages are considered.

The *second* of the missing linkages can be expressed in the hypothesis that the number of interest groups is associated with the adoption of policies designed to minimize the vulnerability of the groups' members to the consequences of market competition. Such actions usually involve the use of government for private economic benefit, either through passive acquiescence by government to private collusive arrangements or through more active regulations to restrict entry, fix wages and prices, and generally minimize the burden of competition. This link in the Olson construct is the one that is most clearly political, addressing both the kinds of policies groups will pursue as well as their ability to realize such goals via influencing governmental decision makers.

But while the logic of this relationship is fully developed in the economic literature on regulation (e.g., Pelzman 1975; Stigler 1971), there has not been enough effort to confront the hypothesis with data. Indeed, in Olson's presentation of the model, these effects are merely listed (1982, 44-45), with little effort to link their adoption and use to the growing number of organized groups. In one of the few studies to assess the relationship between interest-group strength and such

policy adoptions directly, Ambrosius (1985, 12) did find evidence that states with "strong interest groups are more likely to attempt to ensure or improve their economic well-being by adopting economic development policies." But even this evidence must be viewed as tentative, given the scope of the interest-group strength measure used in the study; Ambrosius's measure included a large number of groups, many of which would seem at first glance to have little interest in such policies. In an earlier study, Dye (1980) found that aside from highway spending, no other policy variables independently affected growth rates. Growth in the number of groups must be linked to the adoption of specific policies or practices if Olson's model is to be validated.

*Third*, the Olson model implies that adoption of these policies is associated with changes in the patterns of economic behavior associated with investment, innovation, and adaptation. That is, not only must it be shown that the number of interest groups is associated with the adoption of particular policies, it must be shown that these policies influence the way economic actors conduct their activities. Quite simply, economic growth rates are merely the outcome of a complex set of economic behaviors. The success of this policy-behavior link is fully implied by the Olson model. But while all the evidence is far from in, it has long been argued that the kinds of policies adopted by states under the heading of industrial policy probably have little impact on growth and may in fact be detrimental over the long run (Ambrosius 1986; Barker 1983; Hansen 1984, 1986). The Olson model, however, only assumes that this is the case.

*Fourth*, changes in these economic behaviors are hypothesized to be associated with changes in the ultimate dependent variable: the rate of economic growth. That is, it is these changes in investment,



innovation, and adaptation behaviors presumably induced by the adoption of specific policies that eventually influence the rate of economic growth.

Thus, far from being the simple relationship specified in Equation 1, Olson's model is more correctly specified in the following manner:

$$\text{INTERESTS}_i = a_2 + b_2\text{TIME}_i + e_i \quad (2)$$

$$\text{POLICIES}_i = a_3 + b_3\text{INTERESTS}_i + e_i \quad (3)$$

$$\text{BEHAVIORS}_i = a_4 + b_4\text{POLICIES}_i + e_i \quad (4)$$

$$\text{GROWTH}_i = a_5 + b_5\text{BEHAVIORS}_i + e_i, \quad (5)$$

where in addition to TIME and GROWTH, INTERESTS is the number of groups concerned with particularized benefits, POLICIES is the level of reliance on policies designed to protect group members from the effects of economic competition, and BEHAVIORS is the level of particular kinds of economic behaviors hypothesized to influence the rate of economic growth. All of the coefficients but  $b_5$  are expected to be positive.

Importantly, these effects are interdependent. That is, rejection of any one of these linkages would seriously weaken the chain of logic upon which Olson's construct rests. While we cannot say that failure to generate support for any one of the equations is entirely sufficient to invalidate the construct as a whole, the path outlined by this model is probably the most direct and plausible in an era when government is deeply involved in private economic relationships. Thus, if any one of the links is rejected, the plausibility of the model as a whole must decline. For instance, if little support is found for Equation 3, it must then be shown that institutional sclerosis is entirely generated outside the governmental-policy process via private labor-management accommodations. Such a path would seem unlikely. Thus, failure to sup-

port Equation 3 would seriously weaken the construct as a whole even if empirical support for Equations 2, 4, and 5 were found; simple relationships posited by those equations might still tell us something useful about time, interest groups, and economic growth; but they would not add up to the Olson construct.

While a test of this more complete specification is essential to evaluate the model fully, we have seen that most research attention has focused on the simple relationship between time and economic growth. This approach ignores the important interest-group linkages in Equations 2 and 3, something that is surprising given that the Olson model is preeminently a theory about interest groups. Yet only Brace (1985) addresses interest groups directly, using the Morehouse index of pressure-group strength. Otherwise, the only indirect evidence on the U.S. states consists of Choi 1979, Dye 1980, and Olson's (1982) findings on the growth of unionization over time, an effect that may or may not be related to the hypothesized increase in the number of groups.

### The Problem of Specifying the Interest-Group Effect

The second problem with the Olson construct concerns the precise way in which interest groups are expected both to be influenced by the passage of time and to determine the adoption of specific policies. As presented by Olson, the model addresses only the *number* of interest groups; and even this dimension is often inappropriately measured as the *size* of a single interest group's—the unions'—membership. Yet, Olson's construct has much more to say about the nature of interest groups than simply the number operative at any point in time. What characteristics of groups, then, should the model be attentive to? And more importantly, how are each of these interest-

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group effects related to both the passage of time (Equation 2) and the adoption of policies designed to protect the members of the groups from the forces of economic competition (Equation 3)?

Olson's presentation of the model points to three characteristics of interest groups that are hypothesized to be associated ultimately with economic growth. The *first*, as noted above, is the simple number of organized interest groups in the society. As Olson (1982, 17) states it, "Developed democratic societies will accumulate more special-interest organizations as time goes by." As we have seen, this characteristic of interest groups has received the most empirical attention even if it has been poorly measured.

Importantly, Olson's analysis addresses at least two other factors that have received little attention in empirical evaluations of the model. The *second* factor is what has been called the *encompassingness* of the interest groups. Encompassing groups—those that embrace a large proportion of the members of the society—are argued to be more willing than less encompassing groups to pursue policies associated with enhancing competition so as to promote economic efficiency. For such groups, the pursuit of narrow protectionist policies would hurt more members than it might benefit (Olson 1983, 22–23).

*Third*, Olson suggests that beyond number and encompassingness, the strength of interests vis-à-vis the government is a major characteristic defining the interest group's ability to effect change for its benefit. "The argument here accordingly focuses," Olson (1983, 23) writes, "not only on the absolute power of special-interest groups but also on the *relative* strength of the most encompassing group, the government, versus constituent groups that are much less encompassing." Thus, the characteristics of interest groups alone are not enough to describe interest-group power fully; that

power must be defined in relative terms, taking account of governmental power.

This aspect of Olson's model has been most fully developed by Whiteley (1983; see also Lange and Garrett 1985), who has suggested that "a hidden implication of Olson's model which he fails to bring out, is that the government is the only agency in society with an interest in eliminating retardants to growth." Moreover, Whiteley argues that governmental power should be interpreted in terms of control by parties of the Left. He notes (1983) that "the left is much more likely than the right to intervene in the management of the economy to offset the tendency towards stagnation in advanced capitalism." Whiteley finds some support for this hypothesis in tests using cross-national data. However, Whiteley examines only patterns of party control, not the *relative* power of interests so central to Olson's interpretation.

While these three characteristics are those cited by Olson as essential elements of interest-group power, some have argued that at least one other element must be considered in fully specifying the model. Specifically, Dean (1983) has suggested that the *relative* power of different interest groups must be considered. Noting that Olson and his students largely ignore the role of business interest groups, Dean points to at least two kinds of conflict between groups that can serve either to restrict or to enhance the hypothesized interest-group effect on growth: labor-business conflicts and progrowth business-antigrowth business conflicts. As Dean (1983, 254) notes, "Independently of fragmentation, interest groups should be classified according to the nature of their goals and activities . . . and their relative political strengths. The balance of power in this political dynamic varies crucially between countries and over time; in fact it is, I would venture, the critical element in the growth process." Thus, it is not enough to know the number of groups.



We need to be attentive to interest-group goals on economic growth and their relative power.

To this point, nothing has been said about the *size* of interest groups, the measure of interest-group influence that has actually been used in existing tests of the Olson construct. As we have seen, group size is an inappropriate indicator of the concept it was purportedly measuring—the number of groups. There is simply no necessary relationship between group number and group size. However, this does not mean that group size is an unimportant dimension of interest-group influence. Although Olson's model says little or nothing about group size, it would seem plausible to expect that larger groups would be more politically influential than smaller groups, all other things being equal. Thus, group size is posited as a *fifth* dimension of interest-group influence.

We have, then, five characteristics of interest groups, each of which must be linked to the larger model presented in Equations 2–5. Starting with Equation 2, how should each of these characteristics be related to passage of time since invasion or domestic upheaval? Unfortunately, the answer is clear for only the first of the interest-group characteristics—the number of groups. As we have seen, Olson provides a compelling account of how the number of groups accumulates over time as successive collections of interests overcome the costs of organization. Thus, we specify Equation 6:

$$\text{NUMBER}_i = a_{21} + b_{21}\text{TIME}_i + e_i, \quad (6)$$

where NUMBER is the number of groups and  $b_{21}$  is expected to be positive.

The second characteristic is the encompassingness of groups. But it is not at all clear how the passage of time should influence this characteristic of interest-group organization. Olson's account (1982, 91) of encompassingness addresses

only cross-sectional determinants of this trait, arguing that "small and relatively homogeneous societies obviously would be more likely to have organizations that are relatively encompassing in relation to the society than would large and diverse societies." Indeed, Olson (1983, 91) explicitly cautions against expecting any relationship between time and encompassingness, noting that "leaders of special interest organizations . . . cannot get any of the gains that might result from the mergers that could create a more encompassing organization by 'selling' their organization; a merger is indeed likely to result in the elimination or demotion of some of the relevant leaders. There is, accordingly, no inexorable tendency for encompassing organizations to replace narrow ones." Brace (1985), on the other hand, has argued that encompassingness will increase over time; in particular, parties and legislatures will take on the role of encompassing groups. Thus, any expectations about the relationship between this aspect of interest-group influence and time must remain open. Still, we can test the following equation as an analogue of Equation 6:

$$\text{NONENCOM}_i = a_{22} + b_{22}\text{TIME}_i + e_i, \quad (7)$$

where NONENCOM is the degree to which interest groups are nonencompassing. (Encompassingness is inversely measured so as to facilitate its inclusion in a more complex measure of interest-group power.) However, we have no precise expectation of the sign of the coefficient  $b_{22}$ .

Similarly, we have no initial expectations about the third and fourth characteristics of interest groups—the power of interest groups *relative* to the government and to each other. Olson's discussion of the former again stresses cross-sectional variation in the power of interest groups relative to the government, not temporal factors. And as Dean has suggested,

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Olson says little or nothing about the relative power of different groups. Still, it would seem that some sort of temporal development would be consistent with Olson's larger vision of the mechanics of group organization. That is, as groups interested in growth-retarding policies are successful, the potential benefits to be derived from organization by those harmed by these policies would increase. This increase in the potential rewards of organization should help in overcoming some of the costs of organization. Thus, we might expect to see the growth of interest power to lead to the growth of countervailing power, whether through government or private interests. Thus, similar to Equation 7, we have

$$\text{RELGOV}_i = a_{23} + b_{23}\text{TIME}_i + e_i \quad (8)$$

$$\text{RELINT}_i = a_{24} + b_{24}\text{TIME}_i + e_i, \quad (9)$$

where RELGOV is the power of interest groups relative to government and RELINT is the power of interest groups favoring protection from the rigors of competition relative to other interest groups less supportive of those actions. While not explicit in Olson's presentation of this model, we would expect the coefficients  $b_{23}$  and  $b_{24}$  to be negative in sign over the recent short term. While interest-group power and influence would have to increase before the incentives of countervailing groups would be sufficient to overcome the costs of organization, the realization of interest-group power of the type suggested by Olson would increase those incentives.

The analysis of the impact of time since invasion or domestic upheaval on group size is similar to that of the two relative power terms in the model. That is, Olson's model addresses how groups overcome the costs of initial organization, not how they grow over time. Still, the logic of his analysis would seem to suggest that groups have some incentive to grow as individual interest groups if not

through merger into more encompassing groups. In contrast to the possible lack of incentives of interest-group leaders to merge their groups with others to form more encompassing groups, the benefits accrued from growth in the size of a single group could be realized by the leaders of the group and would seem to provide a powerful motive for interest-group expansion. This relationship can be expressed in the equation

$$\text{SIZE}_i = a_{25} + b_{25}\text{TIME}_i + e_i, \quad (10)$$

where SIZE is the average size of interest-group memberships. While not explicit in Olson's presentation, we would expect  $b_{25}$  to be positive.

If Equations 6 through 10 provide for a more complete account of the impact of the passage of time on characteristics of interest-group influence, what about the impact of those influence characteristics on the adoption of policies designed to shelter groups from the crosscurrents of unregulated competition—Equation 3? We would expect all five influence traits to be associated with such policy adoptions, but not in a simple additive manner. That is, we expect that several of the dimensions of interest-group influence would combine to generate some level of political pressure for the adoption of growth-retarding policies in the political process.

Starting with the groups supportive of such policies, we would expect their absolute influence—POWER—to be reflected in the interaction of group SIZE and NUMBER. Both the number and size of groups are traits that would be expected to translate into political influence. There are, of course, other resources of groups that would also have a direct bearing on their level of political influence, including, especially, campaign contributions. Maitland (1985) has provided an interesting analysis of Political Action Committee (PAC) contributions in the context of

Olson's model at the national level. We were not able, however, to develop measures of such resources at the state level. Therefore, our measure is limited to the influence effects of the size and number of groups. Schlozman and Tierney (1986) have argued that the number of separate groups is particularly important in measuring the influence of business interests, a degree of influence that is missed when membership size alone is considered. We start our specification of Equation 11, then, by initially defining *interest-group influence* as the term  $[(\text{ANUMBER}_i)(\text{ASIZE}_i)]$ , where ANUMBER is the number of antigrowth interest groups and ASIZE is the size of the antigrowth interest groups.

However, the degree to which these groups are free to pursue antigrowth policies is also influenced by the level of group encompassingness. That is, encompassing groups, because they are less homogeneous in membership, must balance the needs of all of their members and are, therefore, less free to pursue more narrow antigrowth policies. This constraint on the ability or willingness (or both) of the antigrowth groups to pursue antigrowth policies can be included as an additional interaction term so that the final measure of the level of political pressure for antigrowth policies (POWER) can be expressed by the term  $[(\text{NONENCOM}_i)(\text{ANUMBER}_i)(\text{ASIZE}_i)]$ , where NONENCOM is the degree of nonencompassingness of the antigrowth groups. High nonencompassingness would serve to enhance the level of pressure for antigrowth policies beyond that determined by group size and group number alone.

The fourth dimension of influence to be addressed is the relative balance of power between groups supportive of and groups opposed to antigrowth policies—RELINT in Equation 9. The absolute influence of progrowth groups, like that of the antigrowth groups, would be reflected in an interaction of group size and number. Therefore, we can more precisely measure

RELINT—the relative influence of progrowth and antigrowth interest groups—with the ratio term  $[(\text{NONENCOM}_i)(\text{ANUMBER}_i)(\text{ASIZE}_i)]/[(\text{PNUMBER}_i)(\text{PSIZE}_i)]$ , where PNUMBER and PSIZE are the number and size, respectively, of the progrowth interest groups.

The final factor is the power of the antigrowth groups relative to the government, RELGOV in Equation 8, which is included in the model in a manner similar to the relative interest-group-power term. That is, following Whiteley and measuring governmental strength in terms of control by the party of the Left, the relative power of antigrowth interest groups vis-à-vis the government as the largest encompassing group can be measured by the ratio variable  $[(\text{NONENCOM}_i)(\text{ANUMBER}_i)(\text{ASIZE}_i)]/[\text{LEFT}_i]$ , where LEFT is the level of control of government by leftist parties.

Thus, we can reformulate the model of antigrowth policy adoption— $\text{POLICIES}_i = a_3 + b_3 \text{INTERESTS}_i + e_i$ —in the following manner:

$$\begin{aligned} \text{POLICIES}_i = & a_3 + b_{31}[(\text{NONENCOM}_i) \\ & (\text{ANUMBER}_i)(\text{ASIZE}_i)] \\ & + b_{32}[(\text{NONENCOM}_i)(\text{ANUMBER}_i) \\ & (\text{ASIZE}_i)]/[(\text{PNUMBER}_i)(\text{PSIZE}_i)] \\ & + b_{33}[(\text{NONENCOM}_i)(\text{ANUMBER}_i) \\ & (\text{ASIZE}_i)]/[\text{LEFT}_i] + e_i, \end{aligned} \quad (11)$$

where the three independent variables measure, respectively, the absolute level of pressure for antigrowth policies (POWER), the degree of that pressure relative to the countervailing pressures of progrowth interests (RELINT), and that relative to the countervailing pressures of government (RELGOV). We would expect  $b_{31}$ ,  $b_{32}$ , and  $b_{33}$  to be positive.

Importantly, though, the likelihood of adopting antigrowth policies does not have a simple relationship to the passage of time (since statehood, domestic up-

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heaval, or invasion), as hypothesized by Olson. As we have seen, time since destruction of accumulated interest groups supportive of antigrowth policies is differentially related to the several variables in Equation 11, with time hypothesized to be positively related to ASIZE and ANUMBER, negatively related to RELGOV, PGROWTH, and PSIZE, and ambiguously related to NONENCOM. Surprisingly, then, the indirect effect of time on the adoption of policies could be the opposite of that suggested by Olson if its main effects are realized through the relative power terms in the equation or if time leads to the development of more encompassing groups.

In sum, this more complete specification of the way in which interest groups are expected to matter both suggests that existing empirical tests are very incomplete and raises serious questions about the kinds of expectations the theory should generate about policy adoptions.

### **The Problem of Testing the Model Empirically**

While the two previous problems concern the theoretical specification of the Olson construct, the remaining problem set concerns the appropriate measurement of the variables in the model and the testing strategies used to evaluate it. At least two major problems of this type can be identified.

*First*, the previous tests of the Olson construct employ an excessively long period of time, and thus weaken any causal link that one could draw between time and policy adoptions inhibiting growth. As Pryor (1983, 91) has noted, "Linking relative growth patterns between 1965 and 1974 to events occurring more than a century before, when industrialization was not far advanced in the states, is a dangerous procedure unless one can obtain evidence that intervening events did not greatly influence current growth

patterns." Therefore, it is essential that tests of at least some portions of the model—especially Equations 4, 5, and 11—be restricted to more limited periods of time and focus on specific adoption patterns of growth-related policies in order that a more rigorous interpretation of temporal cause and effect relationships might be made. Moreover, at least one study (Brace and Dudley 1985) has shown that the Olson hypothesis holds up only for states admitted prior to the Civil War. For post-Civil War states, the relationship between time and growth disappears.

*Second*, the Olson construct has been tested to date largely with simple cross-sectional research designs of the type criticized by Gray (1976). Yet, the model is a theory about change. Change in interest-group power is expected to lead to changes in policy. These changes in policy are then expected to be associated with changes in economic behaviors. Finally, such changes in economic behaviors are expected to be related to changes in economic growth rates. This dynamic is lost in tests employing static measures of interest-group membership. Thus, in developing linkages between the several variables cited in the construct, the use of change measures would seem to be essential.

### **Testing the Olson Construct**

#### **Dynamic Models of Olson's Hypothesis**

The last two considerations suggest that Equations 4, 5, and 11 be reformulated as change-score equations with the dependent variables measuring, respectively, changes in policies, economic behaviors, and growth rates over relatively short periods of time to allow for more precise determination of the cause-and-effect relationships posited in the Olson model. The absolute level of the dependent variables at the beginning of the period

should also be included as independent variables to control for the effects of other long-term forces influencing growth, such as those cited by Pryor (1983, 91). The independent variables of interest should then be included as change scores to capture the short-term forces governing the several processes in question.

However, while we believe that such a revision of the four-equation specification of the Olson construct presented earlier is the most theoretically valid rendering of the model, we test a somewhat simpler specification for two reasons. First, the distinguishing characteristic of the Olson explanation of growth rates is its emphasis on the role of interest groups. Therefore, we focus on that characteristic. Second, while support for all four equations is necessary to validate the Olson model, the requirements of falsification are much less rigorous. That is, if we do not find that the interest group variables are related to economic growth rates in the hypothesized manner, one of the linkages between interest groups and growth rates will be seriously weakened. Given the results, this simpler strategy is appropriate. Thus, our respecified model collapses Equations 4, 5, and 11 such that

$$\begin{aligned} D(\text{GROWTH}_i) = & a_3 + b_{31}\text{LAGGROWTH}_i \\ & + b_{32}D[(\text{NONENCOM}_i)(\text{ANUMBER}_i) \\ & (\text{ASIZE}_i)] + b_{33}D[(\text{NONENCOM}_i) \\ & (\text{ANUMBER}_i)(\text{ASIZE}_i)]/[(\text{PNUMBER}_i) \\ & (\text{PSIZE}_i)] + b_{34}D[(\text{NONENCOM}_i) \\ & (\text{ANUMBER}_i)(\text{ASIZE}_i)]/[\text{GOVPOWER}_i] \\ & + e_i. \end{aligned} \quad (12)$$

Lagged values of the dependent variables have been introduced into the equation to account for its level at the beginning of a more narrow period of time and all of the substantive independent variables are included as change scores, as indicated by the D prefix.

### Data and Time Period

We have argued, as has Pryor (1983, 91), that the Olson construct should be tested using more discrete time periods. Therefore, the growth-rate variables are measured for the period 1977–82, while the interest-group variables are measured for the period 1975–80. While these more discrete periods and lags differ considerably from those used by Olson and Choi, they are identical to those used by Dye (1980) in his assessment of the Olson model, although his data were from the early 1970s. And, importantly, Dye found substantial support for the Olson model using such time periods and lag structures. Also, Lange and Garrett (1985) have found support for the Olson model in cross-national analyses using growth-rate measures covering a six-year period. Thus, if less supportive results are found here, it should not be due to the use of our specific period and lag structures.<sup>4</sup>

Complete data were not available for all states for all variables. Specifically, data on one or more of the variables is missing for Alabama, Alaska, Hawaii, Iowa, Nebraska, Nevada, Rhode Island, Utah, and West Virginia. Therefore, only 41 cases are employed in the tests to follow.

### Measures of the Dependent Variable

As reported in the Appendix, the indicators of the ultimate dependent variable—GROWTH—include two of the measures used by Olson (1983) and Choi (1979): LPI, the exponential rate of growth of income of labor and proprietors from 1977 to 1982; and NPI, the exponential rate of growth of private nonfarm income from 1977 to 1982. Additionally, we employ MPI, the exponential rate of growth of manufactures income from 1977 to 1982, as an additional measure of growth, given the important role of the manufacturing sector. Olson and Choi also tested

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## Economic Growth in the States

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per capita measures of these three growth variables. Accordingly, we also test the growth models using PCLPI, PCNPI, and PCMPI.

### Measures of the Independent Variables

The measure of time since major domestic upheaval or war used by Olson and Choi—the number of years since statehood in the case of nonsouthern states and the number of years since 1865 in the case of states in the Confederacy—is used here in all of the tests to follow.

The interest-group measures are more complex for two reasons. First, given Dean's (1983, 254) comments reported earlier about which groups might be expected to be supportive of growth-retarding policies, we examine two separate sets of interests. The first is unions, the group addressed most extensively in Olson's analysis. Second, the role of business interest groups are examined. As Whiteley (1983) has suggested, "When discussing these groups [Olson] focuses almost exclusively on trade unions and the farm lobby. However, any commercial organization has an incentive to achieve monopolistic control of its market or suppliers if it can, and collude with other organizations in order to achieve monopoly profits. Private corporations are just as likely to act in restraint of competition, and hence to retard economic growth if they get the chance."

Second, the interest-group measures are complex because of the need to examine both static and change-score indicators. Olson employed only the former, to the extent that he examined interest groups at all. We have argued that change scores should also be employed to assess the model more validly. Therefore, tests using both are employed.

The first of the interest-group variables is group NUMBER. The number of union and business interest groups in 1975 were

obtained from Marquis Academic Media (1978). The number in 1980 were collected by Conover and Gray (1983) from lists of organizations that employ registered lobbyists in state legislatures.<sup>2</sup> These indicators thus have the advantage of measuring the number of interest groups where they come into contact with the political system, something that should be useful in evaluating the impact of such groups on the adoption of growth-restrictive public policies. They have the additional advantage of being objective measures rather than reputational measures of influence in state legislatures. Determination of the number of union (UNUMBER) and business (BNUMBER) organizations employing registered lobbyists from such lists was straightforward. Sets of indicators for both groups were developed for 1975, 1980, and the change from 1975 to 1980.<sup>3</sup>

Group SIZE was most easily measured for unions, with USIZE indicated by the percentage of the labor force that is unionized. Comparable measures for business groups were not available. Therefore, the size of business (BSIZE) interest groups were measured by the percentage of the state's labor force employed in manufacturing and services. While this indicator is hardly comprehensive, it should serve to distinguish between states with very large and very small business sectors. Again, separate size measures were developed for 1975, 1980, and the change from 1975 to 1980.

The level of nonencompassingness of unions (UNONENCOM) is measured by the ratio of the number of union organizations employing registered lobbyists to the size of the unionized sector of the labor force. A high value of the ratio would indicate that there are a relatively large number of unions relative to the number of unionized workers. If we assume that the distribution of workers across the unions is not excessively skewed, then this would indicate that

each union would represent a relatively smaller proportion of the workers or that each must represent a fairly discrete set of constituents. This, of course, would be the case of high nonencompassingness. Conversely, a low value of the ratio would indicate that the workers are represented by a small number of unions. If we assume that the states do not vary too excessively in terms of the kinds of industries they have, then this would imply that the unions are more encompassing. Obviously, both of these assumptions—on the nonskewed populations of different unions and the general distribution of different types of industries across the states—are questionable. Thus, our index probably misrepresents the nonencompassingness of unions in states with a single dominant industry, as Montana and Michigan had for many years. Still, given the growing diversification of state economies, these assumptions are less tenuous for the period examined here than they might have been for previous decades.

A measure of business-interest-group nonencompassingness (BNONENCOM) was developed through analysis of the relative number of business-firm and business-trade-association organizations employing registered lobbyists. That is, trade associations are definitionally more encompassing than individual firms. Therefore, the level of business nonencompassingness was measured by the ratio of the number of firms employing registered lobbyists to the number of trade associations employing lobbyists. Indicators of the union and business nonencompassing variables were developed for 1975, 1980, and the change from 1975 to 1980.

The multiplicative interaction POWER indicators for the two groups—UPOWER and BPOWER—are also presented in the Appendix. Again, separate indicators were developed for 1975, 1980, and the change from 1975 to 1980.

The relative-interest-group POWER indi-

cators for 1975, 1980, and the change from 1975 to 1980—RELINT—are simply the ratio of each group's POWER indicator to that of the group most likely to conflict with it. In the case of businesses and unions, each serves as the most likely reference for the other. Thus, the indicator of union power relative to that of business is URELB, the inverse of BRELU, the indicator of business power vis-à-vis unions.

Finally, several choices were available in developing an indicator of interest-group power relative to that of government: RELGOV. The appropriateness of the characteristic Whiteley viewed as most relevant for inhibiting growth-restrictive policies—control of government by liberal parties—is of doubtful utility in analyses of the U.S. states given the relatively nonideological character of state politics. Still, we initially followed Whiteley's lead by using as RELGOV indicators the ratio of the 1975 power terms—UPOWER75 and BPOWER75—to an index of liberal control of state government from 1976 through 1980.<sup>4</sup> The index ranges in value from 1 for complete liberal-party control to 0 from complete nonliberal-party control.

We also tested two other indicators of liberal-party control as the denominator of the RELGOV measures: Klingman and Lammers's (1984) index of general policy liberalism and Wright, Erikson, and McIver's (1985, n.d.) index of state public-opinion liberalism. These last two indexes were moderately correlated (.65), though neither was strongly correlated with the liberal-party control index: .08 and .06 respectively. Still, all three indicators generated very similar results when included in the models to be discussed. In the end, the Klingman and Lammers index was used because it was constructed with measures of actual policy intervention on the part of the states. As Klingman and Lammers (1984) note, their analysis of the index "provides fairly strong evidence in

## Economic Growth in the States

**Table 1. OLS Estimates of Relation between Time since Statehood or Civil War and Six Measures of Economic Growth, 1977-82**

Exponential Rate of Growth, 1977-82	Independent Variable (TIME)		Constant	R <sup>2</sup>
	<i>b</i>	Standard Error		
Labor and proprietors' income	-.032*	.012	2.27	.15
Per capita labor and proprietors' income	.007	.010	1.48	.01
Private nonfarm income	-.033**	.012	2.28	.16
Per capita nonfarm income	.008	.010	1.48	.02
Manufacturing income	-.034*	.015	2.20	.12
Per capita manufacturing income	.007	.011	1.39	.01

Note: Number of cases = 41.

\* $p < .05$ .

\*\* $p < .01$ .

support of the proposition that tendencies toward more or less eagerness in the use of state government may pervade a variety of policy areas."

They also argue, however, that such tendencies are "stable" and "enduring," characteristics that raise a potential problem in the present analysis. Specifically, if we accept their assertion that proclivities for state intervention are stable and enduring, the denominators in our indicators of interest-group power relative to state governments are temporal constants. Thus, the Klingman and Lammers' index is used as the denominator in both the 1975 and 1980 RELGOV measures.<sup>5</sup>

The various models—Olson's original model, Equations 6-10 relating time and interest-group characteristics, and Equation 12, relating economic growth to the interest-group characteristics, were estimated using ordinary least square (OLS) regression analysis. Potential collinearity problems in the estimates of Equation 12 were examined by regressing each of the independent variables on the remaining independent variables. The *R*-square values from these regressions were generally modest, falling in all but a few cases below .50.<sup>6</sup>

### Findings

*Time and Economic Growth.* The results replicating those of Olson and Choi for the 1977-82 period are presented in Table 1. As expected, the number of years since statehood or the Civil War is negatively related to all three of the *total* growth-rate measures. Beyond this similarity, however, the results diverge significantly from those presented by Olson and Choi for the 1965-76 period or by Dye for the 1972-76 period. First, the TIME coefficient is positive though not significant at the usual criterion levels in all of the regressions using per capita measures of growth as the dependent variable. Second, the *R*-square values reported for the *total* growth-measure models are much smaller than those reported by Olson, Choi, and Dye. The *R*-square values of the equations using the *total* growth-rate dependent variables fall far short of the .30-.50 range reported for the 1967-76 period or the .42-.54 range for the 1972-76 period, with none exceeding .17. And it is not at all clear which measures—the total or the per capita—are most appropriate. Olson and Choi make no special claim for either and used both. In any case, at least part of their analysis is not replicated, suggesting



that their results might be an artifact of the particular years they studied.

*Time and Interest-Group Characteristics.* Turning to the tests of the more complete specification of the Olson model, the results from regressing on TIME the various indicators of characteristics of interest-group influence, Equations 6-10 are reported in Table 2. We had hypothesized that TIME would be positively related to the number of interest groups. As seen in the results presented at the top of the table, only very weak support was provided for this hypothesis. While the coefficient is positive in some cases, the sign of the TIME coefficient in the tests using the 1975 measures of NUMBER are often different from those reported for tests employing the 1980 or change-score measures (or both) of NUMBER. Moreover, the reported *R*-square values are very modest, suggesting that whatever relationship there is is very weak.

Slightly more supportive results were found for the SIZE variable. As seen in the next set of results presented in Table 2, the size of the unionized portion of the labor force in both 1975 and 1980 was positively related to time since the Civil War or statehood as expected, and both coefficients were discernibly different from zero. However, the change in the size of the unionized labor force from 1975 to 1980 was negatively related to TIME, though this coefficient was not significant at the .10 level. For the size of the business-interest-sector measures, the TIME coefficient was negative for 1975 but not significant. Both of the other business-model TIME coefficients were positive as expected, and the 1980 TIME coefficient was significant. Again, though somewhat higher than those reported for NUMBER, the *R*-square values reported for the SIZE equations are quite modest.

No strong expectations were developed about the relationship between TIME and the level of encompassingness of interest

groups as measured by NONENCOM, though we have discussed the competing hypotheses of Brace (1985) and Olson (1982, 91) on this issue. And as seen in Table 2, a negative coefficient was generated for five of the six NONENCOM equations, indicating that union and business interest groups are more encompassing in states that have enjoyed the freedom of democratic organization the longest. And while the TIME coefficient is significant in only one case and the *R*-square values are again modest, these findings tend to support Brace's (1985) position on this issue rather than Olson's (1982). And they also suggest that the passage of time since war or domestic upheaval does not have a unidimensional impact on interest-group power. If interest groups do become more encompassing over time, this should weaken their ability or willingness to pursue growth-restrictive policies, even as their size and number increase.

Turning to the interactive-term measures of group power, there seems to be very little relation between the number of years since the Civil War or statehood and interest-group power. Contrary to expectations, all three TIME coefficients in the tests employing dependent measures of union power and one of the coefficients in the business-group power tests are negative, though none are significant. Moreover, none of the *R*-square values in the POWER equations exceed .12, with most falling short of even that modest mark.

The power of interest groups supportive of growth-restrictive policies relative to those opposed to such policies was hypothesized to be negatively related to the number of years since the Civil War or statehood; the organization of the former was presumed to increase the rewards from organization of the latter and thus to encourage their formation. Part of the difficulty of assessing this effect lies in specifying which groups belong in each category, a difficulty that is especially appar-

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**Table 2. OLS Estimates of Relation between Time since Statehood or Civil War and Interest-Group Characteristics, 1975, 1980, and 1975-80**

Dependent Variable		Independent Variable (TIME)		Constant	R <sup>2</sup>
		b	Standard Error		
Number of interest groups	UNUMBER75	-.024	.053	27.65	.01
	UNUMBER80	-.016	.066	26.01	.00
	UNUMBERCH	.000	.064	-1.64	.00
	BNUMBER75	-.131	.283	136.57	.01
	BNUMBER80	.384	.412	149.78	.02
	BNUMBERCH	.515	.327	13.21	.06
Size of interest groups	USIZE75	.001***	.000	.09	.21
	USIZE80	.001***	.000	.10	.20
	USIZECH	-.000	.000	.17	.02
	BSIZE75	-.026	.018	4.52	.05
	BSIZE80	.001***	.000	.25	.24
	BSIZECH	.027	.017	-4.27	.06
Nonencompassingness of interest groups	UNONENCOM75	-.758**	.300	227.93	.14
	UNONENCOM80	-.614	.547	207.12	.03
	UNONENCOMCH	.144	.464	-20.81	.00
	BNONENCOM75	-.001	.001	.93	.01
	BNONENCOM80	-.003	.002	1.67	.06
	BNONENCOMCH	-.002	.001	.74	.05
Absolute interest group power	UPOWER75	-1.04	2.99	913.49	.00
	UPOWER80	-3.11	6.41	1252.72	.01
	UPOWERCH	-2.07	5.86	339.24	.00
	BPOWER75	-.393	.266	103.32	.05
	BPOWER80	.494	.367	41.13	.04
	BPOWERCH	.887**	.406	-61.19	.11
Interest-group power relative to other interest groups	URELB75	.002	.002	.41	.02
	URELB80	-.001	.001	.18	.02
	URELBCH	-.002	.002	.22	.03
	BRELU75	-.002	.002	.41	.02
	BRELU80	.001	.001	.18	.02
	BRELUCH	.002	.002	.22	.03
Interest-group power relative to government	URELGOV75	19.23**	8.90	-2749.40	.11
	URELGOV80	18.49	16.18	-2798.41	.03
	URELGOVCH	-.74	13.05	-49.01	.00
	BRELGOV75	2.22***	.60	-307.05	.26
	BRELGOV80	4.07***	1.31	-507.79	.20
	BRELGOVCH	1.86*	1.01	-200.74	.08

Note: Number of cases = 41.

\*p < .10.

\*\*p < .05.

\*\*\*p < .01.

**Table 3. OLS Estimates of Relation between 1975 Measures of Labor Interest-Group Strength and Economic Growth, 1977-82**

Exponential Rate of Growth	LAG77 <sup>a</sup>	Independent Variable			Constant	R <sup>2</sup>
		1975 Absolute Union Power UPOWER75	1975 Unions Relative to Business URELB75	1975 Unions Relative to Government URELGOV75		
Labor and proprietors' income	-.00001 (.00000)	.0008 (.0007)	-3.31*** (.95)	-.0006*** (.0002)	1.73	.41
Per capita labor and proprietors' income	.083 (.671)	.0002 (.0006)	-1.84** (.92)	-.0001 (.0002)	1.49	.11
Private nonfarm income	-.00001 (.00000)	.0008 (.0007)	-3.07*** (.94)	-.0006*** (.0002)	1.75	.39
Per capita private nonfarm income	.44 (.61)	.0001 (.0006)	-1.72* (.88)	-.0001 (.0002)	1.32	.11
Manufacturing income	-.0002** (.0001)	.0023*** (.0008)	-2.67** (1.22)	-.0001 (.0002)	1.68	.32
Per capita manu- facturing income	-.02 (.85)	.0008 (.0006)	-1.78* (.98)	.0001 (.0002)	1.40	.11

Note: Number of cases = 41. Figures in parentheses are standard errors.

<sup>a</sup>LAG77 is the 1977 value of the dependent variable.

\* $p < .10$ .  
 \*\* $p < .05$ .  
 \*\*\* $p < .01$ .

ent in the use of inverse measures of the business-union relationship in the business and union RELINT equations presented in Table 2. The expected negative coefficients were generated for two of the coefficients in the union equations and one in the business-interest-group equations. However, none were significant at the .10 level. In general, though, the *R*-square values associated with these equations are so small as to suggest that no strong conclusions about the relation between TIME and RELINT should be drawn from these results.

The final results presented in Table 2 address the relationship between TIME and the power of interest groups relative to the level of liberal-party control of government. Mixed signs were again found, with TIME negatively related to URELGOVCH and positively related to the other

dependent measure. And the positively signed coefficients are significant in all but one case. Again, however, the *R*-square values for these equations are not especially strong. Still, the weight of evidence suggests that time is positively associated with group power relative to that of government as Olson implied. Indeed, these findings provide the best support for Olson's view of the impact of time on group influence.

In sum, though, and contrary to the assumptions underlying Olson and Choi's tests, time since statehood or Civil War seems to be neither strongly nor consistently related to the several dimensions of interest-group influence. However, the dimensions of interest-group influence could still be related to economic growth rates in the manner specified in Equation 12.

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**Table 4. OLS Estimates of Relation between Change Measures of Labor Interest-Group Strength and Economic Growth, 1977-82**

Exponential Rate of Growth	Independent Variable				Constant	R <sup>2</sup>
	LAG77 <sup>a</sup>	Change of Absolute Union Power UPOWERCH	Change of Unions Relative to Business URELBCH	Change of Unions Relative to Government URELGOVCH		
Labor and proprietors' income	-.00002 (.00001)	-.00002 (.00050)	2.76*** (.91)	-.0003 (.0002)	1.92	.32
Per capita labor and proprietors' income	.047 (.646)	-.0001 (.0004)	1.58* (.81)	-.0001 (.0002)	1.57	.11
Private nonfarm income	-.00002 (.00000)	-.00003 (.00040)	2.51*** (.88)	-.0003* (.0002)	1.93	.31
Per capita private nonfarm income	.44 (.58)	-.0001 (.0004)	1.44* (.78)	-.0001 (.0002)	1.37	.11
Manufacturing income	-.0002** (.0001)	-.0004 (.0006)	1.71 (1.12)	-.0005** (.0002)	1.89	.27
Per capita manu- facturing income	.19 (.80)	-.0003 (.0005)	1.19 (.87)	-.0003 (.0002)	1.48	.10

Note: Number of cases = 41. Figures in parentheses are standard errors.

<sup>a</sup>LAG77 is the 1977 value of the dependent variable.

\* $p < .10$ .

\*\* $p < .05$ .

\*\*\* $p < .01$ .

*Economic Growth and Interest-Group Characteristics.* These results are presented in Tables 3-6. The union findings reported in Table 3 employ the *static* 1975 measures of UPOWER and UREL<sub>B</sub> while those reported in Table 4 employ the *change* from 1975 to 1980 measures of those variables. Starting with the former, the results presented in Table 3 provide only very weak support for the Olson construct, which would lead us to expect that the UPOWER<sub>75</sub>, UREL<sub>B75</sub>, and URELGOVCH coefficients would be uniformly negative across the equations using the several measures of the rate of economic growth. But the UPOWER coefficients are uniformly positive, though only one was significant. While very weak, these results suggest that if anything, higher levels of union power were associated with *higher*

rates of economic growth. In contrast, though, the UREL<sub>B75</sub> coefficients were negative as expected and significantly different from zero in all six equations. All but one of the URELGOVCH coefficients were negative as expected, and two were discernibly different from zero. Obviously, such results are difficult to interpret. The problem lies in reconciling the conflicting UPOWER<sub>75</sub> and UREL<sub>B75</sub> results. And the most obvious interpretation would be that it is not union power per se that inhibits growth, but union power relative to the power of government and especially business interest groups.

But even this interpretation conflicts with the results presented in Table 4 which employ change-score measures of UPOWER and UREL<sub>B</sub>, measures that we have argued are more appropriate to

**Table 5. OLS Estimates of Relation between 1975 Measures of Business Interest-Group Strength and Economic Growth, 1977-82**

Exponential Rate of Growth	LAG77 <sup>a</sup>	Independent Variable			Constant	R <sup>2</sup>
		1975 Absolute Business Power BPOWER75	1975 Business Relative to Labor BREL75	1975 Business Relative to Government BRELGOV75		
Labor and proprietors' income	-.00002 (.00001)	.015 (.013)	.001 (1.873)	-.007** (.003)	1.80	.37
Per capita labor and proprietors' income	.13 (.65)	.002 (.011)	1.24 (1.57)	-.002 (.003)	1.47	.11
Private nonfarm income	-.00002 (.00001)	.016 (.013)	-.31 (1.86)	-.007** (.003)	1.81	.34
Per capita private nonfarm income	.47 (.58)	.002 (.010)	1.21 (1.52)	-.002 (.003)	1.31	.11
Manufacturing income	-.0002*** (.0001)	.046*** (.015)	-3.72* (2.16)	.001 (.001)	1.75	.34
Per capita manu- facturing income	-.11 (.90)	.017 (.011)	-.25 (1.66)	.003 (.003)	1.43	.13

Note: Number of cases = 41. Figures in parentheses are standard errors.

<sup>a</sup>LAGG77 is the 1977 value of the dependent variable.

\* $p < .10$ .

\*\* $p < .05$ .

\*\*\* $p < .01$ .

assess the cause-and-effect relationship implied in the Olson construct. When the UPOWERCH measure is substituted for UPOWER75, all six coefficients are negative as expected, but none are significant. And all of the URELBCH coefficients change sign from those observed for URELB75, and four were significant at the .10 level. Still, all of the URELGOVCH coefficients carried a negative sign, and again two were statistically significant. At most, these results suggest that the power of unions relative to business is positively related to economic growth rates, the opposite of the conclusion drawn from Table 3.

It remains unclear how union interest-group power is related to economic growth. The union results presented in Tables 3 and 4 provide little support for Olson's claim that union power per se is negatively associated with growth rates.

Clearly, union power *relative* to that of business does matter, but how it matters depends on whether one is viewing the static measures in Table 3 or the change measures in Table 4. More unambiguously, it appears that union power *relative* to government is negatively associated with economic growth.

Turning to the business interest-group test results presented in Tables 5 and 6, we find similar results. Table 5 presents the results for the equations using the 1975 measures of BPOWER, BREL75, and BRELGOV. Like the union results, these equations produced uniformly positive BPOWER75 coefficients, indicating that business interest-group power is positively associated with economic growth. All but one of the coefficients were non-significant, however. More mixed results were found for the 1975 measure of

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**Table 6. OLS Estimates of Relation between Change Measures of Business Interest-Group Strength and Economic Growth, 1977-82**

Exponential Rate of Growth	Independent Variable				Constant	R <sup>2</sup>
	LAG77 <sup>a</sup>	Change of Absolute Business Power BPOWERCH	Change of Business Relative to Labor BREL BCH	Change of Business Relative to Government BRELGOVCH		
Labor and proprietors' income	-.00002 (.00001)	.025** (.010)	-4.27*** (1.28)	-1.09** (.45)	1.93	.35
Per capita labor and proprietors' income	.09 (.72)	.0014 (.0058)	-1.91* (1.12)	-.0016 (.0017)	1.55	.12
Private nonfarm income	-.00002 (.00000)	.0003 (.0006)	-3.61*** (1.14)	-.0058*** (.0016)	1.94	.32
Per capita private nonfarm income	.57 (.66)	.00001 (.00560)	-1.63 (1.08)	-.0020 (.0016)	1.32	.13
Manufacturing income	-.0002* (.0001)	.0052 (.0084)	-2.95* (1.57)	-.0047** (.0023)	1.88	.26
Per capita manu- facturing income	.29 (.91)	.0019 (.0060)	-1.59 (1.20)	-.0019 (.0019)	1.47	.07

Note: Number of cases = 41. Figures in parentheses are standard errors.

<sup>a</sup>LAG77 is the 1977 value of the dependent variable.

\* $p < .10$ .

\*\* $p < .05$ .

\*\*\* $p < .01$ .

business-group power relative to unions; three of the BREL75 coefficients were negative as expected, and one of these was discernibly different from zero. Like the union results, the BRELGOVCH coefficients were again negative in four of the equations, and two were significant at the .05 level. Generally, these results are quite weak and provide only indirect support for the Olson model in the 3 significant and negative coefficients among the 18 substantively important coefficients.

More supportive results were found for the equations employing change scores for the BPOWER and BREL75 terms, as seen in Table 6. Contrary to expectations, the BPOWERCH coefficients are positive, and one is significant. But, as expected, all of the BREL75 and BRELGOVCH coefficients are negatively signed and more than half are significant at at least the .10 level.

Indeed, these results are the most supportive of the Olson construct of those reported here.

Thus, there is less problem interpreting the impact of business interest-group power than there was in the case of union power. It would seem that it is the *relative* power of business interest groups that is negatively associated with retarding economic growth, not business interest-group power per se. If anything, absolute business interest-group power seems to be positively associated with economic growth.

### Interpreting the Findings

Quite clearly, these results provide little support for the Olson construct. The dis-

confirming evidence is both extensive and strong. *First*, we were largely unable to replicate the strong results reported by Olson, Dye, and Choi on the relationship between time since statehood or the Civil War and total growth rates. As seen in Table 1, while the TIME coefficients were negative as expected in the *total* growth-measure equations, they were very weak. Moreover, the per capita measures of growth generated uniformly positive coefficients in Table 1. Thus, our results are more consistent with those of Brace and Dudley (1985) who argue that previous evidence of a relationship between time and growth is spurious. Such a relationship might have appeared because time is highly related with region, and regions have experienced different patterns of growth in part due to federal spending patterns. As federal spending patterns were geographically equalized after the late 1970s (Lowery, Brunn, and Webster 1986), the relationship would be expected to disappear.

*Second*, there appears to be little or no relation between time since statehood or the Civil War and various measures of interest-group influence. The signs of the coefficients reported in Table 2 varied widely depending on whether 1975, 1980, or change-score measures of interest-group influence were used, and even the supportive findings were very weak.

*Third*, the interest-group influence-growth rate relationships tested in Tables 3-6 generated results that were more often the opposite of what Olson would lead us to expect than they were consistent with his model. Generally, the absolute power of interest groups was found to be positively related to the several measures of economic growth rates.

At best, some indirect support was provided the Olson construct in the findings that the *relative* power of interest groups was often negatively related to growth. This was strongest for the business interest-group equations for which the

support was, in comparison to that for unions, relatively consistent. Thus, Whiteley's expectation that business interest groups would be the major actors in growth-restrictive policies is provided some support by these results. Just as importantly, both union and business interest-group power *relative* to governmental power evidenced a consistently negative association with economic growth. Yet, any support these results provide the Olson model remains incomplete, given the weak relationships between the relative power measures and time since statehood, the underlying engine of the Olson model.

Thus, these results provide little support for the Olson construct as applied to understanding economic growth in the U.S. states. What do these results say, then, about the Olson model more generally? We do not want to suggest that these results totally invalidate the Olson construct. The Olson model is theoretically powerful and may have much to say about cross-national growth rates.<sup>7</sup> Still, we have argued that more is required to validate the model than tests of the simple relationship between time since war or since domestic upheaval and growth rates. The Olson construct is made up of a complex set of relationships, each of which must be empirically supported. Thus, even though more elaborate than tests of previous investigators, even ours is not a full test of the model. We would argue that Olson's model remains largely untested.

This conclusion applies especially to the results of Olson, Dye, and Choi. Their findings on economic growth in the U.S. states have provided much of the presumed support for the Olson model to date. However, they fail to test several key linkages of the model. Moreover, their measures of interest-group influence are rudimentary. When better measures are employed and the key linkages are specified, the supportive evidence for

## Economic Growth in the States

Olson in the case of the U.S. states largely evaporates.

We should not be surprised by this. As Pryor (1983, 91) has noted, the time periods involved are simply too long to expect the simple relationships posited by the Olson model to hold. Moreover, much more is involved obviously in the determination of economic growth rates in the U.S. states than is accounted for in the Olson model. From 1977 to 1982, the U.S. states endured the deepest recession of the postwar era, continued to face the growing pressures of international competition, were undergoing major shifts in population from the rust belt to the sun belt, and were actively adopting a number of new economic policies under the rubric of industrial policy. Something more than time since statehood is obviously accounting for the booms and busts experienced by Texas and Massachusetts over the last decade. Neither Choi's test nor those presented here account for any of these factors. While interest groups certainly play some role in economic growth, they are not the sole or perhaps even the most important determinant. In short, the Olson model is too spare and misspecified.<sup>6</sup> Thus, the traditional call for more research is fitting, especially in the context of the current lively debate over "business climate" in the states. Better and more complex tests of the Olson construct are needed. And until those tests are developed, the Olson construct remains an interesting—but unproven—account of varying economic growth rates.

### Appendix

The following indicators were used to test the Olson construct:

*Economic Growth.* LPI = exponential rate of growth of income of labor and proprietors, 1977–82; PCLPI = exponential rate of growth of per capita labor and proprietors' income, 1977–82; NPI = exponential

rate of growth of private nonfarm income, 1977–82; PCNPI = exponential rate of growth of per capita private nonfarm income, 1977–82; MPI = exponential rate of growth of manufacturing income, 1977–82; PCMPI = exponential rate of growth of per capita manufacturing income, 1977–82.

*Time.* TIME = number of years since state admitted to the union (or since the Civil War in the case of Confederate states).

*Number of Interest Groups.* UNUMBER75 = number of union organizations employing registered lobbyists, 1975; UNUMBER80 = number of union organizations employing registered lobbyists, 1980; UNUMBERCH = (UNUMBER80) – (UNUMBER75); BNUMBER75 = number of business organizations employing registered lobbyists, 1975; BNUMBER80 = number of business organizations employing registered lobbyists, 1980; BNUMBERCH = (BNUMBER80) – (BNUMBER75).

*Size of Interest Groups.* USIZE75 = proportion of employees of nonagricultural establishments that are unionized, 1975; USIZE80 = proportion of employees of nonagricultural establishments that are unionized, 1980; USIZECH = (USIZE80) – (USIZE75); BSIZE75 = percentage of the labor force in manufacturing or services, 1975; BSIZE80 = percentage of the labor force in manufacturing or services, 1980; BSIZECH = (BSIZE80) – (BSIZE75).

*Group Nonencompassingness.* UNONENCOM75 = (UNUMBER75)/(USIZE75); UNONENCOM80 = (UNUMBER80)/(USIZE80); UNONENCOMCH = (UNENCOMPASS80) – (UNENCOMPASS75); BNONENCOM75 = (number of business firms employing registered lobbyists)/(number of trade associations employing registered lobbyists), 1975; BNONENCOM80 = (number of business firms employing registered lobbyists)/(number of trade associations employing registered lobbyists), 1980; BNON-



ENCOMCH = (BNONENCOM80) - (BNONENCOM75).

*Absolute Group Power.* UPOWER75 = (UNUMBER75)(USIZE75)(UNONENCOM75); UPOWER80 = (UNUMBER80)(USIZE80)(UNONENCOM80); UPOWERCH = (UPOWER80) - (UPOWER75); BPOWER75 = (BNUMBER75)(BSIZE75)(BNONENCOM75); BPOWER80 = (BNUMBER80)(BSIZE80)(BNONENCOM80); BPOWERCH = (BPOWER80) - (BPOWER75).

*Relative Group Power.* URELB75 = [(BPOWER75)/(UPOWER75)](-1); URELB80 = [(BPOWER80)/(UPOWER80)](-1); URELBCH = (URELB80) - (URELB75); BRELU75 = [(BPOWER75)/(UPOWER75)]; BRELU80 = [(BPOWER80)/(UPOWER80)]; BRELUCH = (BRELU80) - (BRELU75).

*Group Power Relative to Government.* URELGOV75 = (UPOWER75)/(Klingman-Lammers index); URELGOV80 = (UPOWER80)/(Klingman-Lammers index); URESLGOVCH = (URELGOV80) - (URELGOV75); BRELGOV75 = (BPOWER75)/(Klingman-Lammers index); BRELGOV80 = (BPOWER80)/(Klingman-Lammers index); BRELGOVCH = (BRELGOV80) - (BRELGOV75).

## Notes

An earlier version of this paper was presented at the 1986 meeting of the Midwest Political Science Association, Chicago.

1. Specifically, like our measures, Dye's dependent measures of economic growth rates covered a four-year period, 1972-76. And as in our test, Dye's independent variables were lagged two years prior to the beginning of the period covered by the dependent measures—1970. Lange and Garrett's growth measures covered the period 1974-80.

2. Use of lists of registered lobbyists raises the question of the comparability of state laws. Lobbyists are now required to register in 47 of the 50 states. In 35 of the 44 states making up the 1980 data set used here, a lobbyist is defined as anyone receiving compensation to influence legislative action. In the 9 states not using this definition, a more restrictive one is used; a lobbyist is defined as anyone representing someone else's interests or attempting to influence

legislation, or both. The difference in definition appears to be a major problem in only one state, Florida. States also make exceptions to their laws, with some exempting members of the media, religious organizations, or persons who testify before committees. These exemptions do not appear problematical. Most states also exempt state officials. For a description of the specific coding procedures used to construct the interest-group measures, see Conover and Gray 1983.

3. We thank Anne Wall for her coding of these data.

4. The index measures the extent of liberal-party control of government via a weighted average of the proportion of years the Democratic party controlled the governorship, House, and Senate in nonsouthern states. The control of the governorship is weighted twice as heavily as that of the House and Senate to reflect the importance of the governor in economic development policies.

5. While the assertion of temporal stability remains questionable, the use of the Klingman and Lammers index is still warranted given its greater face validity relative to the liberal-party control measure and because the Wright, Erikson, and McIver index also rests on an assertion of temporal stability.

6. In the few cases where higher values were observed, approaching .79 in some cases, all involved the POWER variables, something that should not be surprising, given the fact that they are the numerators in the relative power measures. To further evaluate the collinearity threat, Equation 12 was reestimated with only the POWER terms. The signs and significance levels generated by these regressions were generally the same as those reported in the tables. Moreover, the key problem with the findings is the abundance of wrongly signed coefficients and not nonsignificant, though correctly signed, coefficients. This suggests that collinearity is not the source of the failure to find supportive results for the Olson model.

7. Still, even cross-national tests of Olson's model generate mixed results. For example, see the results in the recent special issue of *Scandinavian Political Studies*, where Lane and Ersson (1986) provide confirmatory support for the model and Gustafsson (1986) provides disconfirming evidence.

8. Also, both our work and that of Olson neglects the potentially positive benefits of group activity. Brace and Dudley (1985) argue forcefully that redistributive goals can be more easily achieved in older states which tend to have higher average incomes though lower rates of growth, an achievement Olson ignores. Moreover, some of the more recent and innovative developments in state industrial policy suggest that business and labor can, at least under certain circumstance, cooperate with government to enhance economic growth opportunities.

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# INSTITUTIONS, THE ECONOMY, AND THE DYNAMICS OF STATE ELECTIONS

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*As the U.S. states develop their political institutions and take greater responsibility for their economic well-being, two concerns that have long driven research on national elections—electoral insulation and economic accountability—should become central in research on state elections. I investigate institutionalization's effects on the vulnerability of state elections to major periodic forces—coattails, turnout, and economic conditions—and how political responsibility for economic growth is apportioned between presidents and governors in state elections. The investigation relies upon dynamic models of state legislative and gubernatorial outcomes estimated with a pooled data set comprised of most states and elections in the years 1940–82. The results, which have important implications for state government more broadly, indicate that institutionalization has substantially insulated legislative elections against major threats and that state legislators and governors have less to fear from their state economies than is often thought, but also that state elections are becoming more susceptible to swings in the national economy.*

State government, long a backwater of U.S. politics, has been moving steadily into the mainstream. Its institutions have been growing in size, competence, and legitimacy while control of them has become the object of increasing competition. The responsibilities of state government have also increased, and that trend, now driven by fiscal constraints upon the national and local governments, is likely to continue apace. Research, of course, has monitored this progress and described aspects of it in valuable detail. Still, it is fair to say that the implications of state development have not been adequately appreciated, for research on the states has not joined the mainstream itself (Jewell 1982).

The problem arguably begins with our understanding of how state government is constituted, that is, with our understanding of state elections. Much of the prog-

ress that state government has been making is a result of a process of political development that has potentially profound implications for state elections. State government has been institutionalizing, and in the process it has been developing not only larger and more complex governing organizations but, perforce, greater amounts of protection from its political environment. Elections are an integral part of this process, for their insulation from powerful political shocks and surges is a virtual prerequisite for institutional stability and development. At the national level, research has amply demonstrated this. Congress did not become thoroughly institutionalized until it was able to stabilize its membership (Brady 1985; Polsby 1968), and its development in recent years has been bound up with the efforts of its members to maintain, if not increase, their electoral secur-

ity (cf. Fenno 1978; Fiorina 1977; Jacobson 1987). At the state level, analogous efforts have been under way for some time. Yet their electoral consequences, and those of institutionalization more generally, have not been investigated with comparable sophistication or thoroughness. In fact, much the same can be said of research into the forces that regularly influence state-election outcomes and that state politicians are presumably trying to resist.

The strongest case in point is the state economy. Election outcomes have been shown to be affected in a wide variety of contexts by economic conditions (e.g., Alt and Chrystal 1983), and there is ample reason to expect the same to hold true in the states. State politicians must be concerned about the condition of their economies not only now that state responsibilities for social welfare and economic development have increased but because economic conditions affect the ability of states to attract and retain businesses and taxpayers (e.g., Oates 1972; Tiebout 1956). Nevertheless, there is no systematic evidence, at least of the kind that has been amassed in research on national elections, that state economic conditions have electoral consequences (but cf. Peltzman 1987). Models of state elections (e.g., Caldeira and Patterson 1982b; J. Campbell 1986; Patterson 1982; Piereson 1977; Tompkins 1984) have so far attended to factors other than the economy and—returning to the general point—been estimated with seriously limited samples of elections. It remains to be seen, therefore, not only how economic and other influences on state elections have responded to institutionalization but precisely what weights those influences have in the first place.

My purpose is to take a look at these issues with the perspective provided by several fairly comprehensive models of state-election outcomes. Examining legislative elections first, and then gubernator-

ial, I will introduce macromodels of state-level outcomes. The models will then be used to estimate the influence of basic economic and noneconomic electoral conditions and to assess how their influence varies across time and across states. The models are estimated using a pooled data set that includes most of the states and all of the even-numbered election years in the period 1940–82. The results, which clarify especially the effects of institutionalization and economic performance on state elections, have plain implications for state government more generally. They suggest that the states are systematically overcoming one of their traditional institutional limitations (see, e.g., Key 1949; Wahlke et al. 1962) and that they are less constrained by their own economic circumstances than is often thought (see, e.g., Gramlich and Laren 1984), but that they have become more vulnerable to swings in the national economy.

### **The Core Model of Legislative Elections**

Although state legislative elections exhibit a potentially bewildering degree of diversity, their aggregate dynamics are strikingly regular. In a pattern every bit as consistent as that in congressional elections, state legislative elections have regularly gone against the party of the president at midterm. Only once in the past 50 years has the president's party increased its share of either lower or upper state legislatures in the even-numbered off-year elections. And since 1950 it has always lost, dropping an average net of 13 legislatures in each midterm election (e.g., Bibby 1983). There is some evidence that this pattern is a product of presidential coattails (see, e.g., J. Campbell 1986), but like the pattern that similarly characterizes congressional elections, it may be the result of other national forces as well.

Whatever its source, the similarity be-

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tween national- and state-level election dynamics is potentially important. State elections are extraordinarily diverse. They number over seven thousand and are contested in single- and multimember districts that represent from 23 hundred to 600 thousand voters. The issues that divide the parties vary from state to state and often differ from those that divide the parties at the national level. The factors that account for variations in normal partisan voting across the states include idiosyncrasies of culture and history that subvert general explanations—not to mention statistical models. It is therefore important that at least in their dynamics these diverse elections have something in common with national elections, which, as a group, are well behaved. It raises the possibility that changing party fortunes in state government may be able to be understood—and modeled—in the same familiar terms as changing party fortunes in the national government.

Accordingly, a core model of state legislative elections is specified along the same lines as a well-balanced dynamic model of congressional elections. The Democratic party's percentage of seats in a state legislature is assumed to be a simple linear function of three types of variables that figure prominently in models of national electoral change.<sup>1</sup>

### **Coattails**

Voting in state legislative elections may be influenced by the preferences that voters hold for candidates for higher office: not only president, but governor, and U.S. senator.<sup>2</sup> To capture these influences at the aggregate level, the coattail effects are assumed to be proportional to the strength of state support for the Democratic candidate for each of these higher offices. The Democratic percentages for president, senator, and governor therefore enter the model during each year in which elections for those of-

fices are held. In years when those offices are not up for election, the respective variables take on the value of 0, indicating the absence of coattails. However, to avoid confusing the absence of coattails that arises when a higher office is not at stake with the weakness of coattails that occurs when a higher office is nominally or poorly contested, dummy variables designating presidential, senatorial, and gubernatorial election years are also specified. These variables, taking on the value 1 if the designated office is at stake, and 0 if it is not, permit the estimation of different base levels of Democratic support (by introducing different intercepts) for years when the ballot is occupied by different combinations—none, some, or all—of higher offices. In addition, the model includes a separate measure of the Democratic percentage for governor in nonpresidential years. Its purpose is to allow for the greater influence of gubernatorial voting on state legislative elections when presidential voting is not a factor—a condition that has become more common as gubernatorial races have recently been moved to off years.

### **Turnout**

Changes in levels of voter participation between presidential and nonpresidential election years may have differential effects on the political parties and consequently on the shares of state legislative seats that they win. For a number of reasons (cf. A. Campbell 1967; Kernell 1977) the party of the candidate that wins the presidency tends to benefit from the surge in turnout that typically occurs during the presidential election year and to suffer from the decline in turnout that occurs during the nonpresidential year. *That* state-election turnout varies predictably with political conditions has been established (see, e.g., Caldeira and Patterson 1982a; Patterson and Caldeira 1983), but

to *what effect* has not. Here that effect is assumed to occur because marginal voters—those who are stimulated to vote in a presidential-election year but are insufficiently motivated to vote in the subsequent midterm election—vote (and then fail to vote) disproportionately for the lower-office candidates of the president's party. This effect is measured by an interaction between (1) the change in turnout between the current and the prior election and (2) the change in the Democratic percentage for president between the most recent presidential election and the one four years previous.<sup>3</sup> For example, in nonpresidential election years the Democratic party will increase its share of state legislative seats (i.e., the interaction will be positive) if its previous presidential candidate fared worse than its candidate four years before and if turnout declined from the presidential to the midterm election.

### National Economic Conditions

The relationship between economic conditions and election outcomes is potentially more complex at the state level than at the national. At the state level the relationship may be a function of the performances of both the state and the national economies and of attributions of responsibility to state officials—for example, governors—as well as to presidents. Still, state election outcomes ebb and flow much like congressional outcomes which depend on the tides of the national economy. It is therefore useful, at least in order to establish a standard for comparison, to estimate the sensitivity of state elections to the national economy—or to the president's perceived performance in managing it. Questions of state-level conditions and responsibilities are best postponed until the simpler one of national effects is answered at least provisionally.

The only question in specifying the more conventional national influence is,

What economic heuristic should voters be assumed to be employing? That of course is a matter of some debate in research on congressional elections (cf. Bloom and Price 1975; Fiorina 1981; Kiewiet 1983; Kramer 1971, 1983; Rivers 1986). In research on state elections it is an open question. Here, voter evaluations of presidential performance in managing the national economy are assumed to be based on *changes* in the annual (calendar-year) rate of growth or decline in national real per capita disposable income. These changes are assumed to affect the president's party symmetrically in state elections, economic improvements helping his party as much as deteriorations hurt it. To associate the economic changes with the appropriate party, the income variable is multiplied by an incumbency dummy variable, +1 if the president is a Democrat, -1 if a Republican.

The model specification is completed with a variable (besides the state intercepts discussed below) that establishes a baseline upon which the party builds its share of the legislature in each election. The baseline is indicated by the percentage of seats held by the Democrats after the preceding state legislative election. The coefficient on this variable indicates the rate at which the party retains its share of seats in the state legislature, all things being equal. The effects of coattails, turnout, and economic conditions add to, or subtract from, the level of support that the coefficient of retention establishes by operating on the prior legislative share.

The model was estimated for lower legislatures only; upper legislatures present serious difficulties like overlapping and unequal terms and were therefore excluded. Also omitted were southern states, because until recently their solid one-party systems provided no change to explain; states holding lower-legislative elections in odd years, because they complicate the even-year time series; states

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**Table 1. Coefficient Estimates for the Core Model of Control of Lower State Legislatures, Non-South, 1940-82**

Variable	Coefficient	t-score
Constant <sup>a</sup>	21.02	6.08*
Prior % Democratic seats <sup>b</sup>	.42	7.40*
% Democratic, president	.32	4.90*
Presidential election year	-10.52	3.13*
% Democratic, Senate	.33	6.87*
Senatorial election year	-16.15	6.52*
% Democratic, governor	.35	5.69*
Off-year % Democratic, governor	.08	1.74**
Gubernatorial election year	-20.32	6.80*
Surge or decline	.008	2.98*
Presidential responsibility for change in national income growth <sup>c</sup>	.53	4.70*
Number of cases	751	
R <sup>2</sup>	.75	
Adjusted R <sup>2</sup>	.74	

Note: The dependent variable is the Democratic percentage of legislative seats by state.

<sup>a</sup>The variable list excludes dummies that were included for 34 states.

<sup>b</sup>The instrumental variables used to estimate this model include all exogenous variables and their two-year lags.

<sup>c</sup>Annual change in the percentage-point growth of national real per capita disposable income.

\* $p \leq .05$ , two-tail test.

\*\* $p \leq .10$ , two-tail test.

with nonpartisan legislative elections; and states with four-year (rather than two-year) terms for the lower house. Still, the data set includes all of the nonsouthern states except Maryland, Minnesota, Nebraska, and New Jersey. The period analyzed extends back from 1982 to 1940; the 1930s were excluded because the economic and electoral volatility of that decade introduced significant statistical outliers that tended to distort estimation. The result is a pooled cross-section time-series data set with 35 states, 22 election years, and 770 cases.<sup>4</sup> The great virtue of such a data set is that it permits the estimation of multivariate models that could not be estimated confidently with the historical time series provided by individual states.

The model was estimated with a limited-information maximum-likelihood program, and the two most likely sources

of bias in the parameter estimates were corrected: the coefficient on the lagged endogenous variable, prior percentage of Democratic seats, was estimated with instrumental variables (as indicated in Table 1), and the constant was divided into separate states "fixed effects"—to accommodate different levels of Democratic strength—by specifying a dummy variable for each state (except one that is represented by the constant itself).<sup>5</sup> The fixed-effects specification should also improve the efficiency of the estimates by reducing serial correlation among the errors and heteroskedasticity (Stimson 1985). Further improvements in efficiency were not pursued, but neither were they strongly indicated.<sup>6</sup>

The results, reported in Table 1, are encouraging: they suggest that variations in state-legislative-election outcomes can be well explained by many of the same fac-

tors that account for variations in national-election outcomes. The model explains most of the variation (74%) in the Democratic share of state legislative seats across states and across time and correctly predicts the party in control of the state legislature in 83% of the cases. Moreover, all of the hypothetical influences on those elections have the correct sign and magnitude, and achieve acceptable levels of significance.

Presidents, senators, and governors all have significant coattails: a 10% increase in the vote for any one increases the Democratic share of seats in the lower house by 3.2%, 3.3%, and 3.5% respectively; off-year gubernatorial coattails add only a little: .8%. By one conventional standard—presidential coattails in congressional elections of roughly 3% (Ferejohn and Calvert 1984; Kramer 1971)—the effects of all higher-office coattails on state legislative elections have been equally sizable.

But by another standard this is not quite so. The coattail coefficients gauge only the marginal influence of votes for higher office on votes for lower office. To gauge the influence of coattails on the outcomes of elections, it is also necessary to consider the vote that the lower office would have received in the absence of a contest for the higher office. A virtue of analyzing coattail effects with data for all legislative election years is that this counterfactual quantity can be estimated. The dummy coefficients for the higher-office election years permit the total effects of the coattails—the products of the coattail coefficients and the respective higher-office votes—to be corrected for the level of Democratic legislative support that can be expected when coattails are not possible. The correction suggests that presidential candidates exert the greatest potential influence over state legislative outcomes.

This is evident in considering the thresholds that higher office support must

exceed before coattails add to the Democratic share of seats that would result without higher offices on the ballot. For Democratic presidential candidates that threshold has been 32.9%; even weak national tickets have contributed something extra to the Democratic result in presidential years in the states.<sup>7</sup> For Democratic senatorial candidates, however, the threshold has been 49.7%, and for gubernatorial candidates 57.7%. If each of these candidates were to receive 50% of the vote, the total effect of presidential coattails would be a hefty 5.43%, of senatorial coattails a trivial .10%, and of gubernatorial coattails a *minus* 2.71%. In other words, presidential coattails are always likely to help the party's legislative cause, but senatorial and gubernatorial coattails will only help if the candidates are unusually popular. This helps to account for the midterm losses that the president's party invariably suffers in state legislative elections. The absence of presidential coattails in midterm elections and the weakness of compensating gubernatorial and senatorial coattails have cost the president's party 4.33% of lower-house seats in the off years.<sup>8</sup>

Contributing further to those losses has been the midterm decline in turnout. With typical turnout swings from presidential years to nonpresidential years in the range of 5–20%, and presidential voting swings in the range of 0–15%, the interaction between the two has averaged 111.9 in absolute value for the 22 elections and 35 states in this sample. When multiplied by the significant surge or decline coefficient of .008, changes in turnout (and its assumed composition) are estimated to have given the party of the victorious presidential candidate a legislative seat boost of .9% during presidential years and a penalty of the same magnitude in off years. This effect is only one-fourth that of the coattail effect, but it indicates nonetheless that presidential races influence legislative races at the state level in



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much the same way that they do at the national: by affecting not only the direction of voter choices but the number of people who show up at the polls to make them.

The final influence on state legislative races that is included in the core model is the national economy and the reward or blame that the president receives for its performance. Changes in the rate of real per capita disposable-income growth are estimated to exert a significant, independent effect on the share of state legislative seats captured by the president's party.<sup>9</sup> A one-point increase in the rate of real disposable-income growth during the year prior to an election increases the proportion of seats held by the president's party by .53%, an effect comparable in magnitude to those typically found in macroanalyses of congressional elections (see Rivers 1986). Because economic conditions are typically changing more favorably during presidential than during midterm election years—this measure averaged +.67% and was never negative during the former but averaged −.64% during the latter—they will also contribute to midterm seat losses by the president's party. On average, changes in real per capita disposable income nationwide have increased the president's party's control over a state legislature by .35 percentage points during presidential years and decreased it by .34 percentage points during off years.

These results indicate that state legislative elections are subject to much the same set of national influences as congressional elections. While coattails, especially those of the president, have exerted the largest average influence, economic conditions, when they slip or improve sharply, can cause significant legislative swings; and voter participation, where it oscillates severely, can induce a sizable measure of legislative instability too. From 1940 to 1982 these influences together were responsible for an average off-year loss by the president's party of 5.48 percentage

points in the typical nonsouthern state legislature—a loss that may well be sufficient to account for the persistently large number of legislatures over which the president's party relinquishes control at midterm. This, of course, is of some importance because that loss has never been adequately explained. But the results may also hold importance for state government more broadly.

### State Economic Conditions and State Legislative Elections

State government is widely believed to be influenced by state economic conditions. Political scientists have come to see state fiscal capacity as a serious constraint upon state policy-making (e.g., Jennings 1979). Economists, going a step further, have suggested that the constraint is even more severe, tightened by the competition among states for businesses and taxpayers (e.g., Oates 1972). This constraint, economists argue, has more profound implications for subnational government than political scientists generally acknowledge (but cf. Peterson 1981), for it pushes those governments away from policies that are anathema to rational taxpayers—for instance, steeply progressive taxation and aggressively redistributive spending—and toward others, such as economic development, that are more nearly Pareto-optimal. Yet, if state government is constrained, either severely or less so, by its economic circumstances, it is not entirely clear how that constraint operates.

Economists stress that citizens impose the constraint through their mobility—their willingness to “vote with their feet” (see, e.g., Tiebout 1956). But the evidence for that, though extensively investigated, is only mixed (Rose-Ackerman 1983). The other logical possibility is that the constraint is imposed through voting in the ordinary sense. This may be done di-

**Table 2. Annual Interstate Variances for State Economic Conditions,  
Averaged by Decade**

Decade	Percentage Growth in Real Income <sup>a</sup>	Change in Real Income Growth <sup>b</sup>
1930s	63.03	117.44
1940s	40.24	63.87
1950s	12.03	36.57
1960s	11.02	22.97
1970s	6.70	21.28
1980s	4.15	8.11

Note: Data from each even-numbered year for 1932-82, inclusive.

<sup>a</sup>Annual percentage-point change in real per capita personal income by state.

<sup>b</sup>Annual change in the percentage-point growth of real per capita personal income by state.

rectly, as economists posit, with voters selecting among packages of burdens and benefits proposed by candidates for state offices. Or it may be done indirectly, as political scientists generally believe, through voter evaluations of government performance. If a state government pursues policies that are economically ill advised (in whatever sense), the state economy will suffer and the incumbent government will be turned out of office. In other words, state government may be held economically accountable through retrospective voting.

That hypothesis, which has not been extensively investigated and which enjoys more anecdotal than systematic support (cf. Jewell and Olson 1982; Turett 1971), is explored here by incorporating into the core model of state legislative elections a measure of state economic conditions: the annual change in real personal income by state.<sup>10</sup> This measure (see Table 2) reveals a great deal of variation in the year-to-year performances of state economies. Annual income growth rates, which fall primarily in the range of  $\pm 5\%$ , have annual interstate variances (col. 1), even after the Great Depression, as high as 40%. The annual changes in those growth rates (col. 2), which are used in the election model, vary even more. While the

variation in both has declined steadily over time—a point that will be addressed subsequently—there would appear to be sufficient differences in state economic conditions for voters to react to them, as well as to common national conditions.

There is, in any case, a potentially different line of political accountability for state conditions. In the core model the influence of the national economy was examined by assuming that it is the president and the president's party (including its state branches) who are held accountable for national economic performance. In this extension of that model the influence of the state economy will be assessed analogously: by assuming that it is the governor and his or her party who are held accountable for state economic performance.<sup>11</sup> The effect of state economic conditions on state legislative elections is therefore specified as an interaction between the annual change in real per capita personal income by state and a dummy variable (+1 if Democrat, -1 if Republican) for the party of the governor. This variable was added to the core model, and the new model was estimated as before.

The results, reported in Table 3, provide little support for the hypothesis, however it is derived, that voters hold state politicians responsible for state eco-

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**Table 3. Coefficient Estimates for Model of Control of Lower State Legislatures, Including Both National and State Economic Conditions, Non-South, 1940-82**

Variable	Coefficient	t-score
Constant <sup>a</sup>	20.60	5.94*
Prior % Democratic seats <sup>b</sup>	.42	7.47*
% Democratic, president	.32	4.90*
Presidential election year	-10.39	3.10*
% Democratic, Senate	.33	6.89*
Senatorial election year	-16.07	6.49*
% Democratic, governor	.35	5.64*
Off-year % Democratic, governor	.08	1.83**
Gubernatorial election year	-20.23	6.78*
Surge or decline	.008	2.97*
Presidential responsibility for change in national income growth <sup>c</sup>	.54	4.83*
Gubernatorial responsibility for change in state income growth <sup>d</sup>	.07	1.37
Number of cases	751	
R <sup>2</sup>	.75	
Adjusted R <sup>2</sup>	.74	

Note: The dependent variable is the Democratic percentage of legislative seats by state.

<sup>a</sup>The variable list excludes dummies that were included for 34 states.

<sup>b</sup>The instrumental variables used to estimate this model include all exogenous variables and their two-year lags.

<sup>c</sup>Annual change in the percentage-point growth of national real per capita disposable income.

<sup>d</sup>Annual change in the percentage-point growth of real per capita personal income by state.

\* $p < .05$ , two-tail test.

\*\* $p < .10$ , two-tail test.

conomic performance. The coefficient on state economic conditions implies a shift of legislative seats of only .07% for every one-percentage-point change in real income, and even that comes with a standard error that is too large to distinguish the effect from 0. Putting aside its statistical significance, its substantive significance pales by comparison to national conditions, which are comparably measured: the effect of presidential performance in managing the national economy is more than eight times as large as the effect of gubernatorial performance in managing the state economy. The influence of state economic conditions is not being swamped, moreover, by the presence of national economic conditions. When state economic changes are the only economic conditions in the model, they remain small and nonsignificant.

The effects of the other variables in the model are essentially the same, whether state economic conditions—and gubernatorial responsibility for them—are included in the estimation or not. Again, state legislative elections appear to turn on factors that are substantially national. To be sure, gubernatorial coattails matter, and these may be influenced by the performance of the state economy. Turnout counts too, and it oscillates differently from state to state. But it is impressive how little state economic management—so central in economic conceptions of state government—seems to influence state legislative elections. This is not to say that politicians do not fear voter retribution for poor economic performance nor pursue policies that will avoid it. After all, they are blamed for those dips in all state economies (i.e., na-

tional downturns) that occur while a president of their party is in office. But state politicians may have less to fear about the economic competitiveness of their states *per se* than is often thought; their elections do not much depend on it.

### **Institutionalization and State Legislative Elections**

One reason why state legislative elections may not turn on state management of economic conditions is that state legislators do not want them to. Relative to the national government, state governments have little control over their economies. They have no access to monetary policy instruments and they are limited in their ability to make fiscal policy by constitutional requirements (at least in 49 of the states) that they balance their budgets. Over time, they have also seen the national economy—not to mention the international economy—become more integrated, as evidenced in Table 2, and in the process factors beyond their control gain in influence over their economic well-being. It may be true that a state's tax and expenditure policies will have a greater effect on its economic condition in a more homogeneous national economy than in one that is not well integrated—those policies representing the major marginal differences between state business climates. But because state politicians can only expect to benefit from such policies if their enactments are not countered by other states, the political benefits of trying to boost the state economy, even when state policies may be most consequential, are uncertain. Of course, wishing to remain blameless for state economic performance does not make one so. However, politicians have ways of protecting themselves against adverse swings in electoral conditions, and there are reasons to believe that state politicians have been seeking this protec-

tion—not only against the vicissitudes of state economies, but against all conditions over which they have limited control.

Traditionally, state legislators were not known for this sort of behavior. The archetypical state legislator was an amateur politician who served a term or two, then stepped down to return to private life (see, e.g., Hyneman 1938; Wahlke et al. 1962). Because they lacked the ambition to make politics a career, state legislators did not worry about protecting themselves against electoral tides. There was also little incentive to behave any differently. State legislatures were poorly paid, and the job occupied relatively little of their time, many legislators being in session for only several months every other year. Even if a legislator happened to find the position attractive, moreover, he or she had few resources, such as personal or professional staff or other perquisites of office, to help maintain it. State legislators were consequently not well insulated against electoral adversity—and state legislatures were not well institutionalized.

This is of considerable importance because institutionalization is generally regarded as a necessary condition for the effective functioning of a democratic organization (see, e.g., Converse 1969; Huntington 1965; Polsby 1968). Without it a democratic organization is prone to unresolved conflict, sudden changes in purpose, unrepresentative behavior, and problems of competence. In an institutionalized organization, members have a stake in its perpetuation, contending interests have incentives to compromise, and problems are more effectively solved through a permanent division of labor. The institution is more motivated to respond to constituent needs and is better organized to serve them.

Though once generally lacking in institutional capacity, and perhaps still seriously short of it (see, e.g., Houseman 1986), state legislatures have, over the last

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two decades, been adding capacity rapidly. Legislative sessions have been increased in length and frequency; committee and personal staffs have been expanded; ancillary research and support agencies have been created; and legislative salaries and perquisites have been increased (Sabato 1982). State legislators consequently have more incentives to try to maintain their positions—to protect themselves against electoral defeat—and superior means to do so. Like members of the U.S. House of Representatives, state legislators now preside over hundreds of domestic programs and have leverage, however limited, over an enormous bureaucracy, larger in total than that of the federal government (Chubb 1985). So situated, many state legislators have access to all of the tools of the incumbent's trade: casework, pork-barreling, credit claiming, and advertising. It follows that state legislators will be pursuing protective strategies, and that state legislatures should be becoming more institutionalized.

The electoral effects of these developments, however, remain rather unclear. It is known (see, e.g., Calvert 1979; Ray 1974, 1976; Rosenthal 1974; Shin and Jackson 1979) that member turnover in state legislatures declined, albeit unevenly, during the period 1940–80, as more incumbents chose to make the legislature a career. This follows, of course, from the increasing attractiveness of state legislative positions. But this is only a part, and in fact only an early part, of the process of institutionalization. Institutions automatically acquire stability when members decide that they value and want to remain in them. But that stability is a long way from that which institutions acquire when members find ways to protect their positions against outsiders—that is, electoral challengers—who also find those positions attractive and can bid for the support of voters. Legislatures do not become thoroughly institutionalized,

therefore, when legislators decide they want to keep their positions but when they demonstrate that they can.

So viewed, the electoral dimension of institutionalization can be gauged by the parameters of the model of state legislative elections. If state legislators are succeeding in securing their positions, the control of state legislatures should prove increasingly unresponsive to forces over which legislators have no control, and increasingly dependent upon prior levels of party strength. To see if this is so, the model, including both national and state economic conditions, was estimated for two successive periods, 1940–66 and 1968–82. The breaking point, to some degree arbitrary, coincides with several developments that profoundly affected the institutionalization of U.S. government at the national as well as the state level: the weakening of political parties and mass partisanship, the ascendancy of entrepreneurial politics in congressional elections, and the expansion of state fiscal, administrative, and legislative capacities. At the national level these developments contributed to the atomization of congressional campaigns and the decentralization of Congress. At the state level they provided incentives and resources for development in the same direction. A comparison of elections on either side of this watershed (regardless, estimation indicates, of which year from the mid-1960s to the early 1970s is chosen to mark it) is a way of gauging the effects that these developments had on the sensitivity or vulnerability of state legislative elections to various electoral forces. It is, in other words, a way of measuring electoral insulation. The comparison made in Table 4 uses the coefficients of the extended legislative-election models, estimated as before.

Plainly, legislative elections give signs of institutionalization. To begin with, election outcomes in the more recent period are three times as dependent on the

**Table 4. Coefficient Estimates for Complete Model of Control of Lower State Legislatures, Non-South, Successive Periods 1940-66, 1968-82**

Variable	1940-66		1968-82	
	Coefficient	t-score	Coefficient	t-score
Constant <sup>a</sup>	19.66	5.18*	13.45	1.91**
Prior % Democratic seats <sup>b</sup>	.23	3.39*	.73	7.06*
% Democratic, president	.52	5.89*	.13	1.25
Presidential election year	-20.64	4.19*	-7.01	1.48
% Democratic, Senate	.40	6.01*	.11	2.08*
Senatorial election year	-20.33	5.99*	-4.28	1.48
% Democratic, governor	.41	5.12*	.22	2.28*
Off-year % Democratic, governor	.08	1.38	-.10	1.40
Gubernatorial election year	-21.57	5.75*	-9.13	2.12*
Surge or decline	.002	.56	.014	4.57*
Presidential responsibility for change in national income growth <sup>c</sup>	.55	4.02*	.99	5.54*
Gubernatorial responsibility for change in state income growth <sup>d</sup>	.03	.57	.05	.66
Number of cases	468		282	
R <sup>2</sup>	.82		.81	
Adjusted R <sup>2</sup>	.80		.77	

Note: The dependent variable is the Democratic percentage of legislative seats by state.

<sup>a</sup>The variable list excludes dummies that were included for 34 states.

<sup>b</sup>The instrumental variables used to estimate this model include all exogenous variables and their two-year lags.

<sup>c</sup>Annual change in the percentage-point growth of national real per capita disposable income.

<sup>d</sup>Annual change in the percentage-point growth of real per capita personal income by state.

\* $p \leq .05$ , two-tail test.

\*\* $p \leq .10$ , two-tail test.

prior level of party support as they are in the earlier period, the coefficient of retention (.73) reaching nearly 75%. This in itself is a measure of increasing electoral stability, but more telling measures are those for the effects of outside electoral forces—which, overall, decline. Coattails, the strongest influence on legislative swings when all years (i.e., 1940-1982) are analyzed together, are an even stronger influence in the earlier period. Each higher office pays a dividend of nearly half a percentage point in seats for each percentage point in Democratic voting it attracts. Moreover, the thresholds of support that must be achieved before these offices exert their influence remain roughly the same.<sup>12</sup> In contrast, coattails

in the more recent period are very short. Those of presidential and senatorial candidates are about one-third their earlier sizes, and those of gubernatorial candidates about half.

There are exceptions to this pattern. Legislators are not better protected against surges and declines in the size of the electorate (hence in the number of presidential supporters). In fact, they are more vulnerable to this phenomenon in the later period than in the earlier. This is simply an indication that presidential elections have not declined in influence over state elections as much as the coattail effect alone suggests. Legislators are also no less immune to the consequences of state economic performance—but not because

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those consequences have become more severe. To the contrary, state economic conditions had little influence on state elections in the forties, fifties, and sixties, and continued to have little influence in the years thereafter. Notwithstanding the considerable variation in state economic performance prior to the 1960s, there is no evidence that state legislatures were held accountable for it. But given the limited ability of state legislators to affect economic conditions within their states and the consequently weak incentives for them to seek responsibility for economic performance, this should not be entirely surprising.

It appears that when it comes to assigning responsibility for economic performance, state voters have generally and increasingly looked outside of the state—to the national economy and the president's imputed performance in managing it. Through the mid-1960s a one-percentage-point change in the rate of real per capita disposable-income growth changed the average state-legislative-seat share of the president's party by one half of a percentage point; after that the effect nearly doubled. This development is perfectly consistent with the steady integration of the national economy and the sustained strength that economic retrospective voting appears to have had at the national level (e.g., Fiorina 1981). But it stands as an important exception to the process of electoral insulation that is under way in state legislatures more generally. While, all things being equal, legislative elections exhibit greater overall stability, less sensitivity to higher-office influences, and continued invulnerability to state economic performance, they do show an increased susceptibility to national economic performance.

How do these changes add up? Despite the increased influence of the president's management of the nation's economy, state legislative elections have become substantially insulated against periodic

swings, including those at midterm against the president. Based on the mean values of electoral forces during the respective periods—which do not differ themselves very greatly—the average estimated loss of seats for the president's party fell from 8.73% in the period 1940–66 to 3.34% in the period 1968–82. Evidently, the incentives and resources that state governments provided their legislators helped to insulate legislative elections against most periodic shocks.

This is confirmed in a comparative analysis of states that have made more and less progress in establishing conditions conducive to legislative reelection. A good proxy for these conditions—because it is highly correlated with other measures of institutional development, such as legislative professionalization (Citizen's Conference on State Legislatures 1971), and is itself a direct measure of reelection incentives—is total annual legislator compensation (or the sum of salary, benefits, and cash allowances). It was used along with state median family income to divide the states into two groups: states that were, toward the end of the period under study, offering legislative compensation equal to, or in excess of, median family income; and states that were not. In the higher-compensation states, the position of legislator was considered to be sufficiently attractive to provide career incentives—an individual could support a family on the income from the position alone—while in the lower-compensation states, it was not. Among the 35 states in this analysis, 10 fell into the higher-salary group and 25 into the lower.<sup>13</sup>

The question, then, is whether states that differ in their political incentives and resources also differ in their electoral dynamics. It appears that they do. The model was estimated as before (but excluding state economic conditions, which consistently proved nonsignificant) for each of the two groups. In higher-salary states (see Table 5) there is strong

**Table 5. Coefficient Estimates for Core Model of  
Control of Lower State Legislatures, Non-South, 1940-82,  
High and Low Legislative Salary States**

Variable	High Salary States		Low Salary States	
	Coefficient	t-score	Coefficient	t-score
Constant <sup>a</sup>	13.95	2.36*	21.43	6.24*
Prior % Democratic seats <sup>b</sup>	.60	6.02*	.41	7.86*
% Democratic, president	.34	3.32*	.37	4.89*
Presidential election year	-8.50	1.42	-13.33	3.40*
% Democratic, Senate	.16	2.38*	.33	6.06*
Senatorial election year	-8.45	2.41*	-16.72	5.72*
% Democratic, governor	.28	2.39*	.40	5.81*
Off-year % Democratic, governor	.15	1.60	.06	1.10
Gubernatorial election year	-16.99	3.12*	-22.25	6.61*
Surge or decline	.008	1.74**	.009	2.50*
Presidential responsibility for change in national income growth <sup>c</sup>	.60	4.12*	.60	6.46*
Number of cases	234		619	
R <sup>2</sup>	.74		.77	
Adjusted R <sup>2</sup>	.72		.76	

Note: The dependent variable is the Democratic percentage of legislative seats by state.

<sup>a</sup>The variable list excludes dummies that were included for 34 states.

<sup>b</sup>The instrumental variables used to estimate this model include all exogenous variables and their two-year lags.

<sup>c</sup>Annual change in the percentage-point growth of national real per capita disposable income.

\* $p < .05$ , two-tail test.

\*\* $p < .10$ , two-tail test.

evidence of institutionalization; in the lower-salary states there is less. Where the incentives and resources for electoral protection are high, the rate at which the prior share of seats is normally maintained is 50% greater than where incentives and resources are low. In the poorer-paying states, most of the influences from which legislators would presumably like to be insulated are stronger than they are in the better-paying states—and none is significantly less influential. Electoral insulation and institutional development indeed appear to go hand-in-hand.

But this is not to suggest that institutionalization is taking place only in those states that have enriched their legislatures the most. A striking result of the comparison of well-endowed and not-so-well-endowed legislatures is that the differences in the coefficients between the

respective models are not as large as might be expected: they are noticeably smaller than the differences (see Table 4) that were estimated for all states for successive periods. Electoral insulation evidently differs more between the past and the present than between the more and less developed states of today. In fact, when separate models were estimated for earlier and later years in the high- and low-salary states, the results indicated that electoral insulation is occurring across the board.<sup>14</sup> The insulation of state-legislative elections is not a widely scattered phenomenon. To be sure, it is more pronounced in some states than in others. But in virtually all states it appears to be establishing a level of electoral, and hence institutional, stability considerably higher than that for which state legislatures have traditionally been known.



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### **Institutional Capacity and Gubernatorial Elections**

State executives, like state legislatures, were once subject to considerable criticism for their incapacity to govern effectively. Executive branches were badly fragmented, their many agencies often headed by independently elected chiefs. The governorship was hemmed in by various constitutional limits on its authority. The individuals who occupied the office were not infrequently political amateurs who lacked an effective base of political power. And this says nothing of the difficulties that governors inevitably faced in overseeing their economies. In recognition of these problems, efforts to reform state executives began to gain momentum shortly after World War II and became a regular part of state politics from the 1950s through the early 1970s. While these efforts addressed many problems, they often focused on gubernatorial weaknesses thought to derive from the circumstances of their election.

Certain gubernatorial election rules, it was argued, tended to undermine the political strength of the governor (Sabato 1982). Two-year terms, prohibitions against self-succession, and elections coinciding with elections for the presidency conspired to produce executive instability and ineffectiveness. Governors were often instant lame ducks, and if they were not emasculated by that status, their prospects for longevity were so uncertain, given the unpredictability of electoral conditions, that they had difficulty establishing a credible position of power while in office. For these reasons, many states reformed their governorships: the number of states providing four-year terms for governor increased from 23 in 1940 to 46 today; the number permitting self-succession (at least once) rose from 28 in the late 1950s to 46 in the 1980s; and the number electing governors in presidential years fell from 30 in 1956 to 13 in 1984.

Coupled with real improvements in the salary, staff, and constitutional authority of state chief executives, changes in the governorship, accumulated throughout the postwar era, established a foundation for political independence and electoral strength in this key state office.

But how has all of this affected the election of governors? As in the case of state legislatures, there is considerable uncertainty. Presidential coattails have obviously become less important in states that moved their gubernatorial elections to off years, but their importance in those states that did not is unclear. While there is some evidence that incumbency provides governors little advantage, efforts to disentangle incumbency from other sources of gubernatorial support (e.g., Piereson 1977; Tompkins 1984; Turett 1971) have reached mixed conclusions about its past and current benefits. To be sure, there is agreement (see, e.g., Jewell and Olson 1982; Patterson 1982) that partisanship exerts a strong influence on gubernatorial elections, but even that result could benefit from a consideration of competing explanations and a larger set of cases. Finally, and this may be of the greatest importance for state government more broadly, the electoral accountability of governors for state economic performance has scarcely been investigated. Are strong state executives, like U.S. presidents, now held accountable for their economic showing?

### **A Model of Gubernatorial Elections**

While state reformers have insulated gubernatorial elections from the direct influence of presidential coattails, there is ample evidence that they have not insulated them from presidential electoral forces more generally. Until 1986, the president's party had gone more than 60 years without increasing its share of

**Table 6. Coefficient Estimates for  
Gubernatorial Election Outcomes, 1940-82**

Variable	Model with National Economic Conditions		Model with National and State Economic Conditions	
	Coefficient	t-score	Coefficient	t-score
Constant <sup>a</sup>	4.51	.43	2.64	.24
Prior % Democratic, governor <sup>b</sup>	.91	4.24*	.94	4.44*
Gubernatorial incumbency	-.59	.52	-.73	.63
% Democratic, president	.01	.15	.01	.16
Presidential election year	-.12	.03	-.11	.03
% Democratic, Senate	.12	2.49*	.12	2.26*
Senatorial election year	-6.36	2.39*	-5.86	2.12*
Surge or decline	.008	2.60*	.009	2.70*
Presidential responsibility for change in national income growth <sup>c</sup>	.32	3.75*	.32	3.68*
Gubernatorial responsibility for change in state income growth <sup>d</sup>			.07	1.79**
Number of cases	666		666	
R <sup>2</sup>	.65		.64	
Adjusted R <sup>2</sup>	.61		.61	

Note. The dependent variable is the Democratic percentage of the gubernatorial vote by state.

<sup>a</sup>The variable list excludes dummies that were included for 34 states.

<sup>b</sup>The instrumental variables used to estimate this model include all exogenous variables and their two-year lags.

<sup>c</sup>Annual change in the percentage-point growth of national real per capita disposable income.

<sup>d</sup>Annual change in the percentage-point growth of real per capita personal income by state.

\* $p < .05$ , two-tail test.

\*\* $p < .10$ , two-tail test.

governorships in a midterm election, and from 1950 to 1984 it lost an average of 6.2 in each one (Bibby 1983). This suggests, of course, that gubernatorial elections are subject to many of the same forces as national and, as we have seen, state-legislative elections. The model of gubernatorial election outcomes is therefore specified using the same potential influences as before.

The percentage Democratic for governor is modeled as a simple linear function of a baseline of support, given by the prior percentage Democratic for governor, and a number of forces expected to produce deviations from it: coattails, turnout, economic conditions, and incumbency. All variables are measured and specified as in the model of state

legislatures except, of course, gubernatorial coattails, which are irrelevant and excluded, and gubernatorial incumbency, which is specified with a dummy variable, 1 if the candidate currently holds the office and 0 otherwise. The model was estimated as before, but for 45 rather than 35 states, for the same period, 1940-82. Only states that held gubernatorial elections in odd years were excluded. The results, including national economic conditions alone and then in conjunction with state economic conditions, are reported in Table 6.

Gubernatorial elections display important differences from state legislative elections. To begin with, they are less predictable. The model correctly "predicts" the winner of slightly less than three-fourths

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of the gubernatorial elections in this analysis while the comparable model gets the control of state legislatures right in more than 80% of the cases. The model also explains 15% less of the electoral variance. Gubernatorial elections, being high-visibility affairs, evidently attract attention to factors more idiosyncratic than those in the model—for instance, the personal attributes of the candidates—and, in consequence, are tougher to forecast. But this is not to say that gubernatorial elections are substantially unpredictable: they do have a core of stability. Much as previous research has suggested, gubernatorial elections have a strong general tendency to reproduce the level of support for the candidate of each party. All things being equal, gubernatorial candidates retain 90% of the support received by their respective parties' previous contestants.

Gubernatorial elections are also less influenced by systematic outside forces than are legislative elections. Presidential coattails, which are being structured out of existence through off-year gubernatorial elections, have a negligible impact when governors and presidents do run for election at the same time. Senatorial coattails are significant, but their impact on the Democratic percentage for governor is one-third their impact on the party's share of the state house. This is not to say that national-level forces are unimportant. Presidential voting in conjunction with oscillations in turnout—surge and decline—produces shifts in gubernatorial voting. And judgments about the president's performance in managing the national economy induce a smaller but still nontrivial response. Finally, state economic conditions, and the assumption of gubernatorial responsibility for them, have a significant impact on gubernatorial election outcomes. The impact, however, is small (one-fourth the impact of national conditions), and it does not increase in size when it is estimated in the absence of national economic conditions.

If gubernatorial elections turn on economic performance, it is generally the performance of the president in managing the national economy that really matters.

Viewed together, these influences lead to several clear conclusions about gubernatorial elections. First, they are strongly influenced by party or, more specifically, the respective levels of support for the parties in the most recent gubernatorial election. Second, gubernatorial elections are regularly, though modestly, perturbed by national conditions. The aggregate average toll of those conditions on voting for the president's party in midterm gubernatorial elections is 1.9% per race, an amount that may well be sufficient, given the competitiveness of these contests, to account for quadrennial losses in governorships by the president. Third, these elections are affected more by national economic conditions and presidential responsibility for them than by state conditions and gubernatorial contributions to them. While state economic conditions do make a detectable difference—they raise the expected average midterm loss by the president's party to 2.2% per race—they indicate that state governors, like their legislatures, are not typically held accountable for state economic performance. Finally, gubernatorial elections display a healthy measure of unpredictability, the model "picking" the winner incorrectly over 25% of the time. While party establishes a firm baseline, and outside influences encourage a regular pattern of change, these elections can easily turn on the qualities of the candidates themselves, qualities that a macromodel can never adequately represent.

How much do these conclusions owe to the reforms of state governorships? That cannot be answered definitively with these estimates because reforms were being instituted over most of the period that has been analyzed. Unlike the reforms of state legislatures that occurred in most states over a fairly short period—that is, the last

20 years—the reforms of state executives spanned the entire postwar era, occurring at different times in different states. It is therefore inappropriate to ask, as we did of state legislatures, whether their elections look different in recent years than in early years or in some states rather than others. There is no simple way to divide the states or the time period of this analysis into “reformed” and “unreformed” groups of cases. A more refined method of categorization, beyond the scope of this inquiry, is plainly required.<sup>15</sup>

Still, the results do not suggest that governors have been given effective control over their own fates. One major reform, the provision of the opportunity for self-succession, appears to have had little affect per se. Incumbency provides no advantage (the coefficient is nonsignificant) over and above that which redounds to any candidate who follows a popular governor of the same party onto the ticket. Another reform, off-year election scheduling, has largely freed governors from direct presidential coattails, but it has subjected them to midterm presidential punishment—meted out for poor national economic performance and manifest, in part, through declines in turnout—that governors cannot overcome by taking credit for their state’s economic performance. In important respects, then, gubernatorial elections remain contests of party and personality and not of performance—at least not that of the governor and the state.

### Discussion

State elections stand to shed considerable light on the recent progress of state governments because their outcomes are logically bound up with important influences on state policy-making—specifically, institutional development and economic accountability. Governments that through institutional development are able to insulate their elections to an

appropriate degree against uncontrollable external perturbations and inevitable changes in economic conditions are more likely to acquire the capacity for effective and representative performance. The capacity of state government has long been suspect because of various institutional deficiencies and economic vulnerabilities. Recent institutional and economic development has eroded some of these suspicions, to be sure. But doubts persist as the states are asked to assume ever greater shares of responsibilities once held by the national government. The directions that these developments will take state government are inevitably unclear. Yet there is more uncertainty than is necessary, for research on state government, even in a fairly well-developed area such as electoral behavior, has simply not kept pace with them.

In this exploratory effort, I have tested out a different approach to the analysis of state elections—an approach inspired largely by analogous investigations of national elections—and taken an initial look at a number of obvious electoral indicators of state economic constraints and institutional limitations. The approach appears reasonably promising. It does have some distance to go before it offers a full account of gubernatorial elections, but any macro explanation is bound to be confounded by the apparent role of personalities in those contests. In any event, it appears to be a very useful means of evaluating explanations of lower-visibility contests such as those for the state legislature. And it offers some telling insights into common assumptions about state government.

To begin with, state elections do not turn appreciably on the performance of the state government in managing its economy. This result at least calls into question the motivating assumption of models of interstate competition. If there is little evidence that state politicians are blamed or rewarded according to their

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performances as economic managers, there is reason to question how substantially those politicians are guided by the belief that they will be. Voters in state elections appear to hold politicians outside of the state, specifically the president and the president's party, responsible for economic conditions and, if conditions in a state should differ from those in neighboring states, to hold the governor only minimally responsible. Obviously, a good deal of research remains to be done on these matters; among other things, alternative specifications of state economic performance—for instance, changes in tax and expenditure policies—merit attention. But it stands to reason that in an increasingly homogeneous national economy, state governments, lacking as they are in economic-policy leverage, would not invite and could arguably deflect responsibility for state economic performance.

There is one other important respect in which state elections confound traditional suspicions. Legislative elections are becoming noticeably well insulated against political adversity—and in a way that has clear implications for the institutional capacity of state governments. Elections to state legislatures, while systematically affected by all of the dominant forces in U.S. elections (coattails, turnout, and economic conditions) have grown fairly immune to all of them (except the national economy) and, in the process, acquired substantial stability. Because these developments were especially pronounced in states that increased the resources of their legislators, there is good reason to believe that electoral insulation developed as part of a more general process of legislative institutionalization. That is important, for it suggests that as states provide legislators incentives and resources to maintain their positions, they can overcome a traditional incapacity of their governments.

## Notes

The first draft of this paper was presented at the 1986 Annual Meeting of the American Political Science Association, and a revised version appeared as a Brookings Discussion Paper in Governmental Studies. The comments, criticisms and assistance of Steven Smith, Paul Peterson, Clifford Winston, Joe White, and Paul Wright were invaluable.

1. It would be preferable to break the relationship between coattail voting and the composition of legislatures into two components: (1) the effect of votes for a presidential candidate on votes for state candidates of the same party and (2) the effect of votes for those state candidates on the party's share of seats in the legislature. This is done for the U.S. Congress by Ferejohn and Calvert (1984). It cannot generally be done for state legislative elections because historical voting returns are not sufficiently available, especially for the years prior to 1970 (cf. Tidmarch, Loneragan, and Sciortino 1986).

2. Voting for the U.S. House of Representatives may also influence voting in individual state-legislative races. But since this analysis is aggregated to the state level, it could easily be misleading to use the average Democratic vote across all House elections in a state as a measure of any particular coattail effect.

3. Because data on turnout for state-legislative elections are not available for most years and most states in the analysis, a surrogate, turnout for U.S. House elections, is employed.

4. The data for this analysis were obtained from the Inter-University Consortium for Political and Social Research (State-Level Congressional, Gubernatorial, and Senatorial Data for the United States, 1824–1972; and Partisan Division of American State Governments, 1834–1984); *Statistical Abstract of the United States, 1976–84*; *Congressional Quarterly Almanac, 1975–83*; *Historical Statistics of the United States: Colonial Times to 1970*; and *Economic Report of the President, 1975*. The states excluded from the legislative analysis are Alabama, Florida, Georgia, Kentucky, Maryland, Minnesota, Mississippi, Nebraska, New Jersey, North Carolina, South Carolina, Texas, and Virginia.

5. The effects of the explanatory variables—coattails, turnout, and the economy—may also vary across the states (on coattails, see, e.g., Campbell 1986). But since there is no reason to expect this variation to be systematic, it will not bias the estimates of the slopes that are specified in this model to apply to all states.

6. Strictly speaking, the estimation procedure remains inefficient because serial correlation within states and heteroskedasticity across them were not corrected. But the inefficiency is likely to be slight: the lagged endogenous term in the model picks up

much of the autodependence in the variables, the multiple state intercepts tend to reduce autocorrelation and heteroskedasticity, and a trend variable, year, included in the preliminary estimation of the model proved nonsignificant. Whatever serial correlation remains in the model is therefore unlikely to distort the standard errors or the tests of statistical significance appreciably—and, of course, the coefficient estimates remain consistent. Moreover, further improvements in efficiency that might be obtained by reducing autocorrelation or heteroskedasticity are best pursued using consistent covariance matrix estimation methods (Newey and West n.d.; White 1980) that have not yet been adapted for pooled applications. The methods that are available, such as generalized difference models that correct each state's error structure individually (Pindyck and Rubinfeld 1981), are inordinately cumbersome and provide dubious improvements.

7. The total coattail effect is given by the formula, (coattail coefficient) (vote for higher office) – (coefficient of dummy variable indicating years when the higher office is contested). Because the dummy variable coefficient is negative, the coattail effect does not become positive until the product in the formula exceeds the dummy variable coefficient. The threshold that the higher-office vote must cross to provide positive coattail effects is therefore obtained by dividing the dummy-variable coefficient by the coattail coefficient and multiplying by –1. For example, the threshold for positive coattails for presidential voting is given by the expression,  $-1(-10.52/.32) = 32.9\%$ .

8. The average percentage Democratic for president over this period was 46.42%; hence, the average effect of presidential coattails on state legislative seat shares was  $(.32)(46.42\%) - (10.52) = 4.33\%$ . In midterm elections the absence of presidential coattails cost the president's party the same amount.

9. Experimentation with other specifications of economic conditions indicated that this specification was robust. The coefficient on the change in the rate of real disposable-income growth maintained its strength when other economic conditions—specifically, changes in the inflation rate and in the unemployment rate—were included in the model. The coefficients of the other economic conditions, while always of the correct sign, varied in magnitude and significance, and were therefore excluded from the final specification.

10. Data on disposable income by state are not available back to 1940, so personal income is used instead. Evidence that the latter is a good proxy for the former, at least for this sort of analysis, comes from using it instead of disposable income as an indicator of presidential economic performance. Its estimated effect is very close in magnitude to that reported in Table 1 for disposable income.

11. It is also possible to assume that the president

and his party are held responsible for state economic conditions. When tested, however, it proved significant only when presidential responsibility for national conditions was omitted from the model. When presidential responsibility for both national and state conditions were specified in the model, national conditions invariably proved to be the better gauge of electorally significant evaluations of presidential performance.

12. The presidential threshold is 39.5%, the senatorial 51.1%, and the gubernatorial 53.1%.

13. The salary and family-income data are from 1979. The higher-salary states are: Alaska, California, Illinois, Massachusetts, Michigan, New York, Oklahoma, Ohio, Pennsylvania, and Wisconsin. The lower-salary states are all of the rest except those excluded from the analysis (see n. 4). Several states fell just short of the higher-salary category and may, from a behavioral standpoint, be misclassified. But the purpose of this part of the analysis is not to model the electoral dynamics of particular states (which data limitations severely constrain anyway); the purpose is to gauge the electoral consequences of institutional maturation, for which legislative salaries are but a proxy.

14. In both groups of states the coefficient on the prior percentage Democratic increased by roughly the same amount, and the coefficients on the coattail variables dropped by approximately the same amounts.

15. What may well be required is a state-by-state specification of policy interventions, taking advantage of pooling to estimate multiple interrupted time series simultaneously.

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# NUCLEAR BRINKMANSHIP WITH TWO-SIDED INCOMPLETE INFORMATION

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**A** brinkmanship crisis with two-sided incomplete information is modeled as a game of sequential bargaining in which each state is uncertain of its adversary's resolve. The sequential crisis equilibria are characterized explicitly and used to analyze the influences of resolve, misperception, and the status quo on escalation and crisis stability. The description of brinkmanship as a contest of resolve is found to be misleading: the state with the greatest resolve may not prevail in the crisis; a state may be less, not more, likely to prevail the greater its resolve; and a states' expected payoff may be less, not more, the greater its resolve. Moreover, reducing misperception may destabilize a crisis. Surprisingly, increasing the stake a potential challenger has in the status quo may not make a challenge less likely. Finally, crises involving severe conflicts of interest are shown to be less likely than crises not entailing a severe conflict of interest.

**R**ecent work on crisis bargaining has studied nuclear brinkmanship when there is one-sided incomplete information in which only the potential challenger is uncertain of the type of its adversary (Powell 1987). I here extend the analysis of nuclear brinkmanship to the case in which there is two-sided incomplete information. A brinkmanship crisis is modeled as a game of sequential bargaining in which each state is uncertain of its adversary's resolve. The game is then solved for its sequential equilibria. Two-sided incomplete information is a more natural setting and allows for a richer bargaining dynamic.

With one-sided incomplete information, it was found that contrary to the implication suggested by the analyses of Ellsberg (1959), Jervis (1972, 1978), Snyder (1971, 1972), and Snyder and Diesing (1977), an increase in an adversary's resolve may make a state more, not less, likely to escalate. Moreover, the state with the greatest resolve may not

prevail. Both findings continue to hold with two-sided incomplete information. But the richer dynamic also shows that the effects of an increase in an adversary's resolve may vary with the stage of the crisis. The greater an adversary's resolve, the less likely an irresolute potential challenger may be to challenge the status quo. But if it does dispute the status quo, it is more likely to escalate if its challenge is resisted.

The richer dynamic also indicates that describing a brinkmanship crisis as a contest of resolve (e.g., Jervis 1979a, 1979b; Schelling 1966) is misleading. This description would seem to suggest that the state with the greatest resolve will prevail; that the greater a state's resolve, the more likely it is to prevail; that the greater a state's resolve, the greater its expected payoff; or that it should at least be possible to describe the states' strategies as functions of the states' levels of resolve. In the model, all of these assertions prove incorrect.

In the model the situation actually facing a state can be distinguished from the situation that the state believes itself to be facing. The degree to which this belief differs from the actual situation provides a measure of misperception and may be used to explore some of the consequences of misperception. The result is that although there would be no crises if misperception could be completely eliminated, reductions in the level of misperception may be destabilizing as well as stabilizing.

The value that a potential challenger places on maintaining the status quo plays a surprising role in crisis bargaining. Detente is at least in part based on the general belief that increasing an adversary's stake in the status quo makes a challenge to the status quo less likely, because disputing a more valuable status quo puts more at risk. This is a belief about foreign policy, but it has an obvious parallel in crisis bargaining. In the model, an increase in the value of the status quo does make a crisis less likely. But at least for small increases in the value of the status quo, the potential challenger is not less likely to challenge the status quo. Rather, the challenger's adversary is more likely to submit if challenged. Apparently, being known to have a greater stake in the status quo may be used as a bargaining lever by a potential challenger to convey an impression of greater resolve to its adversary, thereby making the adversary more likely to back down.

The analysis also implies that some types of crises are more likely than other types. If there is a crisis, then, perhaps surprisingly, the challenger is more likely to be irresolute than resolute. This implies that crises in which there is a severe conflict of interest are less likely than crises in which there is not a severe conflict of interest.

I first develop the model and then present the sequential crisis equilibria (which are formally derived in the Appendix).

The effects of resolve, misperception, and the status quo on escalation and crisis stability are then considered. Finally the distribution of types of crises is examined.

### The Model

If two adversaries possess secure second-strike forces and the requisite command and control capabilities, neither state can credibly threaten to launch a massive unlimited nuclear attack against the other during a crisis. Launching such an attack would bring one's own destruction. Whenever the cost of imposing a sanction, a massive nuclear attack in this case, is more costly than not imposing it, one cannot credibly threaten to invoke the sanction deliberately. Thomas Schelling (1960, 199-201; 1966, 92-125) has used the analogy of brinkmanship to explore the bargaining dynamics of this type of nuclear crisis. The model analyzed below formalizes and simplifies this analogy.

In Schelling's analogy, two adversaries are roped together and standing near a brink. The rope ensures that if one goes over the brink, so will the other. In the confrontation, each adversary tries to coerce the other into submission by raising to an intolerably high level the risk that they will tumble into the chasm.

Brinkmanship captures the essence of the credibility problem inherent in a situation in which imposing the sanction is more costly to oneself than not imposing it. In the analogy, neither party can credibly threaten to jump into the abyss deliberately. Thus, as long as the adversaries are in complete control of whether or not they go over the brink, there is no danger of going over and consequently no way to use the fear of going over to exert coercive pressure.

Schelling's solution to the credibility problem is "the strategy that leaves something to chance." For him, the brink is not

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## Nuclear Brinkmanship

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a sharp edge that one can approach safely before deciding whether or not to jump. The brink is rather "a curved slope that one can stand on with some risk of slipping, the slope gets steeper and the risk of slipping greater as one moves toward the chasm" (1960, 199).

Describing the brink in this way means that one could go over the brink despite both adversaries' unwillingness to take any action that was certain to force them into the abyss. With gusty winds overhead and loose gravel underfoot, the parties are not fully in control of events. Accordingly, each can exert coercive pressure on the other by approaching the chasm and thereby creating a risk that despite one's desire not to slip, one still might slip accidentally. Assuming that neither does slip, then as both near the brink one of them would sooner or later find the risk intolerable and would submit. In this way, nuclear crises become a competition in taking risks.

Before turning to the model, two aspects of this analogy deserve special emphasis because they help to define the kind of crisis for which brinkmanship may be an appropriate analogy. First, there is only one sanction in the analogy and only one means of coercion. The single sanction is going over the brink, and this corresponds to an unlimited nuclear exchange in a crisis. The single means of coercion is manipulating the risk of suffering the sanction. Accordingly, options exercised during a crisis are not to be judged primarily in terms of their effect on the balance of forces but in terms of their effect on the risk of an unlimited nuclear war (Jervis 1979b, 1984; Schelling 1960, 190-94; Schelling 1962; Schelling 1966, 106-16). In actual crises, however, there may be other means of exerting coercive pressure. One of these means is the use of limited attacks intended to inflict pain and to make the threat of future punishment more credible.<sup>1</sup> Another means is the use of options designed to

enhance one's relative military strength. The more important the role that these other means of coercion play in actual crises, the less appropriate is the analogy of brinkmanship.

The second aspect of the analogy to be emphasized is the crucial part that accident plays. Throughout the confrontation at the brink, each participant always prefers any other outcome to that of sliding into the chasm. There are no circumstances in which one would decide to jump off the brink deliberately, and this is common knowledge. By analogy, then, there are no circumstances in which a state would deliberately launch an unrestricted attack, even preemptively. In the brinkmanship analogy, Schelling's "reciprocal fear of surprise attack" (1960, 207-29) plays no role. But if the possibility of falling over the brink is to have any influence at all, there must be some chance of this happening. Since no party can credibly threaten to go over the brink deliberately, it must be possible for the sanction of an unlimited nuclear exchange to be the direct result of an accident. The analogy of brinkmanship places a heavy burden on accident.

Powell (1987) has recently examined the escalatory dynamics of a model of crisis bargaining with incomplete information. The model is presented as an extension of existing models of crisis bargaining in that it explicitly models two essential features of a crisis: a sequence of decisions and the lack of complete information. That model may also be seen as a formalization of Schelling's brinkmanship analogy in which there is only one-sided incomplete information. I will extend that model to include two-sided incomplete information.

Play begins with a potential challenger, I, deciding whether to accept the status quo; dispute the status quo by challenging its adversary, II; or jump off the brink deliberately by launching an unrestricted nuclear attack. If I accepts the status quo,

the game ends with the payoffs  $(q_I, q_{II})$ . Attacking also ends the game and brings the disastrous payoffs  $(d_I, d_{II})$ . If I challenges II, then the onus of escalation shifts to II.

II now has three options, submitting, attacking, or escalating by moving toward the brink. Attacking again ends the game with payoffs  $(d_I, d_{II})$ . Submission ends the game with payoffs  $(w_I, s_{II})$ . The model simplifies the brinkmanship analogy by assuming that the states can move toward the brink in only one way, by increasing the risk of "slipping" by a fixed increment  $f$ . Thus if II escalates, it does so by generating a probability of disaster  $f$ . This is modeled by letting Nature move after II escalates. At this move, Nature imposes an "accidental" disaster with probability  $f$ . Going over the brink, whether accidentally or deliberately, brings a payoff of  $(d_I, d_{II})$ . If there is no disaster, the onus of escalation shifts back to I.

I must now decide if it will submit, attack, or escalate. Attacking, as always, ends the game with payoffs  $(d_I, d_{II})$ . I's submission ends the game with payoffs  $(s_I, w_{II})$ . If I escalates, it must do so by generating a risk of falling over the brink of  $2f$ . Nature then moves by playing disaster with probability  $2f$ . If there is no disaster, the onus of escalation shifts back to II who must now create a risk of  $3f$  if it decides to escalate. The game continues in this way until one state submits or attacks or until there is a disaster.

For each state, the game can end in only one of four ways. The state can prevail, submit, continue with the status quo, or suffer a disaster. Since prevailing is better than submitting and submitting is better than disaster,  $w_I > s_I > d_I$  and  $w_{II} > s_{II} > d_{II}$ . The status quo is also assumed to be better than submitting:  $q_I > s_I$  and  $q_{II} > s_{II}$ . For I, prevailing is better than the status quo:  $w_I > q_I$ . Otherwise, I could obtain its highest payoff by accepting the status quo at the beginning of the game.

There would never be a crisis, for I would never challenge the status quo. The relation between  $w_{II}$  and  $q_{II}$  is uncertain but also irrelevant since the status quo has already been violated by the time that II makes its first decision.

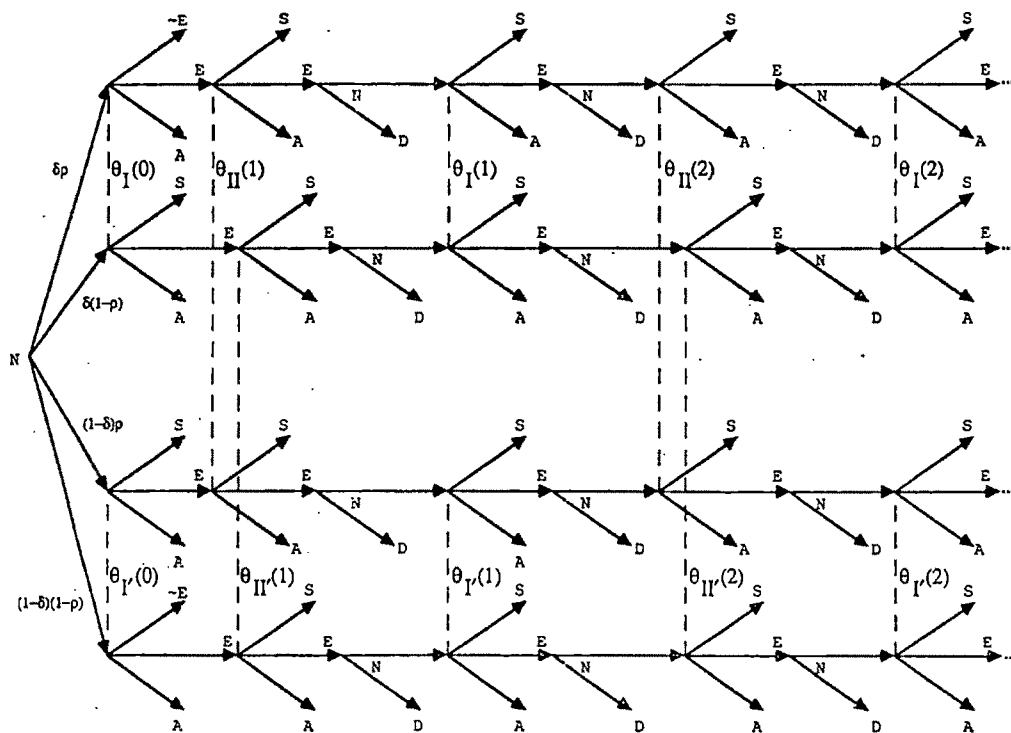
Resolve plays a crucial role in brinkmanship crises. A state's resolve is generally described as the maximum risk of disaster that a state is willing to run in order to prevail (Brams 1985; Jervis 1972; Powell 1987; Snyder 1972; Snyder and Diesing 1977; Wagner 1982). Because in the model a state can submit at any time during the crisis, the maximum risk of disaster that it would be willing to run in order to prevail is the level of risk that makes a state indifferent between submitting and gambling on prevailing. Letting  $R$  denote a state's resolve, then  $(1 - R)w + Rd = s$  and  $R = (w - s)/(w - d)$  where the subscripts have been dropped because the formulae apply to each state.<sup>2</sup>

A state's level of resolve determines an upper bound on the number of times that a state will escalate. To see this, note that the pattern of risks created during the crisis is  $f, 2f, 3f, \dots, Kf$  where  $Kf = 1$  and  $K$  is an integer. As will become clear, it makes no difference whether  $K$  is odd or even, so take  $K$  to be even. It is, however, important to ensure that each state is physically able to escalate at least twice in the game. The means  $4f < 1$ . Since II is the first to generate any risk, the risks it creates as it moves toward the brink follow the pattern  $f, 3f, \dots, (K-1)f$ . But II's resolve,  $R_{II}$ , is the greatest risk that II is willing to run, so the maximum number of times that II might be willing to escalate is the largest integer  $n$  satisfying  $(2n - 1)f \leq R_{II}$ . Let  $N_{II}$  be this integer. Similarly, the maximum number of times that a challenger might escalate by generating some risk of disaster,  $N_I$ , is the largest integer  $n$  that satisfies  $2nf \leq R_I$ .

With two-sided incomplete information, each state is uncertain of the type of its adversary. To simplify matters, there

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Figure 1. Nuclear Brinkmanship with Two-Sided Incomplete Information



are only two possible types of potential challenger, an irresolute and a resolute challenger. Let I denote the irresolute challenger and take  $R_I$  to be such that  $N_I = 1$ , that is,  $2f \leq R_I \leq 4f$ . To avoid some future technicalities, strict inequality will be assumed:  $2f < R_I < 4f$ . The resolve of the resolute challenger,  $R'$ , is such that  $N_{R'} \geq 2$  which means  $4f < R_{R'}$ . There are also only two possible types of challenged state. Again, one of these is resolute and the other is irresolute. II is irresolute with  $f < R_{II} < 3f$  so that  $N_{II} = 1$ , and  $II'$  is resolute with  $3f < R_{II'} < 5f$  so that  $N_{II'} = 2$ . Finally, the probability of a potential challenger being irresolute is  $\delta$  and the probability that its adversary is irresolute is  $q$ . These probabilities are common knowledge.

The game's extensive form is depicted in Figure 1. The game begins with Nature

selecting the types of the states. After these have been determined, play proceeds as described above with the onus of escalation shifting back and forth until the game ends. The options of *not exploiting* the situation, *exploiting* (or *escalating*), *attacking*, *submitting*, or, for Nature, imposing *disaster* are denoted by  $\sim E$ ,  $E$ ,  $A$ ,  $S$ ,  $D$ , respectively.

It will prove convenient to adopt the following notation for the states' information sets and behavioral strategies: Let  $\theta_{II}(n)$  be the information set at which II must decide whether or not to escalate for the  $n$ th time where  $1 \leq n \leq K/2$ . Thus  $\theta_{II}(n)$  presents II with the choice of submitting, attacking, or escalating by creating an autonomous risk of  $(2n - 1)f$ . (Recall that since the onus of escalation shifts back and forth, II only makes bids of odd multiples of  $f$ .)  $\theta_{II'}(n)$  is defined in exactly

the same way for  $I'$ . For  $I$ , take  $\theta_I(n)$  for  $1 \leq n \leq K/2$  to be the information set at which  $I$  must decide to submit, attack, or escalate with probability  $2nf$ .  $\theta_{II}(0)$  is the information set at which  $I$  has to choose among attacking, exploiting the situation, or not exploiting it.  $\theta_{II'}(n)$  is defined similarly for  $I'$ .

A similar notation will be employed for a state's beliefs about the type of adversary it is facing at any given information set. Take an arbitrary information set, say  $\theta_{II}(n)$ . Then,  $II$  believes that it is facing an irresolute adversary at  $\theta_{II}(n)$  with probability  $\beta_{II}(n)$  and a resolute adversary with probability  $1 - \beta_{II}(n)$ .

At each of its information sets, a state has a choice among three alternatives. Accordingly, a behavioral strategy for any state at any information set is defined by two probabilities. At, say,  $\theta_{II}(n)$ ,  $II$ 's strategy may be described by the probability of attacking at that set,  $a_{II}(n)$ , and the probability of escalating,  $e_{II}(n)$ . The probability of submission is given by  $1 - a_{II}(n) - e_{II}(n)$ .

### The Sequential Crisis Equilibria

This section presents the game's sequential crisis equilibria. In a sequential equilibrium, a state's equilibrium strategy starting from any place in the game is an optimal strategy for the rest of the game given the state's beliefs and given the other states' strategies. That is, no state can improve its payoff by deviating from its equilibrium strategy anywhere in the game.<sup>3</sup> Thus in a sequential equilibrium no state can rely on a threat to impose a sanction when actually imposing the sanction is expected to be more costly than not doing so. In this way, sequential equilibria avoid the credibility problems inherent in doctrines like massive retaliation.

For there to be a crisis, there must be a challenge and it must be resisted (Snyder

and Diesing 1977, 13). Accordingly, this analysis will focus only on equilibria in which there is some chance of a resisted challenge. Formally, it must be that  $e_I(0) > 0$  or  $e_{I'}(0) > 0$  and that  $e_{II}(1) > 0$  or  $e_{II'}(1) > 0$ . Equilibria satisfying these conditions will be called crisis equilibria.

Before characterizing the game's sequential crisis equilibria, two remarks are in order. First, note that no state will ever place a positive probability on attacking in a sequential equilibrium. If it did, then this state could improve its payoff by deviating from its strategy by submitting rather than attacking. This, however, is inconsistent with the definition of a sequential equilibrium. Thus, a state's strategy at any information set can be characterized by the single probability of the state escalating at this set.

The second remark simplifies the model further and facilitates the search for the game's equilibria by evading some of the difficulties of deciding what are reasonable conjectures to hold off the equilibrium path. The simplification is to ensure that the resolve of  $I'$  is so great that it is certain to escalate at  $\theta_{I'}(0)$  and  $\theta_{I'}(1)$  regardless of what its adversary does. This assumption further constrains  $N_{I'}$ , which has already been assumed to satisfy  $N_{I'} \geq 2$ . This constraint will be specified when the game's equilibria are derived. For now, simply assume  $e_{I'}(0) = 1$  and  $e_{I'}(1) = 1$ .

**PROPOSITION 1.** *In the game with two-sided incomplete information, there are four generic types of potential sequential crisis equilibrium. Generically, only one of these equilibria can exist at a time and which one, if any,<sup>4</sup> exists depends on the initial beliefs of the states, that is, on the values of  $\delta$  and  $\rho$ .*

Equilibrium 1 exists only if

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$$\delta > 1 - \left( \frac{R_{II} - f}{(1-f)(1-2f)R_{II} + 2f} \right) \left( \frac{R_{II} - 3f}{(1-3f)(1-4f)R_{II} + 4f} \right)$$

$$e < 1 - \left( \frac{w_1 - q_1}{w_1 - d_1} \right) \frac{1}{(1-f)R_1 + f}$$

with strategies

$$e^*_{I(0)} = \left( \frac{1-\delta}{\delta} \right) \left( \frac{1}{1-\beta^*_{II(2)}} \right) \left( \frac{[1-(1-f)(1-2f)](1-R_{II})}{R_{II}-f} + \beta^*_{II(2)} \right)$$

$$e^*_{I(1)} = [\beta^*_{II(2)} (R_{II} - f) / \{ [1 - (1-f)(1-2f)(1-R_{II})] + \beta^*_{II(2)} (R_{II} - f) \}]$$

$$e^*_{I(n)} = 0 \quad \forall n \geq 2$$

$$e^*_{II(n)} = 0 \quad \forall n \geq 1$$

$$e^*_{II(1)} = \left( \frac{1}{1-e} \right) \left( \frac{w_1 - q_1}{w_1 - d_1} \right) \frac{1}{(1-f)R_1 + f}$$

$$e^*_{II(2)} = \frac{R_1 - 2f}{(1-2f)[(1-3f)R_1 + 3f]}$$

$$e^*_{II(n)} = 0 \quad \forall n \geq 3$$

$$e^*_{I(n)} = 1 \quad \forall n \leq N_I$$

$$e^*_{I(n)} = 0 \quad \forall n > N_I$$

and with the relevant<sup>5</sup> beliefs

$$\beta^*_{I(0)} = \beta^*_{I(0)} = e$$

$$\beta^*_{I(1)} = \beta^*_{I(1)} = 0$$

$$\beta^*_{I(2)} = \beta^*_{I(2)} = 0$$

$$\beta^*_{II(1)} = \beta^*_{II(1)} = [\delta e^*_{I(0)}] / [1 - \delta(1 - e^*_{I(0)})]$$

$$\beta^*_{II(2)} = \beta^*_{II(2)} = \frac{[1 - (1-3f)(1-4f)](1-R_{II})}{(1-3f)(1-4f)R_{II} + 4f}$$

Equilibrium 2 exists only if

$$\delta > 1 - \left( \frac{R_{II} - f}{(1-f)(1-2f)R_{II} + 2f} \right) \left( \frac{R_{II} - 3f}{(1-3f)(1-4f)R_{II} + 4f} \right)$$

$$1 - [(w_1 - q_1)/(w_1 - d_1)] [(R_1 - 2f) / \{(1-2f)[(1-3f)R_1 + 3f]\}] \{1/[1-f)R_1 + f]\} > e > 1 - [(w_1 - q_1)/(w_1 - d_1)] \{1/[(1-f)R_1 + f]\}$$

with strategies

$$e^*_{I(0)} = \frac{1-\delta}{\delta} \cdot \frac{1}{1-\beta^*_{II(2)}} \left( \frac{[1-(1-f)(1-2f)](1-R_{II})}{R_{II}-f} + \beta^*_{II(2)} \right)$$

$$e^*_{I(1)} = [\beta^*_{II(2)} (R_{II} - f) / \{ [1 - (1-f)(1-2f)] (1 - R_{II}) + \beta^*_{II(2)} (R_{II} - f) \}]$$

$$e^*_{I(n)} = 0 \quad \forall n \geq 2$$

$$e^*_{II(1)} = 1 - \frac{1}{e} \left[ 1 - \left( \frac{w_1 - q_1}{w_1 - d_1} \right) \frac{1}{(1-f)R_1 + f} \right]$$

$$e^*_{II(n)} = 0 \quad \forall n \geq 2$$

$$e^*_{II(1)} = 1$$

$$e^*_{II(2)} = \left( \frac{1}{1-e} \right) \left( \frac{w_1 - q_1}{w_1 - d_1} \right) \left( \frac{1}{(1-f)R_1 + f} \right) \left( \frac{R_1 - 2f}{(1-2f)[(1-3f)R_1 + 3f]} \right)$$

$$e^*_{II(n)} = 0 \quad \forall n > 2$$

$$e^*_{I(n)} = 1 \quad \forall n \leq N_I$$

$$e^*_{I(n)} = 0 \quad \forall n > N_I$$

and with the relevant beliefs

$$\beta^*_{I(0)} = \beta^*_{I(0)} = e$$

$$\beta_I^*(1) = \beta_I^*(1) = 1$$

$$- (1 - q) \left( \frac{w_1 - q_1}{w_1 - d_1} \right) \frac{1}{(1 - f)R_1 + f}$$

$$\beta_I^*(2) = \beta_I^*(2) = 0$$

$$\beta_{II}^*(1) = \beta_{II}^*(1) =$$

$$\{[1 - (1 - f)(1 - 2f)](1 - R_{II})$$

$$+ \beta_{II}^*(2)(R_{II} - f)\} /$$

$$\{(1 - f)[(1 - 2f)R_{II} + 2f]\}$$

$$\beta_{II}^*(2) = \beta_{II}^*(2) =$$

$$\frac{[1 - (1 - 3f)(1 - 4f)](1 - R_{II})}{(1 - 3f)[(1 - 4f)R_{II} + 4f]}$$

$$\beta_{II}^*(3) = \beta_{II}^*(3) = 0$$

Equilibrium 3 exists only if

$$\delta > 1 - \left( \frac{R_{II} - f}{(1 - f)[(1 - 2f)R_{II} + 2f]} \right)$$

$$\left( \frac{R_{II} - 3f}{(1 - 3f)[(1 - 4f)R_{II} + 4f]} \right)$$

$$q > 1 - \left( \frac{w_1 - q_1}{w_1 - d_1} \right)$$

$$\left( \frac{R_1 - 2f}{(1 - 2f)[(1 - 3f)R_1 + 3f]} \right)$$

$$\left( \frac{1}{(1 - f)R_1 + f} \right)$$

with strategies

$$e_I^*(0) = 1$$

$$e_I^*(1) = \frac{R_{II} - f}{(1 - f)[(1 - 2f)R_{II} + 2f]} -$$

$$\left( \frac{1 - \delta}{\delta} \right) \frac{[1 - (1 - f)(1 - 2f)](1 - R_{II})}{(1 - f)[(1 - 2f)R_{II} + 2f]}$$

$$e_I^*(n) = 0 \quad \forall n \geq 2$$

$$e_{II}^*(1) = \left( \frac{1 - q}{q} \right) \left( \frac{\beta_I^*(1)}{1 - \beta_I^*(1)} \right)$$

$$e_{II}^*(n) = 0 \quad \forall n \geq 2$$

$$e_{II}^*(1) = 1$$

$$e_{II}^*(2) = 1$$

$$e_{II}^*(n) = 0 \quad \forall n \geq 3$$

$$e_I^*(n) = 1 \quad \forall n \leq N_I$$

$$e_I^*(n) = 0 \quad \forall n > N_I$$

and with the relevant beliefs

$$\beta_I^*(0) = \beta_I^*(0) = q$$

$$\beta_I^*(1) = \beta_I^*(1) =$$

$$\frac{[1 - (1 - 2f)(1 - 3f)](1 - R_I)}{(1 - 2f)[(1 - 3f)R_I + 3f]}$$

$$\beta_I^*(2) = \beta_I^*(2) = 0$$

$$\beta_{II}^*(1) = \beta_{II}^*(1) = \delta$$

$$\beta_{II}^*(2) = \beta_{II}^*(2) = \frac{\delta e_I^*(1)}{1 - \delta[1 - e_I^*(1)]}$$

$$\beta_{II}^*(3) = \beta_{II}^*(3) = 0$$

Equilibrium 4 exists only if

$$1 - \left( \frac{R_{II} - f}{(1 - f)[(1 - 2f)R_{II} + 2f]} \right)$$

$$\left( \frac{R_{II} - 3f}{(1 - 3f)[(1 - 4f)R_{II} + 4f]} \right) > \delta >$$

$$\frac{[1 - (1 - 3f)(1 - 4f)](1 - R_{II})}{(1 - 3f)[(1 - 4f)R_{II} + 4f]}$$

$$q > 1 - \left( \frac{w_1 - q_1}{w_1 - d_1} \right) \frac{1}{(1 - f)R_1 + f}$$

with strategies

$$e_I^*(0) = 1$$

$$e_I^*(1) = \left( \frac{1 - \delta}{\delta} \right)$$

$$\frac{[1 - (1 - 3f)(1 - 4f)](1 - R_{II})}{R_{II} - 3f}$$

$$e_I^*(n) = 0 \quad \forall n \geq 2$$

$$e_{II}^*(n) = 0 \quad \forall n \geq 1$$

$$e_{II}^*(1) = \left( \frac{1}{1 - q} \right) \left( \frac{w_1 - q_1}{w_1 - d_1} \right)$$

$$\left( \frac{1}{(1 - f)R_1 + f} \right)$$

$$e_{II}^*(2) = \frac{R_1 - 2f}{(1 - 2f)[(1 - 3f)R_1 + 3f]}$$



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$$e^*_{II}(n) = 0 \quad \forall n \geq 2$$

$$e^*_{II}(n) = 1 \quad \forall n \leq N_I$$

$$e^*_{I'}(n) = 0 \quad \forall n > N_I$$

and with the relevant beliefs

$$\beta^*_{I'}(0) = \beta^*_{I'}(0) = q$$

$$\beta^*_{I'}(1) = \beta^*_{I'}(1) = 0$$

$$\beta^*_{I'}(2) = \beta^*_{I'}(2) = 0$$

$$\beta^*_{II}(1) = \beta^*_{II}(1) = \delta$$

$$\beta^*_{II}(2) = \beta^*_{II}(2) = [1 - (1 - 3f)(1 - 4f)] \\ \left( \frac{1 - R_{II}}{(1 - 3f)[(1 - 4f)R_{II} + 4f]} \right)$$

The four equilibria described in Proposition 1 and derived in the Appendix share the same general pattern of crisis bargaining. In each equilibrium, each state begins with an initial belief about the type of its adversary. As the crisis unfolds, each state revises its belief in light of the strategies of its potential adversary. Generally, the longer the crisis lasts, the more confident a state becomes that it is facing a resolute adversary. Assuming that the states avoid accidentally falling over the brink and suffering the disaster of an unlimited nuclear exchange, then eventually one of the states finds the risk of continuing too great given its current belief about the type of its adversary and its strategy. This state then submits.

Although the four equilibria share the same general pattern of bargaining, there are specific differences among them. These differences are due to the different initial beliefs, that is, to the different values of  $\delta$  and  $q$ . First, compare Equilibria 1, 2, and 3. In all three,  $\delta$ , which is the probability of an irresolute potential challenger, must be relatively high, and it is possible to satisfy the constraint on  $\delta$  in all three equilibria simultaneously. It is, however, impossible for any value of  $q$ , which is the probability of the challenged

state being irresolute, to satisfy more than one of these constraints. For small  $q$ , Equilibrium 1 exists. For intermediate  $q$ , Equilibrium 2 exists. For large  $q$ , Equilibrium 3 exists.

The effects of variations in  $q$  are most immediately evident in the irresolute potential challenger's decision of whether or not to exploit the situation. The larger  $q$ , the more confident I is that it is facing an irresolute adversary and the more likely it is to challenge the status quo. That is,  $e^*_{I'}(0)$  is smallest in Equilibrium 1, larger in Equilibrium 2, and largest in Equilibrium 3.

If there is a challenge, then, because I is least likely to escalate in Equilibrium 1, II is the most confident in this equilibrium that the challenger is resolute. Indeed, II is so confident that it is facing I' that it never escalates:  $e^*_{II}(0) = 0$  for all  $n$ . Since II never escalates, a challenger in Equilibrium 1 is certain that it is facing a resolute adversary if its challenge is resisted. Thus, I is certain that it is facing II' if I's challenge is resisted. But although certain that its adversary is resolute, I does not necessarily submit. Rather, I escalates with probability  $e^*_{I'}(1)$ . The justification for this seemingly irrational act is that II' is uncertain of the challenger's resolve. In fact, II' is so confident that it is facing a resolute challenger, the chances that it will submit are sufficiently large that it is worth it to I to exploit II's uncertainty by escalating. So, I escalates although certain that its adversary's resolve is greater than its own.

In Equilibrium 2, II is more confident that it is confronting an irresolute challenger and now may resist a challenge by escalating with probability  $e^*_{II}(1)$ . I in turn is more confident that it is facing an irresolute adversary if its challenge is resisted. Nevertheless, I is more likely to meet resistance with escalation in Equilibrium 1 where it is certain that it is facing a resolute adversary than in Equilibrium 2 where it is more confident that it is con-

fronting an irresolute adversary. This is explained by the fact that  $II'$  is more likely to reply to a challenger's escalation by escalating a second time in Equilibrium 2. Thus a state may be more confident that it is facing an irresolute adversary yet be less likely to escalate.

In Equilibrium 3,  $q$  is so high and  $I$  is so confident that it is facing an irresolute adversary that  $I$ , like  $I'$ , is certain to challenge. Because  $I$  and  $I'$  follow the same strategy in deciding whether to exploit the situation, a challenged state learns nothing about the resolve of its adversary from the initial strategy of the challenger. A challenge reveals no information about the type of the challenger.

Because  $I$  is most likely to challenge in this equilibrium, its adversary is most confident that it is facing  $I$ . Indeed,  $II'$  is so confident that it is facing an irresolute challenger that it is certain to escalate for all  $n \leq N_{II'}$ . Revealing nothing about its type does, however, bring a balancing advantage for  $I$ . With large  $q$ ,  $II$  is less likely to escalate in this equilibrium than in Equilibrium 2. That is, in Equilibrium 3  $e^*_{II}(1)$  is smaller than in Equilibrium 2 where  $I$ 's strategy is not identical to that of  $I'$  and  $I$  reveals some information about its type.

Finally, consider Equilibrium 4 by comparing it to Equilibrium 3. Both equilibria require  $q$  to be large. But the probability of an irresolute potential challenger,  $\delta$ , is smaller in Equilibrium 4 than it was in Equilibrium 3. Because  $\delta$  is smaller in the former than the latter and  $I$  is certain to challenge in both equilibria, the challenged state is less confident of facing an irresolute challenger in the former. In fact,  $II'$  was so confident that its adversary in Equilibrium 3 was  $I$  that it was certain to escalate for all  $n \leq N_{II'}$ . Being less confident of facing  $I$  in Equilibrium 4,  $II'$  is throughout the crisis less likely to escalate than in Equilibrium 3 where  $II'$  was certain to escalate for all  $n \leq N_{II'}$ .

### Resolve, Misperception, the Status Quo and the Dynamics of Escalation

I now examine the effects of changes in the level of resolve, the degree of misperception, and the value of the status quo on crisis stability and the dynamics of escalation. The analysis is complicated by the existence of multiple equilibria that tend to mirror each other. For example, an increase in the resolve of  $I$  due to a higher return to prevailing, that is, to an increase in  $w_I$ , makes  $II$  more likely to escalate at  $\theta_{II}(1)$  in Equilibrium 2 but less likely to escalate in Equilibrium 3. Nevertheless, the four equilibria do support some generalizations. The characterization of a brinkmanship crisis as a contest of resolve is misleading. Moreover, reducing the level of misperception may reduce, not increase, crisis stability. Finally, a small increase in the irresolute potential challenger's stake in the status quo does not make a challenge to the status quo less likely.

Before taking up this examination, a preliminary observation is in order. Although each state is uncertain of its adversary's resolve and this is modeled by letting Nature begin the game by randomly selecting the levels of resolve, the phenomenon being modeled is a crisis in which each state's resolve, while unknown to its adversary, is fixed. Viewed in this way, Proposition 1 describes the escalatory dynamics of four different types of crisis. These types are described by the types of state that are actually facing each other in the crisis. In, for example, a crisis in which both states are actually irresolute, escalation follows the dynamic specified by the strategies of  $I$  and  $II$  in Proposition 1. If, however, the actual crisis involved two resolute states, then the interaction of the strategies of  $I'$  and  $II'$  would determine the pattern of

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escalation. Let  $(I, II)$ ,  $(I, II')$ ,  $(I', II)$  and  $(I', II')$  denote these crises.

Although there are four distinct types of crisis and each has its own escalatory dynamic, it is important to realize that these types of crisis cannot be separated completely. They are linked by the states' beliefs. For example, an actual crisis of type  $(I, II)$  is linked to  $(I, II')$  because the actual challenger, I, is uncertain of the type of its adversary. I does not know if it is facing II or  $II'$ . That is, I does not know if it is in  $(I, II)$  or  $(I, II')$ . Similarly, the beliefs of II link the actual crisis  $(I, II)$  to  $(I', II)$ . This linkage means that a crisis cannot be described solely in terms of the resolve or, more generally, the types of state in the crisis. The states' beliefs about the resolve or the potential type of their adversary are an integral part of the description of a crisis.

### Crisis Stability

The existence of different types of crisis naturally leads to questions about their relative stability. Intuition suggests that a crisis in which both states are resolute should be more likely to end in war and hence be less stable than a crisis in which only one state is resolute. Furthermore, a crisis in which one state is resolute should be less stable than a crisis in which neither state is resolute. As will be shown, the model supports this intuition.

Crisis stability is usually taken to be a measure of the likelihood of a crisis ending in war. The more stable the crisis, the less likely war. This idea is easily formalized in the model. For example, the probability of, say, a crisis of type  $(I, II')$  ending in war is  $P_W((I, II')) = f + (1 - f)e^*_{I'}(1)[2f + (1 - 2f)e^*_{II'}(2)3f]$ . The probability of a crisis not ending in disaster is  $1 - P_W$ , and this will be taken as the measure of crisis stability.

$P_W$  is calculated from the point at which Nature first has a chance to play disaster. At this point, there is a crisis:

there has been a resisted challenge. Calculating  $P_W$  in this way raises an issue that will become important later but should be introduced here because it affects the way that  $P_W$  is calculated. The probability of a situation of type  $(I, II')$  ending in disaster would not be calculated from where Nature first has a chance to impose disaster. Rather, this probability would be calculated from the point at which a potential challenger is deciding if it will exploit the situation. For I, this probability would be calculated from  $\theta_I(0)$  and would equal  $e^*_{I'}(0)e^*_{II'}(1)P_W$ . Letting  $S_W$  denote this probability, then  $1 - S_W$  measures "situational" stability, which is the probability of a situation not escalating to disaster. In general, situational stability will not equal crisis stability and, more importantly, changes in the model's parameters that, for example, increase situational stability may not increase crisis stability. Thus, the distinction between crisis stability and situational stability or, more generally, the distinction between statements made about a historical sample that only includes crises and statements about a historical sample that includes situations that could have become crises but did not is sometimes crucial. Indeed, this distinction will play an important role in the discussion of the effects of misperception on stability. However, the following conclusions about the relative stability of the four types of crisis hold for both crisis and situational stability.

The crises may be ranked by noting that a resolute state is always at least as likely to escalate and sometimes more likely to escalate than its irresolute counterpart. Thus,  $P_W((I', II')) > P_W((I', II)) > P_W((I, II))$  and  $P_W((I', II')) > P_W((I, II')) > P_W((I, II))$ . Then, a severe crisis that entails a grave conflict of interest because both sides are resolute is more dangerous and less stable than a crisis in which only one state is resolute. A minor crisis in which neither state is resolute is less dangerous and more stable than one in which one or

both states are resolute. The model agrees with the intuitive ranking of these crises by their relative stability.

### Resolve

Several of the conclusions about the effects of changes in the states' levels of resolve suggested by models based on critical risks do not hold up when nuclear brinkmanship is explicitly modeled as a well-defined game of sequential bargaining with one-sided incomplete information (Powell 1987). Jervis argues that in a contest of resolve, the state "willing to run the greatest risks will prevail" (1979b, 631). Moreover, critical-risk models suggest that the greater a state perceives its adversary's resolve to be, the less likely the state is to escalate (Ellsberg 1959; Jervis 1972, 1978; Snyder 1971, 1972; and Snyder and Diesing 1977). Neither of these conclusions holds with one-sided incomplete information. Nor do they hold with two-sided incomplete information. Indeed, recall that I in Equilibrium 1 is certain that it is facing a more resolute adversary if its challenge is resisted. Nevertheless, I exploits its adversary's uncertainty about the resolve of the challenger by escalating with probability  $e^*_I(1)$ . I, certain that its adversary's resolve is greater, is playing a strategy of pure bluff. But in this crisis, I will prevail with probability  $e^*_I(1)(1 - f)[1 - e^*_{II}(1)]$ . Sometimes bluffing works and the state with the least resolve prevails. To see that an increase in an adversary's resolve may make a state more, not less, likely to escalate, note that in Equilibria 2 and 3  $\partial e^*_I(1)/\partial R_{II} > 0$ . In a (I,II)-crisis in these equilibria, an increase in the resolve of I's adversary makes I more likely to escalate.

The richer bargaining dynamic permitted by two-sided incomplete information also reveals something new about the effects of changes in the level of resolve. Note that in Equilibrium 3,  $\partial e^*_I(0)/\partial R_{II} <$

0 but  $\partial e^*_I(1)/\partial R_{II} > 0$ . Hence, the effects of changes in an adversary's resolve may differ depending on the stage of the crisis. An increase in II's resolve in a (I,II)-crisis in this equilibrium makes I initially less likely to exploit the situation. But if I overcomes its greater initial reluctance to dispute the status quo, I pursues its challenge more tenaciously in that it is more likely to escalate if its reluctantly made challenge is resisted.

The remaining discussion of the effects of changes in the level of resolve shows that describing a brinkmanship crisis as a contest of resolve is misleading. Many propositions that intuitively seem to follow from this description turn out to be incorrect when examined formally. One of these propositions has already been noted. Although the notion of a contest of resolve suggests that the state with the greatest resolve will prevail, this is incorrect.

Another proposition that might easily be inferred from this description is that a state would be more likely to prevail, the greater its resolve. But consider a (I,II)-crisis in Equilibria 2 and 3. The probability of II prevailing is  $(1 - f)[1 - e^*_I(1)]$ . This probability decreases as  $R_{II}$  increases. Thus, II is less likely to prevail the greater its resolve.

Still another proposition that might loosely be derived from the description of brinkmanship as a contest of resolve is that the greater a state's resolve, the greater its expected payoff should be. Being more resolute may not help in any given instance, but it should help on average. This proposition is also incorrect. Consider I's expected payoff in a (I,II)-crisis in Equilibrium 3. This is given by

$$fd_1 + (1 - f)[(1 - e^*_I(1))s_1 + e^*_I(1)\{2fd_1 + (1 - 2f)[3fd_1 + (1 - 3f)s_1\}].$$

Recalling that  $R_I = (w_1 - s_1)/(w_1 - d_1)$ ,

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suppose that the resolve of I is greater because  $s_I$  is smaller. Since I's expected payoff decreases as  $s_I$  decreases, its expected payoff decreases as its resolve increases.

Perhaps the least demanding proposition suggested by the notion of a contest of resolve is that the states' strategies should be functions of their levels of resolve. That is, the states' levels of resolve summarize enough information about the payoff structure of the situation to determine their strategies.<sup>5</sup> But even this is not the case. In Equilibrium 2, for example,  $e^*_{II}(1)$  and  $e^*_{II}(2)$  depend on a combination of the payoffs of I that cannot be expressed as a function of the states' levels of resolve. In the contest of resolve, resolve does not even fully describe the dynamics of the contest.

In sum, many propositions that at first seem to follow from the description of a contest of resolve do not follow after a closer examination. Describing a brinkmanship crisis as a contest of resolve may obscure more than it clarifies.

### Misperception and Crisis Stability

In the model, the situation actually facing a state can be distinguished from the situation that the state believes itself to be facing. Indeed, it is possible to specify the degree to which a state misperceives its situation. Suppose, for example, that I and II are actually facing each other. At  $\theta_I(0)$ , I believes that it is facing II with probability  $q$ . Since I is actually facing II, the strength of its belief that it is facing II, that is, the value of  $q$ , measures the degree of I's misperception about the type of its adversary. Conversely, had I actually been facing II, then  $1 - q$  would measure the degree of misperception. Given the types of adversaries actually facing each other, the probabilities  $\delta$  and  $q$  measure their initial misperceptions. As the crisis escalates, each state revises its beliefs about its adversary in light of its

actions and this affects the degree of misperception.

An important qualification to this formulation of misperception must be noted before the effects of misperception are examined. The probabilities  $\delta$  and  $q$  are assumed to be common knowledge. Thus each state knows how badly its adversary misperceives it. For example, I, knowing its type, knows that its adversary's degree of misperception is  $1 - \delta$ , and it knows that its adversary knows this. This assumed knowledge of the other's degree of misperception may be a very serious limitation of this formulation.

If misperception could be completely eliminated, each state would know its adversary's resolve. There would be complete information and no crises (Powell 1987). But what if misperception is reduced but not eliminated? Table 1 summarizes the effects of small reductions in the degree of misperception on crisis and situational stability. (Remember that crisis stability is a measure of the probability of the game ending in disaster once there has been a resisted challenge, whereas situational stability is the probability of this outcome once Nature has selected the states' types but before the potential challenger decides whether or not to dispute the status quo.) These effects are derived by evaluating the sign of the partial derivatives of the probability of war with respect to  $\delta$  and  $q$ . Suppose, for example, I and II are facing each other.  $P_W$  is the probability of a crisis of this type ending in war, and  $P_W = f + (1 - f)e^*_{I(1)}2f$ . The probability of the situation ending in war,  $S_W$ , is  $e^*_{I(0)}e^*_{II(1)}P_W$ . Now,  $\partial S_W / \partial q > 0$  in Equilibrium 2. That is, the situation becomes less stable as  $q$  increases. I, moreover, believes that it is facing II with probability  $q$ . As  $q$  increases, misperception decreases. The situation thus becomes less stable as misperception decreases, and a "-" appears in the cell in the top row under Equilibrium 2 and *Situ-*

Table 1. The Effects of Reducing Misperception on Crisis and Situational Stability

Type of Crisis	Type of Equilibrium							
	1		2		3		4	
	Situation	Crisis	Situation	Crisis	Situation	Crisis	Situation	Crisis
I	0	0	—	0	+	0	0	0
II	0	0	+	0	—	—	0	0
I'	+	0	—	—	0	0	+	0
II'	+	0	+	0	—	—	—	—
I''	0	0	—	0	+	0	0	0
II''	0	0	0	0	0	0	0	0
I'''	+	0	+	+	0	0	+	0
II'''	0	0	0	0	0	0	0	0

ation. The other cells are evaluated in the same way: "+" denotes an increase in stability due to a decrease in misperception; "—" denotes a decrease in stability; and 0 denotes no effect.

Table 1 shows that except for I', reducing a state's misperception can be destabilizing as well as stabilizing. The effects of more accurate perceptions depend very much on the situation that is perceived more clearly.

An overdrawn example demonstrates how reducing misperceptions can reduce stability by making war more likely. Suppose that a potential challenger is actually facing an irresolute adversary. But this state also misperceives the resolve of its adversary and is very confident that its adversary is resolute. Given this misperception, the state will not challenge or resist its adversary. There will be no crisis and no chance of war. If, however, the state more accurately perceives its adversary's low resolve, the state may challenge or resist its adversary. This creates a crisis and some chance of war. More accurate perceptions in this case have reduced stability.

### The Status Quo

Detente is in part based on the belief that the greater the stake a potential challenger has in the status quo, the less likely this state is to put this more valuable stake at risk by challenging the status quo. Although this is an assertion about foreign policy and not about crisis bargaining, it has an obvious parallel in crisis bargaining. In the model, this parallel does not hold. An increase in the value of the status quo to the potential challenger has a more complicated effect on the escalatory dynamics. An increase in the challenger's stake in the status quo does make a crisis less likely. But at least for small increases in the value of the status quo, a potential challenger is not less likely to make a challenge. Rather, the challenged state is less likely to resist by escalating.

To see that an increase in the value of the status quo makes a crisis less likely, consider first the effect of a large increase in the value of the status quo in Equilibria 2 and 4. For these equilibria to exist  $q > 1 - (w_1 - q_1)/\{(w_1 - d_1)[(1 - f)R_1 + f]\}$ . For any value of  $q$  less than one that satis-

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fies this relation, there exists a new, larger value of  $q_1$  for which this relation does not hold and for which these equilibria do not exist. A similar argument shows that if  $q_1$  is large enough, Equilibrium 3 does not exist.

But now consider the effects of a smaller increase in  $q_1$  on the escalatory dynamics of any one of the equilibria. The strategies  $e^*_{I}(0)$  and  $e^*_{I}(1)$  do not depend on  $q_1$ . Changes in  $q_1$  do not affect I's strategy. An increase in the value of the status quo to I does not make I less likely to challenge the status quo. The strategies that may be affected by changes in  $q_1$  are  $e^*_{II}(1)$ ,  $e^*_{II}(1)$ , and  $e^*_{II}(2)$ . When they are, an increase in  $q_1$  makes II and II' less likely to escalate if challenged. Crises thus becomes less likely but only because resistance is less likely.

The intuition behind this counter-intuitive result is that an increase in the value of a potential challenger's stake in the status quo affects the bargaining dynamics of the crisis through two opposing influences. The first is the belief noted above that the more valuable the stake in the status quo, the more a challenger must risk by disputing the status quo. This influence would seem to make a challenge less likely.

The second, opposing influence is more subtle. The heart of a brinkmanship crisis is each state's attempt to convince its adversary that it is resolute. This is what drives escalation. But now suppose that a state, uncertain of a potential challenger's resolve, knows that the potential challenger's stake in the status quo is large. Then there is a challenge. What is the challenged state to infer from a challenge that put a more valuable stake at risk? A reasonable inference would be that the challenger is more determined to have its way than it would have seemed if it had only had to risk a less valuable stake. That is, by risking a more valuable stake, a challenger signals greater determination and greater resolve. Concluding that the

challenger's resolve is greater, the challenged state is less likely to escalate and more likely to submit. In essence, a potential challenger can use its adversary's knowledge that the status quo is more valuable to the challenger as a bargaining lever. With this lever, a challenger can convey an impression of greater resolve to its adversary by disputing the more valuable status quo.

These two influences oppose each other. In the model, the latter dominates for relatively small changes in the value of the status quo. But the precise balance in the model is less important than that the model has exposed an influence that, once seen, seems intuitively plausible and yet has not been appreciated previously.

### The Distribution of Crises

Jervis has argued that one consequence of the nuclear revolution is that crises "should be in peripheral areas where neither side's stake is very high" (1986, 695). The model provides some weak support for the argument that the states' stakes in a brinkmanship crisis will be small. In the model, the probability of the challengers in a crisis being resolute is less than the probability of the challengers being resolute. Moreover, a crisis in which both states are resolute is less likely than crises in which at least one state is irresolute.

Showing this requires a reinterpretation of the meaning of  $\delta$  and  $q$ . Until now a crisis has been assumed to be of a given type and  $\delta$  and  $q$  measured the states' uncertainty about the actual type of crisis. Suppose instead that  $\delta$  and  $q$  are measures of the distribution of interests in the international system so that the probability of a situation of type (I,II) is  $\delta q$ , of (I,II') is  $\delta(1 - q)$ , of (I',II) is  $(1 - \delta)q$ , and of (I',II') is  $(1 - \delta)(1 - q)$ .

The demonstration that the probability of a challenger being irresolute is greater

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But the model contradicts the claims that the state with the greatest resolve prevails or that the greater an adversary's resolve, the less likely a state is to escalate. Indeed, for an irresolute potential challenger, an increase in its adversary's resolve may make the potential challenger less likely to dispute the status quo. But if it does challenge the status quo, it is more likely to meet resistance with escalation.

The analysis also shows that describing a brinkmanship crisis as a contest of resolve is misleading. Contrary to what might be presumed to follow from this description, a state may be less, not more, likely to prevail, the greater its resolve. A state's expected payoff may be less, not more, the greater its resolve. In fact, it is not even possible to express the state's strategies as functions of the states' levels of resolve. Describing a brinkmanship crisis as a contest of resolve may obscure as much as it clarifies.

Because the situation actually facing a state may be distinguished from the situation a state believes itself to be facing, certain aspects of the role of misperception in crisis bargaining may be formalized. Although there would be no crises if misperception could be completely eliminated, reducing, but not eliminating, misperception can be stabilizing or destabilizing.

Increasing a potential challenger's stake in the status quo would seem to reduce the likelihood of this state disputing the status quo, for to do so would be to risk a more valuable stake. The model showed this assertion to be problematic. A greater stake in the status quo may be used by the challenger as a bargaining lever against its adversary to convey an impression of greater resolve to the adversary. The result is that a potential challenger is not less likely to challenge the status quo. But a challenged state is more likely to submit.

Some weak statements about the distribution of crises may also be made.

Given a crisis, a challenger is, surprisingly, more likely to be irresolute than resolute. A severe crisis is, moreover, less likely than a crisis that is not severe.

In an examination of the bargaining that takes place between a buyer and seller, Fudenberg and Tirole "found that some very intuitive results cannot be taken for granted. . . . The moral is that general assertions about the effects of parameter changes on the bargaining process are suspect" (1983, 239). The same would seem to apply to crisis bargaining.

## Appendix

Proposition 1 is proved through backwards programming. As noted above, no state can attach a positive probability to attacking at any information set. Accordingly, determining the sequential equilibria means specifying  $e^*_I(m)$  and  $e^*_I(n)$  for  $0 \leq m \leq (K/2)$ ,  $e^*_{II}(n)$  and  $e^*_{II}(n)$  for  $1 \leq n \leq (K/2)$ , and a consistent set of beliefs.

From the definition of the upper bound  $N_I$ ,  $I$ 's best reply at  $\theta_I(n)$  for  $n > N_I = 1$  is to submit:  $e^*_I(n) = 0$ . Similarly,  $e^*_{II}(n) = 0$  for  $n > N_{II} = 1$ ;  $e^*_{II}(n) = 0$  for  $n > N_{II} = 2$ ; and  $e^*_I(n) = 0$  for  $n > N_I \geq 2$ .

Now,  $e^*_I(n) = 1$  for  $N_I \geq n \geq 2$ . If  $I$  escalates at  $\theta_I(n)$  for  $N_I \geq n \geq 2$ , its adversary, regardless of type, will not subsequently escalate. The risk is too high. But then from the definition of  $N_I$ , the payoff to  $I$  of escalating at these information sets is greater than the return to submitting. So,  $e^*_I(n) = 1$  for  $N_I \geq n \geq 2$ .

Recall that the game was simplified above by assuming that  $I$  would escalate at  $\theta_I(0)$  and  $\theta_I(1)$  regardless of what its adversary does. This, it was observed, may further restrict  $N_I$  beyond the existing constraint that  $N_I \geq 2$ . The worst that can happen to  $I$  is that it is facing a resolute adversary that escalates with prob-



ability 1 at  $\theta_{II}(1)$  and  $\theta_{II}(2)$ . Since  $N_{II} = 2$ , the resolute adversary II' will then submit. To ensure that I escalates at  $\theta_I(0)$  and  $\theta_I(1)$  in this worst case, it will suffice to assume  $R_I > 1 - (1 - f)(1 - 2f)(1 - 3f)(1 - 4f) + (q_I - s_I)/(w_I - d_I)$ . For any  $f$ , this can always be done with a suitable choice of  $w_I$ ,  $q_I$ ,  $s_I$ , and  $d_I$ . If, moreover,  $f < .1$ , as is assumed in the discussion of the distribution of crises, then when  $R_I$  satisfies this inequality,  $R_I$  is also greater than  $4f$  which ensures that  $N_I \geq 2$ . Thus, taking  $R_I$  to satisfy this inequality and taking  $f < .1$  guarantees  $e^*_{I'}(0) = e^*_{I'}(1) = 1$ .

The only strategies left to be specified are those at  $\theta_I(0)$ ,  $\theta_I(1)$ ,  $\theta_{II}(1)$ , and  $\theta_{II}(2)$ . To find these, begin by considering the problem facing II' if the crisis reaches  $\theta_{II}(2)$ . If II' is facing I, escalation brings  $3fd_{II} + (1 - 3f)w_{II}$ . If, however, II' is facing I', escalation yields  $3fd_{II} + (1 - 3f)[4fd_{II} + (1 - 4f)s_{II}]$ . But II' is uncertain whether it is facing I or I', and the expected payoff to escalation is  $\beta_{II}(2)[3fd_{II} + (1 - 3f)s_{II}] + [1 - \beta_{II}(2)][3fd_{II} + (1 - 3f)[4fd_{II} + (1 - 4f)s_{II}]]$ .

Let  $B_{II}(2)$  be the value of  $\beta_{II}(2)$  that leaves II' indifferent between submitting and escalating at  $\theta_{II}(2)$ . Then,  $B_{II}(2) = [1 - (1 - 3f)(1 - 4f)(1 - R_{II})]/\{(1 - 3f)[(1 - 4f)R_{II} + 4f]\}$ . So, the best response of II' at  $\theta_{II}(2)$ , is  $e^*_{II'}(2) = 1$  if  $\beta_{II}(2) > B_{II}(2)$ ;  $e^*_{II'}(2) \in [0, 1]$  if  $\beta_{II}(2) = B_{II}(2)$ ; and  $e^*_{II'}(2) = 0$  if  $\beta_{II}(2) < B_{II}(2)$ .

Beliefs are given by Bayes' rule where this rule can be applied. For  $\theta_{II}(2)$ , this means  $\beta_{II}(2) = \delta e_I(0)e_I(1)/[\delta e_I(0)e_I(1) + (1 - \delta)e_{I'}(0)e_{I'}(1)]$ . Recalling that  $e^*_{I'}(0) = e^*_{I'}(1) = 1$  in equilibrium, substituting for  $\beta_{II}(2)$  shows that II's best reply is

$$e^*_{II'}(2) = 1 \quad \text{if} \\ e_I(1)e_I(0) > \left(\frac{1 - \delta}{\delta}\right) \left(\frac{B_{II}(2)}{1 - B_{II}(2)}\right) \\ e^*_{II'}(2) \in [0, 1] \quad \text{if} \\ e_I(1)e_I(0) = \left(\frac{1 - \delta}{\delta}\right) \left(\frac{B_{II}(2)}{1 - B_{II}(2)}\right)$$

$$e^*_{II'}(2) = 0 \quad \text{if} \\ e_I(1)e_I(0) < \left(\frac{1 - \delta}{\delta}\right) \left(\frac{B_{II}(2)}{1 - B_{II}(2)}\right)$$

Now observe that in no sequential crisis equilibrium can  $e^*_{II'}(2) = 0$ . To see this, assume that  $e^*_{II'}(2) = 0$ . Since II' is certain to submit at  $\theta_{II}(2)$ , I's best reply at  $\theta_I(1)$  is to escalate:  $e_I(1) = 1$ . But if  $e_I(1) = e_{I'}(1) = 1$ , then  $e_{II'}(1) = 0$ , since it makes no sense for II' to escalate at  $\theta_{II}(1)$  if it is certain to quit at  $\theta_{II}(2)$  and I and I' are certain to escalate at  $\theta_I(1)$  and  $\theta_{I'}(1)$ . Similarly,  $e_{II}(1) = 0$ . But this is a contradiction: if  $e_{II}(1) = e_{II'}(1) = 0$ , no challenge is resisted, and there is no crisis.

This observation implies that  $e_I(1)e_I(0) \geq [(1 - \delta)/\delta]\{B_{II}(2)/[1 - B_{II}(2)]\}$  in any these equilibria requires the consideration of two cases. Equality is assumed to hold in the first and yields Equilibria 1, 2, and 4. Inequality is presumed in the second and this gives Equilibrium 3.

### Case 1

Case 1 is defined by:

$$e_I(1)e_I(0) = [(1 - \delta)/\delta]\{B_{II}(2)/[1 - B_{II}(2)]\}$$

Consider the problem facing I at  $\theta_I(1)$ . The payoff to escalation is

$$\beta_I(1)[2fd_I + (1 - 2f)w_I] + [1 - \beta_I(1)] \\ \cdot (2fd_I + (1 - 2f)[(1 - e_{II}(2))w_I \\ + e_{II'}(2)[3fd_I + (1 - 3f)s_I])$$

Let  $\hat{e}_{II'}(2)$  be the strategy of II' at  $\theta_{II}(2)$  that leaves I indifferent to submitting and escalating. Then,  $\hat{e}_{II'}(2) = (R_I - 2f)/\{[1 - \beta_I(1)](1 - 2f)[(1 - 3f)R_I + 3f]\}$ . I's best response at  $\theta_I(1)$  is  $e_I = 0$  if  $e_{II}(2) > \hat{e}_{II'}(2)$ ;  $e_I(1) \in [0, 1]$  if  $e_{II}(2) = \hat{e}_{II'}(2)$ ; and  $e_I(2) = 0$  if  $e_{II}(2) < \hat{e}_{II'}(2)$ .

The expression for  $\hat{e}_{II'}(2)$  may be simplified by substituting for  $\beta_I(1)$ . By Bayes' rule,  $\beta_I(1) = qe_{II}(1)/[qe_{II}(1) + (1 - q)e_{II'}(1)]$ . This gives

$$\hat{e}_{II'}(2) = \left[1 + \left(\frac{q}{1 - q}\right) \frac{e_{II}(1)}{e_{II'}(1)}\right]$$

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$$\left( \frac{R_I - 2f}{(1 - 2f)(1 - 3fR_I + 3f)} \right)$$

Now turn to the problem facing II at  $\theta_{II}(1)$ . Let  $\hat{e}_I(1)$  be the value of  $e_I(1)$  that makes II indifferent to escalating and submitting. Then  $\hat{e}_I(1)$  satisfies

$$\begin{aligned} s_{II} &= \beta_{II}(1)(fd_{II} + (1 - f)\{[1 - \hat{e}_I(1)]w_{II} \\ &\quad + \hat{e}_I(1)[2f + (1 - 2f)s_{II}]\}) \\ &\quad + [1 - \beta_{II}(1)]\{fd_{II} + (1 - f)[2f + (1 - 2f)s_{II}]\} \end{aligned}$$

and this is given by

$$\hat{e}_I(1) = 1 - \frac{[1 - (1 - f)(1 - 2f)(1 - R_{II})]}{\beta_{II}(1)(1 - f)[(1 - 2f)R_{II} + 2f]}$$

Substituting for  $\beta_{II}(1)$  yields

$$\begin{aligned} \hat{e}_I(1) &= 1 - \left[ 1 + \left( \frac{1 - \delta}{\delta} \right) \frac{1}{e_I(0)} \right] \\ &\quad \left( \frac{[1 - (1 - f)(1 - 2f)(1 - R_{II})]}{(1 - f)[(1 - 2f)R_{II} + 2f]} \right) \end{aligned}$$

II's best reply at  $\theta_{II}(1)$  is  $e_{II}(1) = 0$  if  $e_I(1) > \hat{e}_I(1)$ ;  $e_{II}(1) \in [0, 1]$  if  $e_I(1) = \hat{e}_I(1)$ ; and  $e_{II}(1) = 1$  if  $e_I(1) < \hat{e}_I(1)$ .

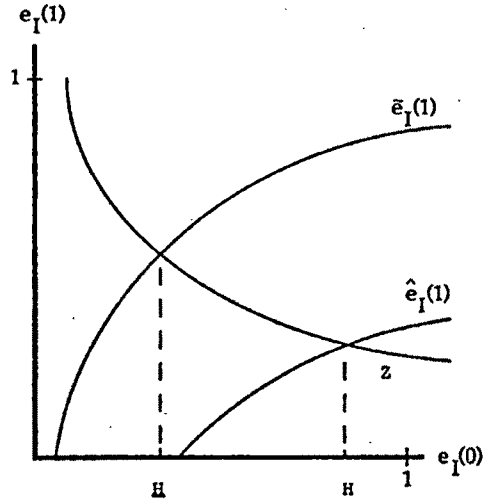
II' faces a similar problem at  $\theta_{II'}(1)$ . Let  $\bar{e}_I(1)$  be the value of  $e_I(1)$  that leaves II' indifferent to escalating and submitting. Then,

$$\begin{aligned} \bar{e}_I(1) &= 1 - \left[ 1 + \left( \frac{1 - \delta}{\delta} \right) \frac{1}{e_I(0)} \right] \\ &\quad \left( \frac{[1 - (1 - f)(1 - 2f)(1 - R_{II'})]}{(1 - f)[(1 - 2f)R_{II'} + 2f]} \right) \end{aligned}$$

The best response for II' at  $\theta_{II'}(1)$  is  $e_{II'}(1) = 0$  if  $e_I(1) > \bar{e}_I(1)$ ;  $e_{II'}(1) \in [0, 1]$  if  $e_I(1) = \bar{e}_I(1)$ ; and  $e_{II'}(1) = 1$  if  $e_I(1) < \bar{e}_I(1)$ .

These best-reply correspondences will now be used to find points that are the best responses to themselves. Note that  $\hat{e}_I(1)$  and  $\bar{e}_I(1)$  are increasing in  $e_I(0)$ . Since  $f < R_{II} < 3f$  and  $3f < R_{II'} < 5f$ , it is easy to show that for any  $e_I(0)$ ,  $\hat{e}_I(1) < \bar{e}_I(1)$ . Figure 2 graphs  $\hat{e}_I(1)$  and  $\bar{e}_I(1)$  and the constraint, Z, that defines Case 1. That is, Z is the curve given by  $e_I(1)e_I(0) = [(1 -$

Figure A-1.  $\hat{e}_I(1)$  and  $\bar{e}_I(1)$



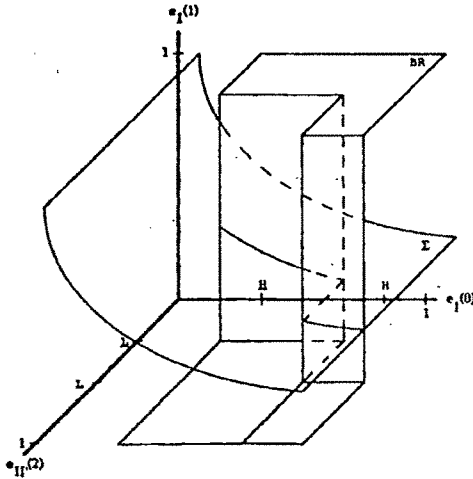
$\delta)/\delta\}\{B_{II'}(2)/[1 - B_{II'}(2)]\}$ . Because in Case 1 this constraint must be satisfied, only points along Z need be considered as potential equilibria.

Let  $\underline{H}$  be the value of  $e_I(0)$  at which  $\bar{e}_I(1)$  satisfies the constraint and let  $H$  be the value of  $e_I(0)$  at which  $\hat{e}_I(1)$  satisfies the constraint. Because  $\hat{e}_I(1) < \bar{e}_I(1)$  for any  $e_I(0)$  and because  $e_I(1)$  is decreasing in  $e_I(0)$  along Z,  $\underline{H} < H$ .

$\underline{H}$  and  $H$  make it possible to specify the best replies of II and II' at  $\theta_{II}(1)$  and  $\theta_{II'}(1)$ . Considering only points along Z, then if, for example,  $e_I(0) < \underline{H}$ , it must be that  $e_I(1) > \bar{e}_I(1)$ . But this means that the best reply of II is  $e_{II}(1) = 0$ . The best-reply correspondence of II at  $\theta_{II}(1)$  is given by  $e_{II}(1) = 0$  if  $e_I(0) < \underline{H}$ ;  $e_{II}(1) \in [0, 1]$  if  $e_I(0) = \underline{H}$ ; and  $e_{II}(1) = 1$  if  $e_I(0) > \underline{H}$ . Similarly, the best-reply correspondence of II' at  $\theta_{II'}(1)$  is given by  $e_{II'}(1) = 0$  if  $e_I(0) < H$ ;  $e_{II'}(1) \in [0, 1]$  if  $e_I(0) = H$ ; and  $e_{II'}(1) = 1$  if  $e_I(0) > H$ .

In a sequential crisis equilibrium there must be some chance of a challenge being resisted. This means that  $e_I(0) \geq \underline{H}$ , because if  $e_I(0) < \underline{H}$ , then  $e_{II}(1) = 0$  and  $e_{II'}(1) = 0$ . Thus the search for sequential equilibria may be limited to points along

Figure A-2. The Best-Reply Correspondences



Z for which  $e_I(0) \geq \underline{H}$ . If, moreover,  $e_I(0) = \underline{H}$ , then  $e_{II}(1) > 0$ .

Now consider  $I$ 's best response if the game reaches  $\theta_I(1)$ . This depends on a comparison of  $e_{II}(2)$  and  $\hat{e}_{II}(2)$ . This comparison may be transformed into one between  $e_{II}(2)$  and  $e_I(0)$ . Recall that  $\hat{e}_{II}(2)$  is a function of  $e_{II}(1)/e_{II}(1)$ . But in equilibrium,  $e_{II}(1)$  and  $e_{II}(1)$  will be best replies to other strategies. These best replies are, as was just demonstrated, functions of  $e_I(0)$ . If, for example,  $\underline{H} \leq e_I(0) < H$ , the best replies of  $II$  and  $II'$  at  $\theta_{II}(1)$  and  $\theta_{II'}(1)$  are  $e_{II}(1) = 0$  and  $e_{II'}(1) > 0$ . Hence,  $\hat{e}_{II}(2)$  evaluated at these best responses is  $[R_I - 2f]/\{(1 - 2f)[(1 - 3f)R_I + 3f]\}$ . Let  $\underline{L}$  be this value and let  $L$  be the value of  $\hat{e}_{II}(2)$  evaluated at  $e_{II}(1) = e_{II'}(1) = 1$ . Then the transformed best-reply correspondence for  $I$  at  $\theta_I(1)$  is

1	if	$(e_I(0), e_{II}(2)) \in [\underline{H}, H) \times [0, \underline{L}]$
[0,1]	if	$(e_I(0), e_{II}(2)) \in [\underline{H}, H) \times \{\underline{L}\}$
0	if	$(e_I(0), e_{II}(2)) \in [\underline{H}, H) \times (\underline{L}, 1]$
1	if	$(e_I(0), e_{II}(2)) \in \{H\} \times [0, \underline{L}]$
[0,1]	if	$(e_I(0), e_{II}(2)) \in \{H\} \times [\underline{L}, L]$
0	if	$(e_I(0), e_{II}(2)) \in \{H\} \times (L, 1]$
1	if	$(e_I(0), e_{II}(2)) \in (H, 1] \times [0, L]$

[0,1	if	$(e_I(0), e_{II}(2)) \in (H, 1] \times \{L\}$
0	if	$(e_I(0), e_{II}(2)) \in (H, 1] \times (L, 1]$

This correspondence is illustrated in Figure 3 and will be called  $BR$ . Figure 3 also shows the constraining curve  $Z$ , which has now become a surface,  $\Sigma$ .

Remember that the problem is to specify the strategies at  $\theta_I(0)$ ,  $\theta_I(1)$ ,  $\theta_{II}(1)$ ,  $\theta_{II'}(1)$ , and  $\theta_{II'}(2)$ . The first of these to fall out is  $e_{II'}(2)$ . If a sequential crisis equilibrium exists it must be in the intersection of  $BR$  and  $\Sigma$ . But at all points of intersection,  $0 < e_I(1) < 1$ . To see this, recall that  $Z$  approaches 0 asymptotically from above. So,  $e_I(1) > 0$ . Over  $\underline{H} \leq e_I(0) \leq 1$ ,  $e_I(1)$  attains its maximum on  $\Sigma$  at  $e_I(0) = \underline{H}$ . But from the definition of  $\underline{H}$ ,  $e_I(1) < 1$  at  $e_I(0) = \underline{H}$ . Since  $0 < e_I(1) < 1$ , the best-reply correspondence for  $I$  at  $\theta_I(1)$  implies  $e_{II}(2) = \hat{e}_{II}(2)$ . In a sequential crisis equilibrium,  $e_{II'}^*(2) = \hat{e}_{II}(2)$ . (Of course,  $\hat{e}_{II}(2)$  is still a function of  $e_{II}(1)$  and  $e_{II'}(1)$ , so a complete determination of  $\hat{e}_{II}(2)$  awaits their specification.)

At this point, it is necessary to consider two subcases. In the first,  $e_I(0) \geq H$  and this will yield Equilibrium 2. The second subcase is defined by  $\underline{H} \leq e_I(0) < H$ , and this will give Equilibria 1 and 4.

The specification of Equilibria 2 begins by noting that because  $e_I(0) \geq H > \underline{H}$  in the first subcase, then  $e_{II'}(1) = 1$ . Moreover,  $1 \geq e_I(0) \geq H$ . Accordingly, it is assumed that  $H < 1$ . The possibility that  $H = 1$  is disregarded because it has measure zero. This is in keeping with the previous assumption that strict inequality held for resolve. That is,  $R_I$ , for example, was restricted to  $f < R_I < 3f$  although the constraint that  $N_I = 1$  only required  $f \leq R_I < 3f$ . This assumption was made to eliminate the multiple equilibria that arise only in the event that  $R_I = f$  which has measure zero.

With  $H < 1$ , it follows by contradiction that  $e_I(0) < 1$ . Assume  $e_I(0) = 1$ . This implies that  $\Sigma$  and  $BR$  intersect at  $e_I(0) = 1$ . It has already been shown that  $e_I(1) < 1$ .

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1. This means that as long as  $H < 1$ , the only place where  $e_I(0) = 1$  in this intersection is at  $e_{II}(2) = L$ . (When  $H = 1$ ,  $e_{II}(2)$  may be anything in the interval  $[L, L]$  in this intersection.) Since  $e_I(0) = 1 > H$ ,  $e_{II}(1) = 1$ . Moreover,  $e_I(0) = 1$  implies that the payoff to I from exploiting the situation at  $\theta_I(0)$  is at least as great as the value of the status quo. This leaves  $q_I \leq q[fd_I + (1 - f)s_I] + (1 - q)[fd_I + (1 - f)s_I]$  where the expression for the return to exploiting the situation has been simplified by taking  $e_{II}(1) = e_{II}(1) = 1$  and noting that because  $0 < e_I(1) < 1$ , I is randomizing at  $\theta_I(1)$  which means that the expected payoff if this information set is reached is  $s_I$ . The right side of this is less than  $s_I$ . This, however, is a contradiction since  $q_I > s_I$ . Hence,  $H < 1$  means  $0 < \underline{H} < e_I(0) < 1$ .

Since  $0 < e_I(0) < 1$ , I must be indifferent to accepting the status quo and exploiting the situation. This leads directly to an expression for  $e^*_{II}(1)$ . Indifference implies  $q_I = q\{[1 - e^*_{II}(1)]w_I + e^*_{II}(1)[fd_I + (1 - f)s_I]\} + (1 - q)[fd_I + (1 - f)s_I]$ . This gives

$$e^*_{II}(1) = 1 - \frac{1}{q} \left[ 1 - \left( \frac{w_I - q_I}{w_I - d_I} \right) \left( \frac{1}{(1 - f)R_I + f} \right) \right]$$

Now that  $e^*_{II}(1)$  has been determined,  $e^*_{II}(2)$  may be fully specified by evaluating  $\hat{e}_{II}(2)$  at  $e^*_{II}(1)$  and  $e^*_{II}(1)$ . The result is the expression reported for Equilibrium 2 in Proposition 1.

The expression for  $e^*_{II}(1)$  places a restriction on the relation among the variables  $q$ ,  $q_I$ ,  $R_I$ , and  $f$ . Examining it carefully yields the last two strategies that remain to be determined,  $e^*_I(0)$  and  $e^*_I(1)$ . The expression for  $e^*_{II}(1)$  implies  $e^*_{II}(1) < 1$ . The best-reply correspondence for II at  $\theta_{II}(1)$  shows that  $e_I(0) \leq H$  if  $e^*_{II}(1) < 1$ . But  $e_I(0)$  has also been restricted to being larger than or equal to  $H$ . Thus,  $e^*_I(0) = H$ .  $H$ , in turn, is the value of  $e_I(0)$  that satisfies the constraint

$\hat{e}_I(1) = [(1 - \delta)/\delta][B_{II}(2)/[1 - B_{II}(2)]] [1/e_I(0)]$ . Solving this for  $e_I(0)$  gives the expression for  $e^*_I(0)$  in Proposition 1. Substituting  $e^*_I(0)$  for  $e_I(0)$  in the constraint defining Case 1 yields the expression for  $e^*_I(1)$ .

The expressions for the states' beliefs are obtained by Bayes' rule and by substituting the expressions for the equilibrium strategies that have just been derived.

A sequential equilibrium will exist as long as the expressions for  $e^*_I(0)$ ,  $e^*_I(1)$ ,  $e^*_{II}(1)$ , and  $e^*_{II}(2)$  satisfy the constraints  $0 \leq e^*_I(0) \leq 1$ ,  $0 \leq e^*_I(1) \leq 1$ ,  $0 \leq e^*_{II}(1) \leq 1$ , and  $0 \leq e^*_{II}(2) \leq 1$ . The first two constraints are satisfied because  $0 < H < 1$ . Only  $H < 1$  is binding and this gives the restriction on  $\delta$  in Proposition 1. The expression for  $e^*_{II}(1)$  is always less than one, and  $e^*_{II}(1) \geq 0$  yields the lower bound on  $q$  for Equilibrium 2 in Proposition 1. Finally, examining the expression for  $e^*_{II}(2)$  after substituting the expression for  $e^*_{II}(1)$  gives the upper bound.

Now consider the second subcase in which  $\underline{H} \leq e_I(0) < H$ . If  $H < 1$ , Equilibrium 1 results. If  $H > 1$ , Equilibrium 4 obtains. The event  $H = 1$  has measure zero and is disregarded.

Assume  $\underline{H} \leq e_I(0) < H < 1$ . From II's best-reply correspondence,  $e_{II}(1) = 0$ . If, moreover,  $\underline{H} < e_I(0)$ , then the best reply for II is  $e_{II}(1) = 1$ . But because  $0 < \underline{H} \leq e_I(0) < H < 1$ , I is mixing at  $\theta_I(0)$ . As has already been shown,  $0 < e_I(1) < 1$  for  $\underline{H} \leq e_I(0) < 1$ , so I is also mixing at  $\theta_I(1)$ . This means that I must be indifferent to the payoffs to exploiting and to not exploiting the situation. With  $e_{II}(1) = 0$  and  $e_{II}(1) = 1$ , this requires  $q_I = q[w_I + (1 - q)[(1 - f)(w_I - d_I) + f(w_I - s_I)]]$ . This, however, is an event of measure zero. Disregarding it leaves  $e_I(0) = \underline{H}$ .

Substituting for  $\underline{H}$  gives the expression for  $e^*_I(0)$  for Equilibrium 1 in Proposition 1. Substituting the expression for  $e^*_I(0)$  in the constraint that defines Case 1 gives  $e^*_I(1)$ . Finally,  $e^*_{II}(2)$  is obtained by not-

ing that  $e_{II}(2) = \underline{L}$  for all points in the intersection of  $\Sigma$  and  $BR$  for  $e_I(0) < H$ . Thus,  $e^*_{II}(2) = \underline{L}$ .

The only strategy left to be specified is  $e_{II}(1)$  which is no longer necessarily 1 as it was when  $e_I(0) > \underline{H}$ . With  $e_I(0) = \underline{H}$ ,  $II$  may mix at  $\theta_{II}(1)$ . To obtain  $e^*_{II}(1)$ , note that because  $I$  mixes at  $\theta_I(0)$  and  $\theta_I(1)$  and  $e_{II}(1) = 0$ ,  $q_I = w_I - (1 - q)\{e^*_{II}(1)w_I + [1 - e^*_{II}(1)][fd_I + (1 - f)s_I]\}$ . Solving this for  $e_{II}(1)$  gives the expression for  $e^*_{II}(1)$  for Equilibrium 1 in Proposition 1.

Combining Bayes' rule and the expressions for the states' strategies gives the beliefs reported in Proposition 1. The constraint that  $1 \geq \underline{H}$  yields the restriction on  $\delta$ . Constraining the expressions for  $e^*_I(0)$  and  $e^*_{II}(1)$  to be less than 1 gives the restriction on  $q$ .

If  $H > 1$ , Equilibrium 4 results. With  $H > 1$ , if  $\underline{H} < e^*_I(0) = 1 < H$ ,  $e^*_{II}(1) = 0$ ,  $e^*_{II}(2) = \underline{L}$ , and  $e^*_I(1) = [1 - \delta]/\delta[B_{II}(2)/[1 - B_{II}(2)]]$ .

Beliefs are given by Bayes' rule and by substituting the expressions for the states' strategies. The constraint  $H > 1$  yields the upper bound on  $\delta$ , and the expression for  $e^*_I(1)$  gives the lower bound. Because  $e^*_I(0) = 1$ , the expected payoff to exploiting the situation must be at least equal to the payoff to not exploiting the situation. This gives the restriction on  $q$  for Equilibrium 4 in Proposition 1.

## Case 2

Case 2 is defined by:

$$e_I(1)e_I(0) > [(1 - \delta)/\delta]\{B_{II}(2)/[1 - B_{II}(2)]\}$$

In this case, the best-reply correspondence for  $e_{II}(2)$  implies  $e^*_{II}(2) = 1$ . This leaves  $e^*_I(0)$ ,  $e^*_I(1)$ ,  $e^*_{II}(1)$ , and  $e^*_{II}(2)$  to be specified.

The specification begins with two observations. First, because the resolve of  $II$  is resolute, and with  $e_{II}(2) = 1$ ,  $I$ 's best seem intuitively that if  $e^*_{II}(1) > 0$ , then

$e^*_{II}(1) = 1$ . Algebra shows that the value of  $\beta_{II}(1)$  that sustains  $e_{II}(1) > 0$  is strictly greater than the value of  $\beta_{II}(1)$  that sustains  $e_{II}(1) = 1$ . But,  $\beta_{II}(1) = \beta_I(1)$ , so if  $e_{II}(1) > 0$ , then  $e_{II}(1) = 1$ .

The second remark is that in a crisis equilibrium it must be that  $e^*_{II}(1) > 0$ . Suppose the contrary, that is,  $e^*_{II}(1) = 0$ . In a crisis equilibrium, there must be some chance of resistance, so  $e^*_{II}(1) > 0$ . But given these strategies, resistance unambiguously signals that the challenged state is resolute, and with  $e_{II}(2) = 1$ ,  $I$ 's best reply is  $e_I(1) = 0$ . This, however, contradicts the inequality defining Case 2. Thus,  $e^*_{II}(1) > 0$ . These remarks imply that  $e^*_{II}(1) > 0$  and  $e^*_{II}(1) = 1$ .

Now consider the problem facing  $I$  at  $\theta_I(1)$ . Let  $B_I(1)$  be the value of  $\beta_I(1)$  that leaves  $I$  indifferent to escalating and to submitting. Then  $B_I(1) = [1 - (1 - 2f)(1 - 3f)](1 - R_I)/\{(1 - 2f)[(1 - 3f)R_I + 3f]\}$ . Thus,  $I$ 's best response at  $\theta_I(1)$  is  $e_I(1) = 1$  if  $\beta_I(1) > B_I(1)$ ;  $e_I(1) \in [0, 1]$  if  $\beta_I(1) = B_I(1)$ ; and  $e_I(1) = 0$  if  $\beta_I(1) < B_I(1)$ .

Applying Bayes' rule and substituting for  $\beta_I(1)$  implies that  $I$ 's best-reply correspondence can be written as  $e_I(1) = 1$  if  $e_{II}(1) > [(1 - q)/q]\{B_I(1)/[1 - B_I(1)]\}$ ;  $e_I(1) \in [0, 1]$  if  $e_{II}(1) = [(1 - q)/q]\{B_I(1)/[1 - B_I(1)]\}$ ; and  $e_I(1) = 0$  if  $e_{II}(1) < [(1 - q)/q]\{B_I(1)/[1 - B_I(1)]\}$ .

The next strategy to be specified is  $e^*_{II}(1)$ . By the inequality defining Case 2,  $e_I(1) > 0$ . It will now be shown that  $e_I(1) < 1$ . Assume that  $e_I(1) = 1$ . Then  $e_{II}(1) \geq (1 - q)B_I(1)/\{q[1 - B_I(1)]\}$ . If, moreover,  $e_I(1) = 1$  and  $e^*_I(1) = 1$ , then  $II$  will submit at  $\theta_{II}(1)$  because there is no chance of its prevailing. But if  $e_{II}(1) = 0$ , then  $e_{II}(1) < (1 - q)B_I(1)/\{q[1 - B_I(1)]\}$ , a contradiction. This contradiction leaves  $e^*_I(1) < 1$ . The fact that  $0 < e^*_I(1) < 1$  and the best-reply correspondence for  $I$  at  $\theta_I(1)$  yields  $e^*_{II}(1) = (1 - q)B_I(1)/\{q[1 - B_I(1)]\}$ .

The strategies left to be specified are  $e^*_I(0)$  and  $e^*_I(1)$ , and this begins by obtaining a relation between them. For there

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to be an equilibrium  $0 \leq e^*_{II}(1) = (1 - \rho)B_I(1)/\{\rho[1 - B_I(1)]\} \leq 1$ . That is,  $\rho \geq B_I(1)$ . Disregarding the event  $\rho = B_I(1)$  because it has measure zero and taking  $\rho > B_I(1)$ , then  $0 < e^*_{II}(1) < 1$ . With  $0 < e^*_{II}(1) < 1$ , II is indifferent to escalating and submitting at  $\theta_{II}(1)$ . So

$$\begin{aligned} s_{II} = & \beta_{II}(1) [fd_{II} + (1 - f) \\ & ([1 - e^*_{II}(1)] w_{II} + e^*_{II}(1) \\ & \{2f + (1 - 2f)s_{II}\}) \\ & + [1 - \beta_{II}(1)][2f + (1 - 2f)s_{II}] \end{aligned}$$

Substituting for  $\beta_{II}(1)$  yields

$$\begin{aligned} e^*_{II}(1) = & 1 - \\ & \left[ 1 + \left( \frac{1 - \delta}{\delta} \right) \frac{1}{e_I(0)} \right] \\ & \left( \frac{[1 - (1 - f)(1 - 2f)][1 - R_{II}]}{(1 - f)[(1 - 2f)R_{II} + 2f]} \right) \end{aligned}$$

This relation is the same as the relation between  $e_I(0)$  and  $\hat{e}_I(1)$  that was derived in Case 1. In Case 2, the sequential equilibria must lie along  $\hat{e}_I(1)$  in Figure A-1 above the constraint Z. This implies  $0 < e^*_{II}(1) < 1$ .

I's best-response correspondence at  $\theta_I(0)$  will now be determined. Note that because I randomizes at  $\theta_I(1)$ , I's expected payoff if the game reaches this information set is  $s_I$ . Then letting  $Q_I$  be the value of the status quo at which I would be indifferent to exploiting the situation and accepting the status quo gives

$$\begin{aligned} Q_I = & \rho\{[1 - e^*_{II}(1)]w_I + e^*_{II}(1) \\ & [fd_I + (1 - f)s_I]\} + (1 - \rho) \\ & \{[1 - e^*_{II}(1)]w_I + e^*_{II}(1)[fd_I + (1 - f)s_I]\} \end{aligned}$$

Substituting the expressions for  $e^*_{II}(1)$  and  $e^*_{II}(1)$  produces

$$\begin{aligned} Q_I = & \{\rho - B_I(1)/[1 - B_I(1)] \\ & [(1 - f)(w_I - s_I) + f(w_I - d_I)] \\ & + fd_I + (1 - f)s_I \} \end{aligned}$$

The best-reply correspondence for I at

$\theta_I(0)$  is, therefore,  $e_I(0) = 1$  if  $q_I < Q_I$ ;  $e_I(0) \in [0, 1]$  if  $q_I = Q_I$ ; and  $e_I(0) = 0$  if  $q_I > Q_I$ .

The final step in determining  $e^*_{II}(0)$  and  $e^*_{II}(1)$  is to note that if there is to be a sequential crisis equilibrium, then  $q_I \leq Q_I$ . Otherwise, I will never challenge the status quo. A challenge will then unambiguously signal that the challenger is resolute. This challenger escalates with probability 1 for all  $n \leq N_I$ . There will be no resistance and no crisis. Hence,  $q_I \leq Q_I$ .

Disregarding the event  $q_I = Q_I$  which has measure zero, the equilibrium strategies  $e^*_{II}(0)$  and  $e^*_{II}(1)$  may now be specified. If  $q_I < Q_I$ , then  $e^*_{II}(0) = 1$  and  $e^*_{II}(1)$  is the value of  $\hat{e}_I(1)$  evaluated at  $e_I(0) = 1$ , which is the expression for  $e^*_{II}(1)$  in Proposition 1.

Expressions for the relevant beliefs are obtained with Bayes' rule and by substituting the expressions for the states' strategies.

Several conditions are needed to ensure the existence of the sequential crisis equilibrium in Case 2. First,  $\hat{e}_I(1)$  must be above the constraint Z at  $e_I(0) = 1$ . This is equivalent to assuming  $H < 1$ , which, when solved for  $\delta$ , gives the restriction stated in Proposition 1. The second condition is that  $q_I < Q_I$ , which means that  $\rho > 1 - \{[1 - \beta^*_{II}(1)](w_I - q_I)\}/\{(w_I - d_I)[(1 - f)R_I + f]\}$ . When this second condition is met, so is the assumption made above that  $\rho > B_I(1)$ . The final conditions are that  $e^*_{II}(1) \leq 1$  and  $e^*_{II}(1) \leq 1$ . But  $H < 1$  assures  $e^*_{II}(1) < 1$  and  $\rho > B_I(1)$  that  $e^*_{II}(1) < 1$ . QED

A final observation is in order. Note that when  $\delta = 1$  the model reduces to that investigated in Powell 1987, in which there was only one-sided incomplete information in that only the potential challenger was uncertain of the level of resolve of its adversary. As to be hoped and expected, the equilibria found above degenerate to the unique equilibrium

found with one-sided incomplete information. With  $\delta = 1$ , Equilibrium 4 does not exist. Equilibria 1 and 2 disappear because with  $\delta = 1$ ,  $e^*_1(0) = 0$ . This makes it impossible to satisfy the inequality defining Case 1. Finally, for  $\delta = 1$ , the expressions describing Equilibrium 3 reduce to those in Powell 1987.

### Notes

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1. For a discussion of this means of exerting coercive pressure see Kaplan 1962. For a comparison of this means of exerting pressure to the manipulation of the risk of an unlimited nuclear exchange, see Powell 1985 and Schelling 1962.

2. For a discussion of the various ways that resolve has been formalized, see Powell 1987 and Wagner 1982.

3. See Kreps and Wilson 1982 for a definition and more detailed discussion of the properties of sequential equilibria.

4. If  $\delta$  and  $\rho$  do not satisfy the constraints defined in Proposition 1, then no sequential crisis equilibria exist. The only sequential equilibria that exist are not also crisis equilibria.

5. Because both I and I' always submit for  $n > N_I$  and both II and II' always submit for  $m > N_{II'}$ , the beliefs off the equilibrium path at  $\theta_I(n)$  and  $\theta_{I'}(n)$  and at  $\theta_{II}(m)$  and  $\theta_{II'}(m)$  can be anything. These arbitrary beliefs, however, do not affect the distribution of the game's outcomes and in this sense are irrelevant.

6. In other words, the states' levels of resolve are sufficient statistics for their payoffs.

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## CRISIS LEARNING GAMES

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*Findings from a data-based study of bargaining in recurrent crises between evenly matched states provide the foundation for the construction of four crisis-learning games. Symmetrical and asymmetrical nuclear and nonnuclear sequential three-by-three games assuming complete information and nonmyopic play are presented and analyzed. The empirical study indicated that states that were unsuccessful in one crisis were likely to move to more coercive bargaining strategies in the next crisis. The four sequential games offer insights as to why this is likely to produce unwanted consequences, while demonstrating the importance of the participants' initial strategy choices. With the realpolitik lessons suggested by the earlier study removed, the dynamics of the games present a case for beginning with a cooperative strategy and moving to reciprocating, or tit-for-tat, bargaining.*

Game theorists (Brams 1985; Kilgour and Zagare 1987; Powell 1987; Zagare 1985) have devoted considerable attention to bargaining in interstate security crises, particularly in nuclear crises. But an important related problem—the effects of changes in the strategy choices of policymakers from one crisis to the next—has been largely ignored. I examine that problem through four crisis-learning games constructed from findings drawn from a data-based study of bargaining in recurrent crises (Leng 1983).

The data-based study was concerned with two questions: What are the lessons that policymakers draw from one crisis experience to guide their behavior in the next crisis? and What are the likely consequences of applying those lessons?

### Previous Research

Regarding the first question, common sense suggests that the success or failure of the bargaining strategy employed in

one crisis is likely to influence the choice of bargaining strategy in the next crisis. Jervis (1976) has hypothesized that a tendency to see their own state's behavior as decisive encourages policymakers to consider unsuccessful outcomes as failures of policy so that they are likely to react by switching to a different bargaining strategy in the next encounter. The same reasoning encourages them to repeat bargaining strategies associated with successful outcomes.

Jervis leaves open the question of *what* sort of change to expect if the outcome of the preceding crisis was unsatisfactory. I have hypothesized elsewhere (Leng 1983), that national policymakers are most likely to shift to a more coercive bargaining strategy. This proposition is based on the pervasiveness of a realpolitik tradition that views interstate crises as dominated by considerations of power politics and prescribes bargaining strategies that demonstrate power and resolve. Empirical support for this hypothesis appears in recent studies (Leng and Wheeler 1979;



Snyder and Diesing 1977) demonstrating the pervasiveness of coercive bargaining across a wide range of twentieth century crises.

These hypotheses were combined in a "realpolitik experiential learning" (REL) model of bargaining behavior in recurrent crises (Leng 1983). The REL model begins with the proposition that national policymakers assume that the outcome of the preceding crisis was related directly to the bargaining strategy that they employed. The experiential learning component of the proposition posits that policymakers will repeat those strategies that led to victory and change those that led to unsatisfactory outcomes. The realpolitik component of the proposition posits that policymakers are likely to assume that the undesirable outcome was the result of a failure credibly to demonstrate resolve (Leng 1983, 380-82). This leads to a prescription to employ a more belligerent bargaining strategy in the next crisis with the same adversary.

The same realpolitik reasoning that prescribes the switch to a more belligerent strategy by the loser encourages the previous winner to respond in kind once it observes the change in its adversary's behavior. If both parties are dissatisfied with the outcome of the preceding crisis, the REL proposition posits that each will begin the next with a more coercive bargaining strategy. Thus, in all instances except those when the two parties reached a mutually satisfactory compromise in the preceding crisis, REL predicts that each succeeding crisis between the two adversaries will become more belligerent. A test of these hypotheses (Leng 1983), employing events data from 18 twentieth century crises involving six pairs of nations of relatively equal military capability yielded findings supporting the REL propositions. The study's answer to the second question—What are the likely consequences of following the lessons that policymakers draw from crisis experi-

ences?—is that each successive crisis will become more dangerously conflictual.

### The Research Question

The findings from the empirical study raise an important prescriptive question: How can policymakers avoid this trap? If coercion only begets coercion, is there a better approach to crisis bargaining in recurrent crises? Data-based studies of crisis bargaining more generally (Huth and Russett 1987; Leng 1984; Leng and Wheeler 1979), have indicated that "firm-but-fair," or tit-for-tat bargaining strategies are superior to escalating coercive strategies in crises between states relatively evenly matched in military capabilities and motivation. Axelrod (1984) has used prisoner's dilemma games to explore the strategic logic behind the efficacy of tit-for-tat strategies, and he offers a persuasive rationale for its effectiveness.

Game theory is employed here to trace the consequences of following an REL approach to the initial strategy choice in recurrent crises in which the players otherwise employ a strictly rational bargaining approach. The outcomes of these cases will be compared to those in which a rational bargaining strategy is followed from the outset. Then those outcomes will be compared to the outcomes obtained by following a simple tit-for-tat bargaining strategy. The results offer insights into the consequences of initial strategy choices, providing, as well, a provocative answer to the question of the best approach to bargaining in recurrent crises.

### Game Theory and the REL Model

The REL model is the foundation for the construction of four sequential three-by-three crisis learning games in which the strategy options for each player are based on the strategy employed in the preceding crisis: (1) a repeat of the same strategy, (2) a change to a more cooperative

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strategy, or (3) a change to a more coercive strategy than either party employed in the preceding crisis. The label attached to each strategy denotes the degree of coercion or cooperation *relative* to that employed in the preceding crisis. A "more cooperative" bargaining strategy, for example, may not appear to be very cooperative when viewed in absolute terms.

The crisis is assumed to have been initiated through a precipitant event or situation, which has led to a confrontation between the two states (see Leng and Singer n.d.; Snyder and Diesing 1977, chap. 1). The initial moves represent the opening bargaining strategies of the two governments. The basic REL proposition that the crisis begins with the loser of the preceding crisis choosing a more coercive strategy and the winner of the preceding crisis repeating the same strategy is retained. When the preceding crisis ended unsatisfactorily for both parties, the game begins with both parties choosing a more coercive strategy. These moves produce the initial outcome, which determines the starting point for a sequential game in which the two players make alternating moves until the game reaches a final outcome.

Allowing for the possibility of states breaking out of the REL trap, however, requires that once the initial strategies are selected, the requirement to respond in kind to coercive moves be dropped to permit the players to assess their bargaining strategies on a purely rational basis, that is, in terms of maximizing their pay-offs. Strictly following the REL proposition would produce a trivial game with the two sides becoming locked in to more coercive strategies on the second move.

Four types of crisis-learning games are identified, based on two distinctions: nuclear and nonnuclear crises, and asymmetric and symmetric pay-off structures. It is assumed that being locked in to mutually coercive strategies on the road to war represents the worst possible out-

come in a nuclear crisis, but not in a non-nuclear crisis. The pay-offs also differ according to whether the outcomes of the preceding crises were the same for both parties (symmetric) or there was a winner and a loser (asymmetric). I will outline the structure and rules of the games, then discuss the sequence of moves in each of the four types.

### Structure and Rules of the Game

#### Strategies

There are three possible strategies for each player: C = a more cooperative bargaining strategy than that used by either player in the preceding crisis, S = the same strategy used by that player in the preceding crisis, and E = a more coercive strategy than that used by either player in the preceding crisis.

#### Outcomes

The REL model posits that the players will make the following assumptions about the relationship between the strategies and the outcomes:

- ES or EC = diplomatic victory for strategy-E player
- SC = diplomatic victory for strategy-S player
- SS = same outcome as the preceding crisis
- CC = mutually satisfactory compromise
- EE = war

The first two assumptions are based on the realpolitik notion that demonstrating greater resolve and toughness prevails over a more accommodating opponent. The SS outcome represents a repeat of the last crisis outcome. CC is a mutually satisfactory compromise. The EE pay-off occurs when the players are locked in to mutually coercive strategies; that is, if the

**Table 1. Asymmetrical  
Nonnuclear Game**

Row	Column		
	C	S	E
C	5,6	3,9	2,8
S	9,4	8,2	1,7
E	7,3	6,1	4,5

player with the next move does not depart from an EE outcome, the game ends in war.

### Players' Goals

The primary objective of each player is to maximize its self-interest. In a nonnuclear crisis, winning is preferred over compromise, compromise over war, war over submission. In a nuclear crisis, the preference ordering is: winning, compromise, submission, war. The secondary objective is to minimize the level of coercion employed. Two assumptions lay behind the secondary objective: (1) the use of coercion increases the risk of war, so it is best to win at the lowest possible level of risk; (2) if it is necessary to compromise, or to submit, the costs increase relative to the extent to which the player has invested its reputation for resolve in an unsuccessful coercive strategy.

### Preference Orderings

The players' outcome preferences vary according to which of the four games is being played. In the asymmetric nonnuclear crisis, for example, where Row was the winner of the preceding crisis and Column was the loser, the outcome preference orderings would be:

Row's preferences:

1. Win SC > SS > EC > ES
2. Compromise CC > EE
3. Submit CS > CE > SE

Column's preferences:

1. Win CS > CE > SE
2. Compromise CC > EE > EC
3. Submit SC > SE > EC > ES

(The symbol ">" is interpreted as "preferred over.") The result is the three-by-three ordinal game exhibited in normal form in Table 1. The first entry in each cell represents the pay-off to Row, the second entry is Column's pay-off. The outcome pay-offs are ranked from *most preferred* to *least preferred*, with a score of nine representing the most preferred outcome. In asymmetric games, Row represents the winner of the preceding crisis and Column represents the loser.

### Play of the Game

The initial outcome of the game is determined in accordance with the strategies predicted by the REL model. Asymmetric games begin with Row (winner of the preceding crisis) choosing the same strategy (S) as that employed in the last crisis, and Column (loser of the preceding crisis) moving to a more coercive strategy (E). In a symmetric game, where both sides were dissatisfied with the outcome of the preceding crisis, each player chooses strategy E. If they both were satisfied with the preceding outcome, each would choose strategy S. In the asymmetric game shown in Table 1, the initial outcome is at (1,7).

Row is given the first opportunity to move from the initial outcome. This is based on the REL assumption that, in asymmetric games, the "winner" of the preceding crisis must decide how to react to the changed strategy of the "loser" of the preceding crisis. In symmetric games either player may move first; the interpretation of the dynamics of the game remain the same.

From this point on, the rules of play are based on the theory of moves (Brams and Wittman 1981) for sequential games. If

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Row chooses to change its strategy and move to a new outcome, then Column must decide whether to stay at that outcome, thereby ending the game, or to switch its strategy and move to a new outcome. The moves continue to alternate until the player whose turn it is decides to stay at the current outcome.

What constitutes a rational choice—move or stay—at each outcome depends on whether the game is viewed myopically or nonmyopically. In a game played myopically, each player simply asks whether there is a better outcome that can be obtained on the next move. If the game is played nonmyopically, each player has the foresight to calculate its moves based on the knowledge of the other player's response, its counterresponse, and so on, in an extended series of strictly alternating moves. Thus, each move is made in light of its long-term consequences. This is a more accurate depiction of the reality of crisis decision making, where efforts are made to "look down the road" to gauge the consequences of action-response sequences. Just *how far* down the road real world policymakers can, and do, look in the course of an ongoing crisis is an open question. I do not attempt to answer that question here; however, in the games that follow, I will note the degree of farsightedness necessary to achieve stable nonmyopic outcomes. This will afford some sense of just how farsighted decision makers must be to make the best strategy choices.

The assumption of nonmyopic play has important implications for how the game will end. Brams and Wittman (1981) have extended notions of stability and equilibrium to include nonmyopic calculation. Kilgour (1985) has extended and broadened these concepts to base them on the game's final outcome.<sup>1</sup> Using Kilgour's (1985) approach, the current outcome is *nonmyopically stable* for the player concerned unless it can do better by departing to set in motion a series of

moves and countermoves that will yield a preferable final outcome. If an outcome is nonmyopically stable for both players, it is a *nonmyopic equilibrium*. Kilgour's approach is particularly apt for our purposes. National leaders often accept crisis outcomes that fall short of total success in order to terminate increasingly intolerable situations or to avoid potentially disastrous outcomes.

Before turning to a discussion of the four crisis-learning games, the reader should be alerted to some of the limitations in the crisis models as they are presented. First, all of the assumptions in the study are limited to crises between states that are relatively evenly matched in usable military capabilities and their perceptions of the gravity of the values at stake. This is not an uncommon phenomenon in recurrent crises, but it does exclude those cases where there are significant differences in capabilities or motivation. More problematic is the assumption that each of the players possesses complete information regarding the preferences of the other player. Real-world policymakers must make subjective estimates of the preference orderings of the other party; in fact, part of the bargaining process is designed to discover those preferences (See Snyder and Diesing 1977, chap. 2). The assumption of complete information is a simplification designed to clarify the essential components of the games. The transformation of the models to games of incomplete information, however, is a straightforward process. An attempt also has been made to alert the reader at those points, as in the discussion of "brinkmanship," where the assumption of complete information becomes especially problematic.

Finally, there is the very simplicity of the strategic choices and outcomes presented in the game matrices. Obviously, the range of choices and outcomes facing real-world policymakers is more complex. The simple typology of choices parallels

those presented in the data-based study (Leng 1983). As such, it depicts the most salient aspects of the crisis-learning model, rather than a richly descriptive image of real-world crisis bargaining. A more complex representation might be appropriate in later refinements of the model, but at this stage of the research it could obscure the fundamental relationships of interest.<sup>2</sup>

Beginning with the asymmetric nonnuclear crisis-learning game depicted in Table 1, I will describe the sequence of moves and outcomes in which the initial moves are based on the REL prescription.

### Four Crisis-Learning Games

#### Asymmetrical Nonnuclear Game

The game in Table 1 assumes that the preceding crisis ended in a diplomatic victory for Row and a diplomatic defeat for Column. As noted above, the outcomes are ranked according to the players' preference orderings, with nine representing the most-preferred outcome and one the least-preferred outcome. The players do not possess usable nuclear weapons; therefore each prefers war (EE) over losing to the other player.

The simultaneous opening moves place the game at (1,7). Row's choices are to move to (2,8) or (4,5), or stay at (1,7). The rational choice is to move to (4,5). The (4,5) outcome is not only a Nash equilibrium, which would mark the end of the game if it were played myopically, it is also a nonmyopic equilibrium as defined by Kilgour (1985); that is, even with the capacity to look ahead to the final outcome, neither player could improve its position by departing from (4,5). Nor could Row have done better with a different choice at (1,7). Staying at (1,7) would yield Row's worst outcome. If Row moved to (2,8), a nonmyopic Column would stay at that outcome and obtain its second-best pay-off. (Moving to [3,9]

would allow Row to move to [8,2], etc.) Played rationally by both players, the game ends at (4,5) if it is played either myopically or nonmyopically.

In this game it is easy to play through the possibilities to see the final nonmyopic outcome, but that is not always the case; therefore, the sequences of nonmyopic play for each of the four games has been computed by machine, employing a program developed by Kilgour (1985).

The move by Row to (4,5) is consistent with REL reasoning, that is, that a more coercive strategy by Column is most likely to evoke a response in kind from Row. The two sides then become locked in to mutually coercive strategies that cause the crisis to escalate to war. It is not hard to envision real-world illustrations of such a pattern. A classic example would be the Russians (diplomatic losers to the Austrians in two crises in the Balkans) and Austria on the eve of World War I. Given the pay-offs in Table 1, the war (4,5) outcome can be avoided only if both players choose C as their initial strategies. That would contradict assumptions of the REL model, but it does suggest the critical nature of the initial strategy choices, an issue to which I will return.

#### Symmetrical Nonnuclear Game

The symmetrical game occurs when the previous crisis ended in a compromise, stalemate, or war.<sup>3</sup> We will begin the examination of this type with the assumption that neither player was satisfied by the outcome, so that each is inclined to attempt a more coercive strategy in the next crisis. There is no identifiable winner or loser, so the preference orderings of the players are mirror images of each other, with equal pay-offs for CC, SS, and EE, as shown in Table 2. Either player can make the first move from the initial outcome. From there on, the moves alternate as in the asymmetrical game.

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In those cases where both sides were dissatisfied with the outcome of the previous crisis, each starts with strategy E, so that the initial outcome is (4,4). It is also the final outcome, as (4,4) is a Nash equilibrium and nonmyopic equilibrium, signifying the termination of either a myopic or nonmyopic game. In fact, if *either* player chooses E as its beginning strategy, the game will end at the EE outcome. The outcome highlights the consequences of the only nonrational component of the game—the initial moves based on REL reasoning. Unhappy with the outcome of the preceding crisis, perhaps goaded on by publics demanding more assertive action, policymakers on both sides turn to initially more coercive bargaining strategies that result in swift escalation to war. A good illustration of this phenomenon occurred in the Indo-Pakistani crisis preceding the Second Kashmir War in 1965. It had been preceded by a cease-fire and diplomatic stalemate in 1948 that left both sides frustrated. When the smoldering conflict rekindled into a crisis in 1965, it swiftly escalated to war.

If both players *were* satisfied with the outcome of the previous crisis, so that each repeated its earlier strategy (S), the initial outcome would be (5,5). Played myopically, the game still ends at (4,4), but if it is played nonmyopically the final outcome becomes (5,5); that is, if Row has enough foresight to look ahead just one move, it will resist the move to (7,1), recognizing that Column will respond by moving to (4,4); instead, Row will stay at (5,5), which is a nonmyopic equilibrium.

A third nonmyopic equilibrium, which is Pareto-superior, occurs at (6,6), but this obtains only if both players begin the game with C strategies, a condition that runs contrary to the assumptions of the REL model. In fact, provided that neither player begins the game with strategy E, the game will yield at least the (5,5) outcome.

**Table 2. Symmetrical Nonnuclear Game**

Row	Column		
	C	S	E
C	6,6	3,9	2,8
S	9,3	5,5	1,7
E	8,2	7,1	4,4

### Asymmetrical Nuclear Game

The asymmetrical nuclear game that appears in Table 3 starts from the same preference orderings as the asymmetrical nonnuclear game, except that the EE outcome (nuclear war) now represents the worst crisis outcome for either player.

The game begins at (2,7), with previous winner Row using the same strategy as in the preceding crisis (S), and previous loser Column using a more coercive strategy (E). Played myopically, this game cycles through the upper right-hand corner of the matrix in a counterclockwise fashion. Row chooses (3,8) over either (1,1) or staying at (2,7), prompting Column to move to (4,9); Row moves to (8,3), and Column moves back to (2,7) to restart the cycle. The game ends in a stalemate, a frustrating, but safe, outcome.

This would also be the outcome of a nonmyopic game if we interpreted a move by either party to (1,1) to result in nuclear war, as the rational players would avoid that suicidal outcome. I interpret a move to (1,1), however, not as the outbreak of

**Table 3. Asymmetrical Nuclear Game**

Row	Column		
	C	S	E
C	5,6	4,9	3,8
S	9,5	8,3	2,7
E	7,4	6,2	1,1

nuclear war but as a commitment to go to war (in this case, nuclear war), failing a more cooperative move by the other party; just as a move to (6,6) would represent a commitment to a compromise, provided the other party did not move away from that outcome.<sup>4</sup> Following the rules of a sequential game, the outcomes obtain only if the player with the option to depart from the outcome decides to stay. Under these circumstances, the best choice for Row is to move from (2,7) to (1,1), thereby forcing Column to choose between staying at (1,1) or moving to (6,2) or (7,4). A rational Column will depart and, after seven moves, the game ends at (9,5).

Row's move to (1,1) to turn a stalemate into victory, suggests an act of "brinkmanship," that is, the creation of a shared risk of nuclear war, with the responsibility for avoiding the mutual disaster shifted to Column. In fact, the asymmetrical nuclear game offers some interesting insights into real-world nuclear crises, but it also demonstrates some of the limitations of viewing real world crises as sequential games of complete information, particularly with regard to the use of brinkmanship.

Played myopically, the asymmetric nuclear game bears some resemblance to the Berlin Wall crisis of 1961, with Row representing the United States and its Western allies, and Column representing the Soviet Union. The initial Soviet strategy was decidedly coercive (an ultimatum on Western rights in West Berlin); whereas the West began by expressing its determination to defend its access to Berlin, much in the manner of the 1948 crisis. The 1961 crisis then cycled back and forth between cooperative and coercive moves by both sides. The United States did not resort to an act of brinkmanship to break out of the cycle, and the crisis finally died down without any settlement. Most observers saw the outcome as falling somewhere between a stalemate and a Soviet victory.

The nonmyopic version of the game, which includes the move through (1,1), raises the issue of brinkmanship. Not coincidentally, the United States did employ brinkmanship in its next crisis with the Soviet Union in 1962. The Cuban crisis has been subjected to innumerable analyses, and Brams (1985, 47-62) has demonstrated how different hypotheses regarding the key decision makers' perceptions of the pay-offs associated with alternative strategies can lead to significantly different interpretations of the game. It is, nonetheless, worth a digression here, to consider the implications of the use of brinkmanship, and some of the descriptive limitations of a sequential game played with complete information.

The asymmetrical nuclear game grants the brinkmanship option to the party in the most defensive position, that is, the party that must respond to the coercive initiative of the other. This is consistent with the notion that the defender of the status quo has an edge in motivation that makes resort to brinkmanship on its part a more credible strategy.<sup>5</sup> In fact, in the Cuban missile crisis, virtually all of the elements in the balance of motivation favored the U.S. side: the United States was defender of the status quo, its security was more immediately threatened, and it was the relative loser of the last crisis.<sup>6</sup> The United States could make a case for resorting to brinkmanship that could not be made by the Soviets, nor could it have been made by the United States in the 1961 Berlin crisis, when the United States would have been on the light side in both motivation and usable military capability.

The sequential game with complete information, however, disguises other aspects of brinkmanship that contribute to the enormous risks it entails. When Row moves to (1,1) in the crisis game, it shifts the responsibility to Column for triggering or avoiding a nuclear war. This is not inconceivable in the real world; in fact, Schelling (1966) has argued that

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Kennedy did just that when he publicly committed the United States to defend the quarantine during the Cuban missile crisis. But Kennedy's commitment was not truly irrevocable, and it is hard to imagine instances in real-world crises where either party would deliberately close off all possibilities of retreating from the brink of nuclear war. Moreover, there is always the risk that one party will not move because it assumes that the other is bluffing. In the sequential game, Column must make the next move or trigger nuclear war. The assumption of complete information means Row knows that and also knows that Column will move rather than accept its least-preferred outcome. That, of course, is not true in the real world where policymakers considering an act of brinkmanship must make subjective estimates of the other party's preferences, and of the other party's estimates of their preferences. Finally, there is the question of how much time the parties have before it is too late to move from (1,1).

It would not be difficult to alter the model to reflect these uncertainties. A straightforward approach would be to treat the situation as a game of incomplete information, with probability estimates of the likelihood that the player with the opportunity to move from (1,1)—or both players if one wished to modify the theory of moves to give each the opportunity to depart from (1,1)—would actually do so. A random element could be added to the calculation to include the possibility of the players running out of time, or accidentally "slipping over the brink." Modifications along these lines would more accurately model the concept of "shared risk" that lies behind the notion of brinkmanship (see Powell 1988). Such refinements would be appropriate at a later stage of the development of the models; at this point, however, they would detract from one of my basic objectives—to illustrate the effects of initial

**Table 4. Symmetrical Nuclear Game**

Row	Column		
	C	S	E
C	6,6	4,9	3,8
S	9,4	5,5	2,7
E	8,3	7,2	1,1

moves on the dynamics of the games, even when the players are equipped with the farseeing rationality that obtains in a non-myopic game with complete information.

### **Symmetrical Nuclear Game**

The game matrix for a symmetrical nuclear learning game, in which the players hold the same views regarding the outcome of the preceding crisis, appears in Table 4.

The game is most interesting in those cases where both parties were unhappy with the outcome of the preceding crisis. With both players turning to more coercive bargaining strategies, the initial outcome is at (1,1). The game cycles if it is played myopically; however, this avoids the nuclear war outcome, with the crisis ending in a stalemate. For example, if Row makes the first move away from (1,1), the outcomes will be (3,8), (4,9), (7,2), (8,3), (9,4), (2,7), (3,8), and so on.

Played nonmyopically, the game has an intriguing property. The first player to depart from (1,1) is at a disadvantage. If Row moves to (3,8), Column's best choice is to stay and end the game at that outcome. If Column has the first move and departs to (8,3), Row's best choice is to stay and end the game at that outcome. The potentially disastrous (1,1) outcome gives the symmetrical nuclear learning game properties similar to that of chicken.<sup>7</sup> As a sequential game, it nicely illustrates the dynamics of the original teenage game of chicken played with



automobiles. The game begins with both players headed on a collision course; the first to swerve from the path to mutual disaster loses. The dynamics of chicken as a two-by-two sequential game have been analyzed extensively by Brams (1983, 1985), so the discussion here will be directed to its implications for the REL model.

As posited by the REL model, the initial outcome is at (1,1) because both parties, taking a realpolitik perspective, assume that the failure to achieve their goals in the previous crisis resulted from a failure to demonstrate adequately their resolve. Each begins the crisis determined to make the other side "blink" first. If both remain determined, the nuclear disaster occurs. If not, the first party to move to a more cooperative bargaining strategy loses. Increasing the credibility of one's commitment to stay at (1,1) increases the likelihood of winning if the opponent lacks resolve, but it also makes it more difficult for one to retreat if the opponent does not. As in the asymmetrical nuclear game, a more accurate depiction of the desperate risks involved in this game could be obtained by transforming it into a game of incomplete information, with each side forced to make probability estimates of the likelihood of the other party moving from (1,1), along with a random element to represent the danger of both parties waiting too long before moving. But for our immediate purposes, the lesson behind the game as it now stands is clear enough: the desperate situation is the result of where the game begins, that is, with each party choosing a more coercive initial bargaining strategy.

There are two nonmyopic equilibria in the game, at (5,5) and (6,6). The (5,5) equilibrium can be reached if neither player chooses strategy E as an opening move. The (6,6) outcome can be reached only if both players choose C as their opening strategy. As in the other four types of crisis-learning models, the

players can achieve a mutually beneficial outcome only if the "lesson" from the preceding crisis that the REL model says is most likely to be learned—for dissatisfied players to begin with more coercive strategies in the next encounter—is rejected by both players.

### Summary

The notion that an unsatisfactory outcome to the preceding crisis will encourage decision makers to adopt more coercive bargaining strategies in the next confrontation lies at the heart of the REL model of state behavior in recurrent crises. A data-based study (Leng 1983) of six pairs of nations in three successive crises each offered empirical evidence to support the descriptive validity of the proposition, while suggesting that it was a recipe for disaster.

The examination of the dynamics of the four crisis-learning games reinforces that impression. Even with the deterministic element of the REL model—which posits that resorting to a more coercive bargaining strategy by one party will cause the other to follow suit—removed, these are dangerous games. In each of the two non-nuclear games, a final outcome of escalation to war (EE) is inescapable if *either* party begins the game with a more coercive (E) bargaining strategy. The situation hardly improves in the two nuclear games. Played myopically, the asymmetrical nuclear game cycles to a stalemate. The stalemate can be avoided if the game is played nonmyopically but only through an act of brinkmanship on the part of Row. And the symmetric nuclear game begins on a collision course, with the first player to depart from EE the loser. In none of the games is the player who begins with a more coercive bargaining strategy than it employed in the previous crisis at an advantage.

## Crisis Learning Games

### Finding the Best Strategy

What is the best opening strategy? To examine this question, we will drop the fixed opening moves prescribed by the REL model, along with the requirement that the previous winner have the first opportunity to move from the initial outcome. Then we will consider the final nonmyopic outcome for each possible opening move, assuming that either player can change its initial strategy and set in motion the sequence of alternating moves (Brams 1983, 75-79). If the final outcome varies according to which player departs from the initial outcome, two sets of final outcomes will be presented for that cell in the matrix, one for each player making the first move. In these cases, the final outcome with Row making the first move will be listed first.

### Rational Bargaining

In effect, we will be considering the games strictly from the perspective of rational calculations, with the REL assumptions removed. The game begins with both players aware that they have become embroiled in another crisis and each choosing among the three possible strategy choices. What remains are the realpolitik assumptions at the heart of the pay-off matrix, with more coercive (E) strategies prevailing over repeat (S) strategies, and S strategies prevailing over changes to more cooperative (C) strategies.

Table 5 presents the original and final outcome matrices for game A, the asymmetrical nonnuclear game.

The cell entries in the final outcome matrix in Table 5 indicate where the game would end, provided that the initial outcome was in that cell. The upper half of the table simply reproduces the initial outcome matrix appearing in Table 1.

The dominant strategy for either player in the asymmetric nonnuclear game is C,

**Table 5. Asymmetrical Nonnuclear Game, Original and Final Outcome Matrices**

Row	Column		
	C	S	E
Game Matrix			
C	5,6	3,9	2,8
S	9,4	8,2	1,7
E	7,3	6,1	4,5
Final Outcome Matrix			
C	5,6	4,5	4,5
S	4,5	4,5	4,5
E	4,5	4,5	4,5

the more cooperative strategy. If both players choose C, the initial outcome would be (5,6), where each of the nonmyopic players would choose to stay. Any other strategy choice by either player would produce a final outcome of (4,5). If, for example, the game started at SC, or (9,4), Column would depart to (1,7), and Row would move to (4,5), where the game would end.

C is also the best initial strategy in game B, the symmetrical nonnuclear game. Its initial and final outcome matrices appear in Table 6.

**Table 6. Symmetrical Nonnuclear Game, Original Game Matrix and Final Outcome Matrix**

Row	Column		
	C	S	E
Game Matrix			
C	6,6	3,9	2,8
S	9,3	5,5	1,7
E	8,2	7,1	4,4
Final Outcome Matrix			
C	6,6	5,5	4,4
S	5,5	5,5	4,4
E	4,4	4,4	4,4

Table 7. Asymmetrical Nuclear Game,  
Original Game Matrix  
and Final Outcome Matrix

Row	Column		
	C	S	E
Game Matrix			
C	5,6	4,9	3,8
S	9,5	8,3	2,7
E	7,4	6,2	1,1
Final Outcome Matrix			
C	9,5	9,5	9,5
S	9,5	9,5	9,5
E	9,5-3,8	9,5-3,8	3,8-9,5

Choosing C offers either player the possibility of the highest possible outcome with no greater danger than any other initial strategy. Although this game has some of the properties of prisoner's dilemma, the dynamics of the sequential game eliminate the danger of being "suckered" from being too cooperative. On the other hand, if either player begins with strategy E, the game ends at EE.

The outcome matrix for game C, the asymmetrical nuclear game, appears in Table 7.

This game is more complex. Convergence on final outcomes is not reached until after seven moves, and there are three instances in which the final outcome varies according to which player is the first to depart from the initial outcome. For example, if the initial outcome is EC, Row can receive its most-preferred outcome (9) if it is the first to depart, and Column can receive its second favored outcome (8) if it departs first.

Row can assure its best outcome by choosing either strategy C or strategy S. C would be preferred based on the secondary objective of minimizing the amount of coercion necessary to achieve the objective. The choices for Column, loser in the last crisis, are more intriguing. The balance of probable outcomes is

equal, but the ordering of the final outcomes across row E suggests an interesting property of the game. If Column chooses either C or S as an opening strategy when Row chooses E, then Column's success is dependent on making the first move away from the initial outcome to obtain a (3,8) final outcome. One could argue that in a real-world crisis, the expectation would be that Column would be the first to move, in order to respond to the challenge posed by Row's more coercive E strategy (Leng 1983; Snyder and Diesing 1977). That assumption, however, is not part of the game. A glance at the matrix suggests that both players have an incentive to be the first to depart from the initial outcome at either EC or ES.

If Column's initial strategy choice is C or S, its success or failure is at least in its own hands. On the other hand, if Column chooses strategy E, so that the initial outcome is at EE, Column's hopes for achieving its second-best pay-off are in the hands of Row. Row must depart first if Column is to avoid its fifth-best pay-off (9,5) or a disaster (1,1). Unfortunately for Column, the relative costs of being the first to depart from (1,1) do not suggest that Row—which would end up stuck with its seventh-best pay-off—would be more likely to move first. Certainly, in a real-world conflict there would be no reason for Column to choose this difficult problem in compulsion over the opportunity to have the possibility of success lie in its own actions by choosing strategy C or S.

But even if these more subtle distinctions are ignored, given the primary and secondary objectives of the game, Column should choose strategy C. The potential pay-off is equal to that of any other strategy, and the potential level of coercive action is lower than that of any other strategy.

Finally, as the final outcome matrix in Table 8 illustrates, strategy C is the domi-

## Crisis Learning Games

nant opening strategy for game D, the symmetrical nuclear game, with strategy S favored over strategy E.

The outcomes in both of the nuclear games are influenced by the presence of the brinkmanship option; however, actually taking this dangerous route can be avoided by farsighted players, provided that neither player begins the game with strategy E. To illustrate, consider the game matrix for the asymmetrical nuclear game in Table 7. If Column chooses an opening strategy of E and Row chooses S, resulting in an initial outcome of (2,7), Row's best farsighted move is to (1,1), to force Column to move to (7,4). In each of the nuclear games, moving from the initial outcome to (1,1) is always the best choice if the other player began the game with an E strategy.

Now suppose the initial outcome was the intersection of two S strategies, (8,3). If Column had the first opportunity to depart from the initial outcome, it could move to (2,7), but then Row would move to (1,1)—the brinkmanship option—and force Column to choose (7,4). This would allow Row to move the game to (9,5), where it would end. A farsighted Column player would be better-off moving directly from (8,3) to (9,5), consistent with the game's secondary objective—to achieve the pay-off at the lowest level of coercive bargaining. This would avoid the costs to its reputation for resolve that would be incurred in retreating from Row's act of brinkmanship.

What does this suggest about a real-world nuclear crisis? First, it highlights the importance of farsightedness, of looking ahead to avoid moves that will only add to the costs and risks of an inescapable outcome. As we have seen, opening the game with a more coercive strategy encourages the other player to respond in kind. Second, it illustrates the importance of understanding how each move affects the options available to the other side. It is important in the asymmetrical nuclear

**Table 8. Symmetrical Nuclear Game, Original Game Matrix and Final Outcome Matrix**

Row	Column		
	C	S	E
Game Matrix			
C	6,6	4,9	3,8
S	9,4	5,5	2,7
E	8,3	7,2	1,1
Final Outcome Matrix			
C	6,6	5,5	8,3
S	5,5	5,5	8,3
E	3,8	3,8	3,8-8,3

game, for example, that Column recognize that Row has no option other than a move to (1,1) should Column move from (8,3) to (2,7).

### Tit for Tat

By setting aside the REL prescription that parties dissatisfied with the outcome of the preceding crisis begin the next with a more coercive bargaining strategy, we found that the best initial bargaining strategy—for either party, in all four games—was to be more cooperative. It offered equal or higher pay-offs at lower levels of coercive behavior than either retaining the same bargaining strategy or moving to a more coercive strategy. Given the realpolitik design of the pay-off matrices, this is a surprising finding; but it is consistent with a number of studies suggesting the efficacy of the use of initially cooperative bargaining strategies in a wide range of conflict situations (Axelrod 1984; Esser and Komorita 1975; Osgood 1962), including interstate crises (Leng and Wheeler 1979).

The play of the games did not suggest that moving to a more coercive strategy was totally dysfunctional, rather that it was most effective as an option held in reserve as a potential response to a

similar move by the other player. In this respect, effective play in the sequential crisis-learning games is close to a "tit-for-tat" or reciprocating strategy—open with cooperation, then respond in kind to the other side's moves. A tit-for-tat strategy, in fact, does quite well in these games. In 20 of the 24 possible instances, a player using a simple tit-for-tat strategy will achieve the same final outcome as that obtained by playing the best possible mix of farsighted strategies illustrated in Tables 5–8. And of the four exceptions, two end in compromise outcomes at CC, which possess their own long-term benefits. Since real-world crises are not games of complete information, in which the players possess the knowledge of each other's outcome preferences and the farsightedness presumed available to the players in the crisis-learning games, tit-for-tat would appear to be the most feasible strategy.<sup>8</sup>

### Conclusions

The investigation of crisis-learning games has been a methodological departure from most game-theoretical examinations of crisis bargaining in that the foundation for the construction of the four crisis-learning games was laid in a data-based study of recurrent twentieth century crises (Leng 1983). Too often, formal modeling and data-based research have appeared to stand in opposition to each other. Yet, as the results of this study demonstrate, each has much to offer the other in the construction of more robust theories of interstate conflict behavior.

The set of hypotheses developed in the data-based study suggested the approach to modeling changes in the player's preferences from one crisis to the next, an area in which little previous work has been done by game theorists. By the same token, the formal modeling of the dynamics of the crisis-learning games offers new insights into the consequences of

the initial strategy choices indicated by the findings in the data-based study, as well as providing a means to explore the prescriptive questions that they raised. The use of sequential games, in which myopic and nonmyopic outcomes are compared, illustrates the importance of evaluating the unfolding consequences of bargaining strategies over the course of an evolving crisis.

The substantive conclusions to be drawn from the crisis-learning games are limited to crises between states of relatively even capabilities and motivations, as well as by the simplifying constraints of the models, which assume farsighted, rational play with complete information. Nonetheless, they are very suggestive not only in relation to the empirical study of bargaining in recurrent crises (Leng 1983) but also in light of comparable findings in studies of conflict bargaining more generally.

One such finding is the critical importance of the choice of initial bargaining strategy. The *realpolitik* experiential learning hypotheses posited that with the exception of crises ending in mutually satisfactory compromises, at least one party would begin with a more coercive bargaining strategy than it employed in the previous crisis. The other party, equally determined to demonstrate its resolve, would respond in kind, setting in motion a sequence of increasingly coercive threats and counterthreats. There is a depressingly deterministic quality to the hypotheses—and the findings that support them—in the data-based study. Yet when that requirement was relaxed after the initial outcome in the crisis-learning games, the players still did not fare well, despite having the advantage of complete information over the full sequence of moves and countermoves.

This brought us to the prescriptive question of what is the most effective bargaining strategy in a recurrent crisis between evenly matched adversaries. The

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## Crisis Learning Games

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best *initial* strategy choice turns out to be radically different from that prescribed by REL reasoning. The most effective initial strategy is to be more cooperative, a finding that is consistent with studies of conflict bargaining by social psychologists (Esser and Komorita 1975), and in repeated plays of prisoner's dilemma games (Axelrod 1984). These studies more generally suggested the efficacy of an accommodative opening move followed by tit-for-tat responses on subsequent moves. Tit-for-tat or "reciprocating" bargaining strategies have also been found to be superior to escalating coercive bargaining strategies in real-world crises (Leng 1984; Leng and Wheeler 1979) and in extended deterrence (Huth and Russett 1987). That turned out to be the case in the crisis learning games as well.

What can we conclude from all of this? The relative effectiveness of one bargaining strategy or another is dependent on what is signaled to the other party regarding one's intentions and the effect that the move has on the options available to each player. Most studies of crisis bargaining, whether prescribing a strong demonstration of resolve (Ellsberg 1959; Jervis 1978; Snyder and Diesing 1977), or the importance of signaling a willingness to reciprocate cooperative moves (Leng and Wheeler 1979), have focused on the importance of the *intentions* signaled to the other player. The sequential learning games illustrate the importance of the second factor: the effect that each move has on the players' remaining *options*. Although the players in the crisis-learning games were possessed of unlimited farsightedness and complete information regarding the other player's preferences, certain outcomes were foreclosed once particular opening moves had been made, so that less desirable options became the only feasible moves. Beginning with a more cooperative move and then moving to tit for tat significantly reduced that danger.

The problem of foreclosing options is not considered in the 1983 study from which the games were constructed; nor does it receive more than passing consideration in other data-based studies of crisis behavior; yet it has been a critical factor in policy-making in real-world crises from the 1914 crisis to the U.S.-Soviet alert crisis of 1973. Thus, the immediate policy relevance of the games lies in their demonstration of the critical importance of having the foresight to avoid moves that can foreclose desirable options and force policymakers to consider acts of brinkmanship or war. As a theory-building exercise, the games suggest the need for a closer look at the role of this phenomenon in the evolution of real-world crises, as well as its implications for the relative effectiveness of different bargaining strategies, especially tit for tat.

## Notes

I thank Steven J. Brams, D. Marc Kilgour, and Zeev Maoz for their comments on an earlier draft of this article.

1. Brams and Wittman (1981) do not assign a final outcome to any departure unless one player has the opportunity to end the game at its most preferred pay-off. Only then is an outcome declared nonmyopically stable, and only if both players achieve their most-preferred pay-offs is it declared a nonmyopic equilibrium. Kilgour (1985) relaxes this condition by basing the concepts of nonmyopic stability and equilibrium on a comparison of each player's pay-off at the current outcome with that at the final outcome, assuming extended foresight on the part of the players.

2. One approach to a more detailed representation of the range of choices and outcomes might be through a presentation of the games in extensive form. This, however, runs the risk of depicting—in bargaining strategy choice—a degree of precision in making fine quantitative judgments along a continuum of cooperation and conflict that may be inconsistent with the necessarily cruder choices of real-world policymakers.

3. Two-by-two games are defined as *symmetric* if the outcome ranks are the same for both players along the main diagonal, with the ranks along the off-diagonal mirror images of each other. Here the

term is used more loosely, as only the first condition is met.

4. The question of the circumstances under which party—if either—may move through (1,1) has been addressed by Zagare (1984) through adjustments in the theory of moves based on the strategic capabilities of the parties.

5. Brams and Hessel (1984) have introduced the notion of *threat power* to attempt to account for such real-world asymmetries in credibility in sequential games; however, the interesting aspect of the asymmetry in this instance is that it can occur as a result of the strategy choices in the course of the crisis, as opposed to being present at the outset.

6. In this last respect, the Cuban missile crisis does not fit the asymmetric game, which grants the brinkmanship option to the winner of the previous dispute in response to the loser's resort to a more coercive strategy.

7. The game is not a direct three-by-three analogue of the two-by-two game. Strategy E dominates C, but it does not dominate S.

8. The four exceptions: (1) With an initial outcome of (S,C) in the asymmetrical nonnuclear game, Column is better-off moving to strategy E on the first move rather than matching Row's strategy S; (2) the same situation obtains for Column in the asymmetrical nuclear game; (3) Row should depart from CC to SC on the first move in the asymmetrical nuclear game; (4) in the symmetrical nuclear game, with the initial outcome at CE, Row can do better by departing from tit for tat to move from EC to SC to achieve a final outcome of (9,5), rather than moving to CC for a (6,6) outcome.

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# THE CORE AND THE STABILITY OF GROUP CHOICE IN SPATIAL VOTING GAMES

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**T**he core of a voting game is the set of undominated outcomes, that is, those that once in place cannot be overturned. For spatial voting games, a core is structurally stable if it remains in existence even if there are small perturbations in the location of voter ideal points. While for simple majority rule a core will exist in games with more than one dimension only under extremely restrictive symmetry conditions, we show that, for certain supramajorities, a core must exist. We also provide conditions under which it is possible to construct a structurally stable core. If there are only a few dimensions, our results demonstrate the stability properties of such frequently used rules as two-thirds and three-fourths. We further explore the implications of our results for the nature of political stability by looking at outcomes in experimental spatial voting games and at Belgian cabinet formation in the late 1970s.

**T**wo of the most important questions in politics are the extent to which the outcome of collective-decision-making procedures can be expected to be (1) predictable and (2) stable. The game-theoretic notion of a core in principle provides important insight into both these questions. The core of a voting game is the set of undominated outcomes, that is, those that once in place, cannot be overturned. If games have a core, we expect that outcomes will be in the core and thus predictable. We consider those group-decision-making situations where the set of possible outcomes are points in (Euclidean)  $W$ -dimensional space. We shall refer to the phenomena we examine

as *spatial voting games*. In this context the conditions under which a core can exist are now well understood. Even when a core exists, however, it may be *structurally unstable*, in the case that arbitrary small changes in preference are sufficient to render the core empty. We shall argue that the existence of structurally stable core outcomes is of fundamental importance in understanding political decision making.

For a simple-majority-rule voting game with no ties, the core, if one exists, is simply the majority winner, that is, that alternative which can receive a majority in paired contest against each and every other alternative. The practical difficulty



is that under majority rule we expect in general there will be no core except under highly restrictive symmetry conditions (Davis, DeGroot, and Hinich 1972; Feld and Grofman 1987; Matthews 1980; McKelvey and Schofield 1987; McKelvey and Wendell 1976; Plott 1967; Schofield 1983a; Sloss 1973) or in the case that there is only one dimension (Black 1958). Subsequent work has deepened this pessimistic conclusion by showing that in general not only will there be no majority winner, but *all* alternatives will cycle with each other (Bell 1978; Cohen 1979; Cohen and Matthews 1980; McKelvey 1976, 1979; Schofield 1978a, 1978b, 1980, 1983a, 1985). Thus, by manipulating the agenda through restricting choices to some finite set to be voted upon in a specified order, *any* alternative can be made the group choice. This set of instability results is sometimes referred to as the "chaos theorems." (See the review in Riker 1980, 1982 for further discussion and alternative interpretations.)

Recent work has, however, provided grounds for a more optimistic view. There have been seven important lines of research. (Cf. an alternative typology in Grofman and Uhlaner 1985.)

The dominant line of research has been one that looks for *structure-induced equilibria*. Such equilibria occur because of imposed limitations on the set of alternatives that can be considered, for instance, a germaneness restriction, a closed rule, a mandatory final vote against the status quo, a budget constraint, a yes-no vote on an alternative proposed by an agenda setter, and so on (McCubbins and Schwartz 1985; Riker 1980; Romer and Rosenthal 1978; Shepsle 1979; Shepsle and Weingast 1981, 1984).

In another line of attack (Ferejohn, Fiorina, and Packel 1980; Ferejohn, McKelvey, and Packel 1984; Packel 1981), which has not yet been absorbed into the mainstream of the social choice literature, a group of scholars has shown that in

spatial voting games there may be *probabilistic* convergence of outcomes to a small and well-defined area of the space centered around what has been called the *yolk*. Closely related is the work on agenda construction that demonstrates, among other things, that agendas that move toward the center of the yolk are much easier to construct than agendas away from the center (Feld and Grofman 1986; Feld, Grofman, and Miller 1985).

A third promising line is work on the uncovered set and various subsets thereof (Banks 1985; Banks and Bordes 1987; Feld and Grofman n.d.; Feld et al. n.d.; McKelvey 1986; Miller 1977, 1980, 1983; Miller, Grofman, and Feld 1985; Moulin 1984; Shepsle and Weingast 1984). The uncovered set can be thought of as a weakening of the concept of the core. The *uncovered set* is the set of points that are majority-preferred to all other alternatives either directly or at one remove; that is, if  $x$  is uncovered, then for all  $y$  either  $x P y$  or there exists  $z$  such that  $x P z$  and  $z P y$ .

A fourth new approach has been to focus on the Copeland winner (also known as the strong point). The *Copeland winner* is the alternative that is defeated by the fewest other alternatives (Copeland 1951; Glazer, Grofman, and Owen 1985; Grofman 1972; Grofman et al. 1987; Henriot 1984; Owen and Shapley 1985; Straffin 1980). The strong point, too, can be thought of as a weakening of the concept of core.

A fifth line of research has been to look at *von Neumann-Morgenstern externally stable solution sets*, which have the property that for any alternative outside the set, there exists an alternative in the set that beats it. Wuffle et al. (n.d.) show that the V-M externally stable solution of minimal area can play a role analogous to that of the core and that it is identical to the core if one exists.

Riker's (1982) work on a "liberal" concept of democracy as a referendum on the

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status quo may be thought of as constituting a separate sixth line of attack. It involves "a conceptualization of the notion of democracy so that the chaos of the McKelvey-Schofield results is irrelevant" (Riker, personal communication, 1986).

The seventh line of research is one that looks at supramajoritarian decision making and the minmax set (for definition see next section). Early work in this tradition includes Craven 1971; Ferejohn and Grether 1974; Kramer 1977; Packel 1981; Simpson 1969; and Slutsky 1979. It is this seventh line of research with which we will deal.

Recent work in this research tradition (Greenberg 1979; McKelvey and Schofield 1986, 1987; Schofield 1983b, 1984a, 1984c, 1985, 1986; and Strnad 1985) has, for spatial voting games, found conditions (a) under which a core will be guaranteed to exist, and (b) such that it is possible to construct a core that will be structurally stable. The research above states results not just for supramajoritarian games but also for the widest possible class of noncollegial voting rules (where a *noncollegial voting rule* means one in which no one voter or set of voters has sufficient power to be in all winning coalitions). These papers are *quite* technical; they appear (with one exception) in economics journals and provide (with one partial exception) no discussion of any potential or actual empirical applications.

Our aim is threefold: *first*, to present the most empirically relevant of these results in a nontechnical fashion for a political science audience, since it will turn out that these results really are not hard to understand, even though they are quite hard to prove in their most general form; *second*, to extend the results by reformulating key theorems in a way which makes their practical significance clearer; in particular, we look at the stability implications of two-thirds and three-fourths rules in two and three

dimensions and at the stability characteristics of weighted voting and veto rules; *third*, to demonstrate the importance of these results for the understanding of actual group-decision processes by reanalyzing some recent experiments on spatial voting games (Fiorina and Plott 1978; Wilson and Herzberg 1984) and by offering an illustrative example of how we can model the stability of coalitions in multiparty coalition governments—looking at the 1978 Belgian political-party system.

Some of the most significant implications of the basic results we give are

1. When there are only two dimensions of choice, rules only marginally stronger (i.e., closer to unanimity) than a two-thirds rule will invariably give rise to a core, and rules that are only marginally stronger than simple majority may give rise to a structurally stable core if certain symmetry assumptions are met. Moreover, for two dimensions and few alternatives these conditions are not particularly stringent. In particular, in two dimensions, requiring only one voter above simple majority may make it possible to obtain a structurally stable core.
2. When there are only three dimensions of choice, rules only marginally stronger (i.e., closer to unanimity) than a three-fourths rule will invariably give rise to a core, and no rule that is less than two-thirds can give rise to a structurally stable core. Rules in between two-thirds and three-fourths may give rise to a structurally stable core if certain symmetry conditions are met.
3. Every voting situation in which there are one or more players with veto power has a core.
4. The core will always exist if the only feasible coalitions are those that are

connected (Axelrod 1970) with respect to some given single dimension.

### Basic Results about the Minmax Set and Structurally Stable Voting Rules

Let  $n$  be the number of voters. Let  $q$  be the number of votes needed to replace the status quo with a new alternative. We shall refer to such decision rules as *quorum rules* or *q-rules*. For  $n$  odd, if  $q = (n + 1)/2$  we have simply majority rule. If  $q > (n + 1)/2$ , that is, if decision making is supramajoritarian, then we may obtain a core. (Of course we must always obtain a core if  $q = n$ .)

To examine spatial voting games that are *q-rules*, we need to introduce a few concepts. We suppose that the set of outcomes is a subset of Euclidean space of dimension  $W$ . We generally assume the set is convex and compact (i.e., closed and bounded) unless we state otherwise. We also assume that individual preference is *convex* (i.e., for each alternative the set of preferred alternatives is a convex set).<sup>1</sup> To further simplify our exposition, we assume that the game is what is called a tournament; that is, there are no ties. Dealing with ties is, however, straightforward. For a given dimension  $W$  and society size  $n$ , we can find the *smallest* integer  $q$  such that any *q-rule*, with  $q' \geq q$ , must have a nonempty core. The core for the *q-rule* so defined is called the *minmax set* in dimension  $W$  (Simpson 1969; Kramer 1977).

Kramer (1977) showed that a sequential two-candidate spatial voting game, in which candidates sought to maximize their plurality, converged to outcomes in the minmax set. Let  $q^* = q/n$ ; that is, since  $q$  is the minimum number of players in a winning coalition then  $q^*$  is the size of the minimal winning majority expressed

as a proportion. Kramer (1977, 328, n. 4) conjectured that the smallest  $q^*$  that guarantees (i.e., is *sufficient* for) the existence of a core was given by  $W/(W + 1)$ , where  $W$  is the dimensionality of the space. We shall refer to the minimum  $q^*$  which guarantees the existence of a core as the *Kramer number*, and denote it  $q_k^*$ . Kramer's conjecture was proved by Greenberg (1979) and generalized further by Schofield (1983b, 1984a) and independently by Strnad (1985).

If a *q-rule* satisfies  $q/n < q_k^*$ , it is possible to show that there is a configuration of convex voter preferences such that the core is empty. However, it may also be possible to find a configuration of convex voter preferences for which a core *does* exist and is, moreover, structurally stable.

Results in Schofield (1986) can be used to prove a result closely related to Kramer's conjecture: for large electorates the minimum  $q^*$  for which there exists a configuration of voter preferences resulting in a structurally stable core is given by  $(W - 1)/W$ . We shall call this the *Schofield number*  $q_s^*$ . We shall also provide an exact expression that relates  $q$  and  $q_s^*$  to  $n$  and  $W$ .

In much of the literature, results are stated in terms of the lowest dimension in which instability occurs (the *instability dimension*). Here, we shall focus on finding what *q-rules* are sufficient for stability, or give rise to the possibility of a structurally stable core, when there are only a few dimensions. We believe such results are of considerable practical relevance, since much real-world decision making takes place with only a very limited set of issue dimensions under simultaneous consideration.

In this section we state in informal terms the key theorems and some implications derived from them.

**THEOREM 1.** *For any given  $n$ , the minimum value of  $q$  sufficient to guarantee the existence of a core in a *q-rule* game*

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is given by the smallest integer  $q$  such that

$$q > \frac{nW}{W+1} \quad (1)$$

Greenberg (1979) shows that for supra-majoritarian decision rules, the minimum dimensionality sufficient to guarantee a core is given by  $W < q/(n-q)$ . Theorem 1 follows after some elementary algebra.

Greenberg (1979) and Schofield (1984b) have also shown that if  $W$  is an integer and satisfies  $W \geq q(n-q)$  then it is possible to find a set of preferences such that the core is empty. Thus, if  $q \leq nW/(W+1)$ , the existence of a core cannot be guaranteed.

It is useful to express Theorem 1 in terms of proportions rather than absolute numbers. As before, let  $q^* = q/n$ .

**COROLLARY 1 TO THEOREM 1.** *For any given  $n$ , the Kramer number,  $q_k^*$ , i.e., the minimum special majority,  $q^*$ , sufficient to guarantee the existence of a core, is given by the smallest  $q_k^*$  which is a multiple of  $1/n$  such that*

$$q_k^* > \frac{W}{W+1} \quad (2)$$

*Proof.* Substitute  $q^* = q/n$  in Relationship 1. QED

**COROLLARY 2 TO THEOREM 1.** *For  $W = 1$ , the one-dimensional case, for any value of  $n$ , there exists a majority-rule core.*

*Proof.* Substituting  $W = 1$  in Relationship 1 we obtain  $n/2$ . The smallest integer greater than  $n/2$  is  $(n+2)/2$  for  $n$  even, and  $(n+1)/2$  for  $n$  odd. QED

This corollary is familiar as Black's famous (1958) median-voter theorem, since, in one dimension, for simple-majority rule, the ideal point of the median voter is in the core.

**COROLLARY 3 TO THEOREM 1.** *For  $W = 2$ ,*

*the two-dimensional case, for any value of  $n$ , if  $q^* > 2/3$  then a core is guaranteed.*

*Proof.* Substitute  $W = 2$  in Relationship 2. QED

Analogously, for 3 dimensions, a core is guaranteed if  $q^* > 3/4$ , and so on.

Theorem 1 provides conditions sufficient to guarantee the existence of a core for  $q$ -rules.

Even though a core cannot be guaranteed if  $q \leq nW/(W+1)$ , for a high enough  $q$ , a structurally stable core can be constructed by appropriately configuring voter preferences, as the following theorem proves:

**THEOREM 2.** *For any given  $n$ , any  $q$ -rule satisfying*

$$q \geq \frac{2 + n(W-1)}{W} \quad (3)$$

*a structurally stable core can be constructed.*

Schofield (1984b, 1986) shows that for supramajoritarian decision rules, a structurally stable core can be constructed for some configuration of preferences if

$$W \leq \frac{n-2}{n-q} \quad (4)$$

Theorem 2 follows after some simple algebra.

It is useful to express Theorem 2 in terms of proportions rather than absolute numbers. Let  $q^* = q/n$  as before.

**COROLLARY 1 TO THEOREM 2.** *For any given  $n$ , the Schofield number,  $q_s^*$ , i.e., the special majority,  $q^*$ , sufficiently large to permit the existence of a structurally stable core is given by the smallest  $q_s^*$  which is a multiple of  $1/n$  such that*

$$q_s^* \geq \frac{(W-1)n+2}{Wn} = \frac{W-1}{W} + \frac{2}{Wn}$$

*Proof.* This follows directly from Relationship 3 by substituting  $q = q^*n$ . However, we must be careful to confine ourselves to  $q^*$  which are multiples of  $1/n$ . QED

COROLLARY 2 TO THEOREM 2. For  $W \geq 2$ , then as  $n$  gets large, the Schofield number,  $q_s^*$ , is given by

$$\lim_{n \rightarrow \infty} q_s^* = \frac{W-1}{W}$$

*Proof.* Follows directly from Relationship 2. QED

COROLLARY 3 TO THEOREM 2. For  $W = 2$ , for given  $n$ , the smallest integer  $q$  sufficiently large to permit the existence of a structurally stable core is given by  $n/2 + 1$  for  $n$  even and  $(n+1)/2 + 1$  for  $n$  odd.

*Proof.* If  $n$  is even, Relationship 4 gives  $W \leq (n-2)/(n-q)$  or  $q \geq n - n/2 + 1$  in the case  $W = 2$ . Hence  $q \geq n/2 + 1$ . If  $n$  is odd, the largest integer greater than  $(n+2)/2$  is simply  $(n+1)/2 + 1 = (n+3)/2$ . QED

Corollary 3 to Theorem 2 shows that it may be possible to generate a structurally stable core in two dimensions with  $q$  only one more than simple majority.

More generally, Corollary 2 to Theorem 2 shows that for a very large electorate, it is always possible to construct a structurally stable core as long as a sufficiently high quorum rule  $(W-1)/W$  is chosen. For example, for  $W = 3$  we can always find a way to construct a structurally stable core if  $q^* > 2/3$  (i.e., we can always find a configuration of preferences in three-dimensions for which such a core will exist). It may be possible to find a structurally stable core for lower quorum rules, as the following necessary condition indicates.

THEOREM 3. For any given  $n$ , then for a structurally stable core to exist it is

necessary that

$$q \geq \frac{W+n-1}{2} \quad (5)$$

McKelvey and Schofield (1986) have shown that if  $W \geq 2q - n + 2$ , then a structurally stable core can never exist on a compact set of alternatives. Thus a necessary condition for a structurally stable core is  $W \leq 2q - n + 1$ . Rearranging gives the desired result.

For  $W = 2$ , Relationship 5 is simply  $q \geq (n+1)/2$ ; that is, in two dimensions with  $n$  odd a structurally stable core can occur for simple-majority rule. However, the techniques used by McKelvey and Schofield (1986) show that it must then lie on the boundary of the set of alternatives (see also McCubbins and Schwartz 1985). Moreover, McKelvey and Schofield (1986) show that if the set of alternatives has an empty boundary, then a structurally stable majority-rule core in two dimensions with  $n$  odd is impossible. More generally, if the set of alternatives is open, then a necessary condition for a structurally stable core is that  $q \geq (W+n)/2$ .

Define  $[(W+n)/2]$  to be the smallest integer greater than or equal to  $(W+n)/2$  and denote  $q_m^* = 1/n[(W+n)/2]$ , the McKelvey number.

Writing the Kramer, Schofield, and McKelvey numbers as  $q_k^*$ ,  $q_s^*$  and  $q_m^*$  note that  $q_k^* \geq q_s^* > q_m^*$  for any dimension and electorate size.

As yet it is not known whether  $q \geq (W+n)/2$  is sufficient in general to guarantee the possibility of a structurally stable core. In other words there are decision rules that lie between  $q_s^*$  and  $q_m^*$  for which we are not sure whether a structurally stable core configuration of voter preferences can be constructed.

Note that if a structurally stable core is possible, then the probability (P) of obtaining a core must be nonzero when preferences are randomly distributed. Thus we can rephrase the previous results as

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**Table 1. Supramajoritarian Decision Rules That Guarantee a Core and Those That Make Possible a Structurally Stable Core**

Number of Votes	W (Dimension)									
	1		2		3		4		$\infty$	
	$q_k^*$	$q_s^*$	$q_k^*$	$q_s^*$	$q_k^*$	$q_s^*$	$q_k^*$	$q_s^*$	...	$q_k^*$ $q_s^*$
3	2	2	3	2	3	3	3	3		
	3	3	3	3	3	3	3	3		
4	3	3	3	3	4	4	4	4		
	4	4	4	4	4	4	4	4		
5	3	3	4	4	4	4	5	4		
	5	5	5	5	5	5	5	5		
7	4	4	5	5	6	6	6	6		
	7	7	7	7	7	7	7	7		
9	5	5	7	6	7	7	8	8		
	9	9	9	9	9	9	9	9		
27	14	14	19	15	21	19	22	21		
	27	27	27	27	27	27	27	27		
43	22	22	29	23	33	30	35	33		
	43	43	43	43	43	43	43	43		
101	51	51	68	52	76	68	81	77		
	101	101	101	101	101	101	101	101		
501	251	251	335	252	376	335	401	377		
	501	501	501	501	501	501	501	501		
$\infty$	1	1	2	1	3	2	4	3	...	1   1
	2	2	3	2	4	3	5	4		

Note:  $q_k^*$  is the Kramer number, the decision rule that guarantees that a core will exist;  $q_s^*$  is the Schofield number, the decision rule such that a structurally stable core is possible for some configuration of voter preferences.

follows:  $P(\text{core} / q^* \geq q_k^*) = 1$ ;  $P(\text{core} / q^* \geq q_s^*) > 0$ ; and  $P(\text{core} / q^* < q_m^*) = 0$ .

In Table 1, for given values of  $n$  and  $W$ , we show the Kramer number (i.e., the values of  $q^*$  sufficient for a core) and the Schofield number (i.e., the values of  $q^*$  for which it is always possible to find a set of preferences with a structurally stable core). For example, if there are nine voters, then in two dimensions a core is guaranteed if  $q = 7$  (i.e., using a seven-ninths rule), while a structurally stable core is possible if  $q = 6$  (i.e., a two-thirds

rule). For the case of four voters in two dimensions, a three-fourths rule guarantees a core. In like manner we may read out results from Table 1 for any given values of  $W$ ,  $n$ , and  $q$ . Note that, as indicated above,  $q_k^* \geq q_s^*$  for all  $W$  and  $n$ .

We shall provide a graphical illustration of a game without a core, a game with a structurally unstable core, and a game with a core that is structurally stable. First, however, we look at some additional results for collective-decision rules in which not all players are weighted equally.

### Weighted Voting Games

Most political systems do not use majority rule for all decisions; not only do they require extraordinary majorities ( $q^*$  rules) for certain kinds of decisions (e.g., in situations ranging from constitutional changes to confirmation of ambassadorial appointments), but they also make use of hierarchies of committees or constituencies and veto and override procedures. Results similar to those in the theorems given above can be obtained for these more general kinds of voting rules (for work in this direction, see Schofield n.d.[a]). We provide below some important results for the class of weighted voting games.

In a *weighted voting game* each player,  $i$ , has a weight,  $p_i$ , and there is a quorum rule,  $q^*$ , such that a motion passes only if the sum of the weights of the players who support it is at least  $q^*$ . For convenience, we let  $\sum p_i = 1$ . For example, we may have a five-voter game where weights are  $4/11$ ,  $3/11$ ,  $2/11$ ,  $1/11$ , and  $1/11$ , respectively, with a simple majority, six-elevenths, needed for passage, or we might increase the number of votes needed for passage, say, to eight-elevenths. The first game is commonly denoted  $(6; 4, 3, 2, 1, 1)$ , and the second  $(8; 4, 3, 2, 1, 1)$ . In the first game, the extension of Greenberg's Theorem (Greenberg 1979) by Schofield (1985) shows that a core can only be guaranteed in one dimension but that a structurally stable core exists in two dimensions. (See Theorem 5 below.) In the second game, the player with a  $4/11$  weight belongs to every winning coalition, and so that player's most preferred point is the core (see Theorem 4 below).

Weighted voting games are a much more general concept than they might first appear. Almost all multitiered representation systems with veto power or veto power with override that are in common use can be reexpressed as weighted voting games (Brams 1975; Straffin 1980). For

example, in the UN Security Council, the Big Five (U.S., USSR, Great Britain, France, China) have only one vote apiece but they also have veto power (a negative vote cast by any of them defeats any motion); the 10 other members of the Security Council have 1 vote, and, absent a veto, 9 of 15 votes are needed for passage. This voting situation can be reexpressed in weighted-voting terms as  $(39; 7, 7, 7, 7, 7, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1)$ . Note that we now have a thirty-nine forty-fifths  $q^*$  rule (Lucas 1976). To see that this weighted voting game is equivalent to the five-power veto, merely note that a bill can pass if and only if at least 9 countries vote for it, including all 5 of the big powers. Call a game with a veto player a *collegial game*.

In illustrating the existence of a core, it is often convenient to assume that each voter has preferences that can be represented in terms of the distance from the voter's ideal point. Call such a situation a *Euclidean voting game*. In general, when the core exists, it will be the *convex hull* of some set of voters' ideal points (i.e., the smallest convex set containing the particular points).

There is a simple result for weighted voting games that are collegial.

**THEOREM 4.** *All collegial weighted voting games have a core. If voter preferences are Euclidean, then the core contains the convex hull of the vetoers' ideal points. (In particular, games with a single veto player have the core located at that player's ideal point.)*

This result follows straightforwardly from Schofield (1985, Corollary 4.3.8).

For noncollegial weighted voting rules, a quite general result can be stated. First, a definition is essential.

**DEFINITION.** *The Nakamura number,  $N$  is the cardinality of the smallest set of winning coalitions with the property*

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that the intersection of its members is empty (Nakamura 1979; Schofield 1984a).

Since this definition is not at all obvious, an example will help. Consider the five-voter game  $(6;4,3,2,1,1)$ . Label the players A, B, C, D, and E in that order. The minimal winning coalitions are AB, AC, ADE, BCD, and BCE. Any two minimal winning coalitions have at least one member in common, that is, their intersection is nonempty. Such games are said to be *proper*. If, however, we consider the coalitions  $\{A,B\}$ ,  $\{A,C\}$  and  $\{B,C,D\}$ , these three have an empty intersection. With three minimal winning coalitions we can get a nonempty intersection, but with two coalitions the intersection is nonempty. Hence the Nakamura number for this example is 3.

**THEOREM 5.** *Let  $N$  be the Nakamura number. If  $N \geq W + 2$  then the core is nonempty.*

For a proof see Schofield 1984a.

To see how Theorem 5 generalizes Theorem 1, consider a  $q$ -rule with  $n = 5$  and  $q = 4$ . The winning coalitions  $\{ABCD\}$ ,  $\{ABCE\}$ ,  $\{ABDE\}$ ,  $\{ACDE\}$ , and  $\{BCDE\}$  do not intersect, and so  $N = 5$ . By Theorem 5 a core is guaranteed only if  $W \leq 3$ . Greenberg's result (Theorem 1) requires that  $W < 4/(5 - 4)$ ; that is  $W \leq 3$ .

For weighted voting games (and other more complex games), the Nakamura number tells us in what dimensions it will be possible to construct a core. The higher the Nakamura number, the higher the dimensionality in which outcomes of the game are predictable. For example, if  $N = 3$ , then a core can exist in only one dimension. By convention, if a game has a veto player, then we shall define the Nakamura number to be infinity. Hence, if a game has a veto player, by Theorem 5 it always has a core regardless of the dimen-

sionality of the space; that is, Theorem 4 can be taken to be a direct corollary of Theorem 5.

One important way in which a core can be created in weighted voting games is if certain coalitions are prohibited. This can have the effect at making the game collegial. For example, take the game  $(6;4,3,2,1,1)$  that we previously mentioned. If we now require that the only coalitions that can form are those that are connected along the  $ABCDE$  dimension (i.e., if, say, A and C are in a coalition, then B, who is between them, must also be in that coalition) then the only minimal winning (connected) coalitions are  $\{A,B\}$  and  $\{B,C,D\}$  (see Axelrod 1970 and Grofman 1982). Hence the game is now collegial. Let us take a further example  $(9;4,3,2,1,1,1,1)$ : if we imposed a connectedness requirement, then the only feasible coalitions would be  $\{A,B,C\}$  and  $\{B,C,D,E,F,G\}$  since most of the (minimal) winning coalitions are not connected. Again, we would have a collegial voting game and thus a core.

**COROLLARY 1 TO THEOREM 5.** *If the only feasible winning coalitions are those that are connected in some given dimension and the game is proper (i.e., the complement of any winning coalition is losing), then there always exists a core.*

*Outline of Proof.* Since the game is proper, the Nakamura number is at least 3, so by Theorem 5 there always exists a core in one dimension (as in Black 1958). But then there exists at least one player who belongs to every connected feasible winning coalition. Hence the game is collegial, and by Theorem 4 the core is nonempty. QED

Many examples where certain coalitions are "ruled out" occur in real life; for instance, the juxtaposition of ideological extremes in the same coalition is quite unlikely and certain parties (e.g., the



Communists and the far right in many European countries) are not regarded as possible coalition partners. Also, some parties choose not to be part of the governing coalition. Reducing the number of relevant players in the set of feasible winning coalitions increases the likelihood that winning coalitions will intersect. That is to say, the existence of parties who choose to remain outside the government may increase the size of the Nakamura number and thus promote stability. More generally, the fewer the parties who enter the coalition game, the more likely the game is to be collegial. This line of argument indicates that a multiplicity of parties intent on entering government can make it less likely that there will be a core—and the absence of a core makes for political instability in the form of an absence of enforceable political consensus.

Stability may also occur if groups in a weighted-voting-rule setting (of which simple-majority rule is a special case) operate under a *de facto* rule that is more than the *de jure* rule. Then, such a consensus-oriented norm (even though slight) may allow a core to exist. For example, while  $(8;4,3,2,1,1,1,1)$  need not have a core in two dimensions,  $(9;4,3,2,1,1,1,1)$  does, as we shall show below. Such pressure toward larger than minimal majority coalitions may occur if certain decisions, such as veto override, require more than simple majority or if coalitional allegiances are uncertain or simply if social norms exist that lead voters to try to seek consensus in order to avoid conflict. More generally, Schofield (1986) has conjectured that the imposition of multiple tiers or other forms of complex (but noncollegial) weighted voting rules will make likely the existence of a structurally stable core even in issue spaces of relatively high dimensions.

When there are several key actors who are found in almost all minimal winning coalitions, the Nakamura number will be relatively high, so we will be able to con-

struct a core in two or perhaps even three or more dimensions. We provide an example of such a game:  $(9;4,3,2,1,1,1,1)$  with players A, B, C, D, E, F, G. Here the minimal winning coalitions are ABC, ABDE, ABDF, ABDG, ABEG, ABFG, ACDEF, ACDEG, ACDFG, ACEFG, and BCDEFG. Any two or any three minimal winning coalitions have a nonempty intersection. But, for example,  $\{A,B,C\}$ ,  $\{A,B,D,E\}$ ,  $\{A,C,D,E,F\}$  and  $\{B,C,D,E,E,G\}$  have an empty intersection. Hence the Nakamura number is 4. Since  $4 - 2 = 2$ , there must exist a nonempty core in two dimensions.

One further observation worth emphasizing is that, for Euclidean preferences, in the highest dimension that a structurally stable core for a particular game is possible, the core may be at the preferred point of one of the players, that is, the minmax set will be a point. In general that player will be the most powerful player in the game. We now discuss an example to illustrate this possibility and also provide an example of a weighted voting game that occurred in Belgium in 1978 for which no majority-rule core exists. We then show how a structurally stable core for this game can be created in two dimensions.

### Applications of Basic Results to Experimental Games and Cabinet Formation

Our results suggest that at least when there are only a few dimensions (e.g.,  $W = 2$  or 3), social-decision processes are apt to result in stable choice within a delimited area of the space if groups use a supramajoritarian procedure or a weighted voting rule. For example, in each of three games with equally weighted players where there is no majority-rule core—a game in two dimensions with five players used in experiments by Wilson and Herzberg (1984, Fig. 3), a game in

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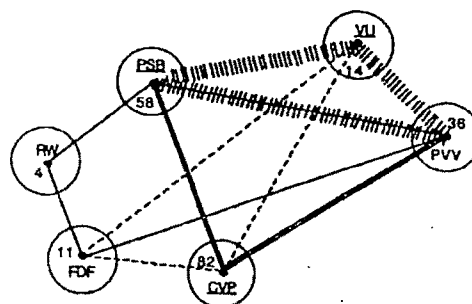
two dimensions with seven players considered in Kramer (1977, 321), and another two-dimensional five-voter game, Game 3 of the experiments by Fiorina and Plott (1978, 579, Fig. 3)—the minmax set appears to have useful predictive powers.<sup>2</sup> Also, in all three games the minmax set is relatively small.<sup>3</sup> In each game we find that a rule of one more than simple majority gives rise to a core, since  $4/5 > 2/3$  and  $5/7 > 2/3$  (see Corollary 2 to Theorem 1).

We now wish briefly to consider the results of the 1978 Belgian general election. We show in Figure 1 a two-dimensional spatial map of the Belgian party system in 1978 based on data prepared by Hearl under the auspices of the European Party Manifestos Project directed by Ian Budge (Budge, Robertson and Hearl 1987).

The core cannot be guaranteed in two-space in that election since three of the minimal coalitions, namely {CVP, PSB}, {CVP, PVV}, and {VU, PVV, PSB} have an empty intersection. Hence the Nakamura number is 3, and by Theorem 5, the core is guaranteed only in one dimension. For purposes of exposition we shall assume that each party has Euclidean preferences based on its ideal point. In Figure 1 we associate to each winning coalition the convex set generated from its members' ideal points. For the core to be nonempty, the five relevant sets must intersect, which they clearly do not.

Another way to see this is to use the notion of "median lines." Consider the line through the VU and CVP positions. The parties on and to the "left" of this line comprise a winning coalition with 169 seats, while the parties on and to the right of the line also comprise a winning coalition with 132 seats. Such a line is a median line, and for the core to be nonempty it must lie on the intersection of all median lines. However, the median lines VU-CVP, PVV-FDF and CVP-PSB do not intersect. (For a nontechnical exposition of the geometry, see Feld and Grof-

**Figure 1. Belgian Political-Party Space and Seats Held in 1978**



PSB	Socialist party
VU	Volksunie
FDF	Francophone Front
RW	Rassemblement Wallon
PVV	Liberals
CVP	Christelijke Volksparti

### Minimal Winning Coalitions

CVP, PSB	140 seats
CVP, PVV	118 seats
RW, FDF, PVV, PSB	110 seats
PSB, VU, PVV	108 seats
FDF, CVP, VU	107 seats

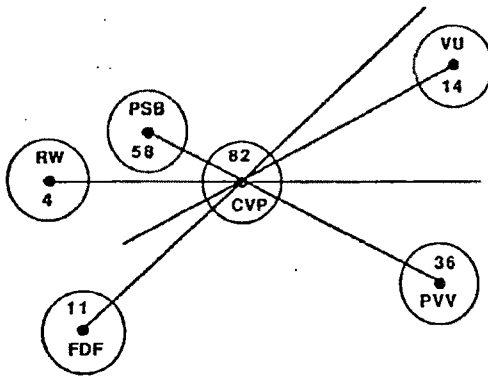
Source: Adapted from Derek Hearl, Belgium 1946-1981, in *Ideology, Strategy, and Party Change*, ed. Ian Budge, David Robertson, and Hearl (Cambridge: Cambridge University Press, 1987).

Note: Underlined parties in the figure are members of the cabinet (PSB and CVP). Independents (I, 3 seats) and Communists (KPB, 4 seats) are not shown.  $N = 212$ ; 107 seats are needed for majority.

man 1987. For an extension of this analysis to a situation with non-Euclidean preferences, see McKelvey and Schofield (1987).

However, if we move the CVP to be colinear with the PVV and the PSB, as shown in Figure 2, then there is a core at the location of the CVP. Note, however, that if the CVP position is moved slightly, then the core becomes empty since once again the set of median lines does not intersect at a single point. To see this, note that the line connecting the PSB and PVV remains a median line even though it

**Figure 2. Hypothetical Transformation of Figure 1 to Create a Structurally Unstable Majority-Rule Core**



no longer passes through CVP. Thus the core in Figure 2 is structurally unstable. However, we can move party locations so as to create a core. For example, if both the CVP and VU positions are changed, as in Figure 3, then there exists a structurally stable core at the preferred point of the CVP, because now the line between PSB and PVV is not a median line and all median lines pass through CVP.

There is some evidence that the parties tend to change position in this policy space, as though "hunting the core." We can see a possible reason for this phenomenon. In a situation such as represented in Figure 1, no matter which coalition were to form, there always exists another majority group that stands to

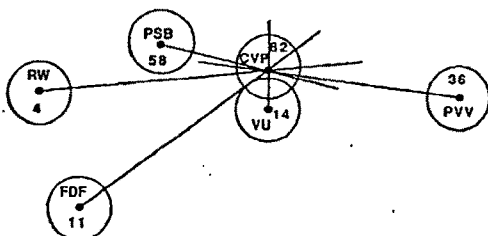
gain by bringing down the government. One would therefore expect governments to be quite short-lived. Indeed, in Belgium in the period since 1971, average coalition-government duration is 16 months (Schofield n.d.[b]). However, as Figures 2 and 3 illustrate, if the party positions were different, then a core could exist. Suppose the core were nonempty but structurally unstable, as in Figure 2. No winning coalition has in fact an incentive to bring down the government, but the smallest error in judgment or perception would lead parties to the belief that there were such an incentive. Only for the structurally stable core situation would one expect relatively long-lived governments.

Short-lived government does impose a cost on parties. If parties are willing to make a trade-off between their preferred policy positions and the advantages accruing from membership of government, then they may be aware of the fact that changing policy position somewhat can in fact lead to a core situation. Moreover, if one party makes the correct type of policy movement, then its preferred point would indeed be at the structurally stable core. In that case the party would know that it would belong to the government coalition and would also expect that the government coalition would be relatively long-lived. Thus the game has a high strategic context as parties become aware of the advantages of finding the structurally stable core.

Instead of moving party locations, another way to obtain a stable core is to impose a supramajoritarian rule on a coalition game. In particular, for Belgium in 1978, if we require 125 or so votes to have a "comfortable" (but not necessarily large) majority, then the only minimal winning coalitions (with Communists and independents excluded) are those that include the CVP.

A third way we can create a core is by treating one of the dimensions as more

**Figure 3. Hypothetical Transformation of Figure 1 to Create a Structurally Stable Core**



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## Spatial Voting Games

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important, and positing that all coalitions will be connected with respect to that dimension. In the example of Figure 1, if we collapse to the x-axis (i.e., look at party projections on that axis), then CVP is in the core.

A point worth noting from the Belgian case that we just examined is that the CVP is the only party whose preferred point can be a structurally stable core *point*. The next largest party, the PSB, with just under 30% of the seats, is too small to be in a structurally stable core position. (See Schofield 1986 for a fuller discussion of this example.) It is evident that this feature endows the CVP with far greater bargaining power than any other party.

To adapt Daalder's (1971) terminology, it is reasonable to identify the Belgian political system as *unipolar*. The same may be said of the Netherlands and Italy, as well as certain other countries. In some countries, on the other hand, the dimensionality of the policy space is such that *two* quite different structurally stable cores may occur, each one at the preferred position of one of two parties. Such a system could be called *bipolar*. This notion could be carried further to identify *multipolar* systems as those where three or more parties may occupy the structurally stable core position. All such parties could be regarded as being of equal power, irrespective of the differences in the actual number of seats they control. Note that this suggestion, if valid, gives a very precise method of formalizing the qualitative analysis of political party systems carried out by Daalder (1971), Sartori (1966) and others. In other words, by specifying how many of the parties in a given system could be core points, we can provide a nonarbitrary classification for party systems as unipolar, bipolar, tri-polar, and so on (see Schofield n.d.[a]).

The 1978 Belgium example is intended to be purely illustrative. We hope in further work to pursue the question of adequately modeling the dynamics of party

evolution over time (in Belgium and elsewhere) and to trace the link between the existence of core points in party spatial arrays and features of political life such as cabinet stability, voter volatility, and sharpness of ideological conflict.

## Discussion

We believe that understanding the conditions under which group choice can be expected to be stable is important. Policy cannot be intelligibly formulated or intelligibly implemented in an environment where decisions can always be revoked and no outcome is final. The results we have presented here suggest one important reason why, in the real world, stable outcomes occur. Outcomes in the minmax set offer the minimum consensus needed to achieve stability. Once an outcome in the minmax set has been picked, it is unlikely to be altered, especially if the core is structurally stable.<sup>4</sup>

Moreover, we believe that outcomes in the minmax set occur even in situations where the *formal* voting rule is one that is insufficient to give rise to a core. The reason for that view is quite simple. In real groups there is a reluctance to permit "bare"-majority decision making. Rather, consensus is sought (Zablocki, 1971, 155-58). Also in the political-party context, bare-minimum majorities are unsafe, because they are too vulnerable to blackmail through threats of defection. Thus, the *de facto* rule in group decision making is apt to be larger than the *de jure* rule.<sup>5</sup> This may also help explain why, in experiments on spatial voting games, outcomes may tend to be "tightly clustered" (Fiorina and Plott 1978, 590).

For decision processes where all voters have the same weight, we have shown that stability can occur with relative ease when the dimensionality of the space is low. In this case decision rules very near to bare majority may give rise to a struc-

turally stable core, and a two-thirds-plus rule (in the case of  $W = 2$ ), or a three-fourths-plus rule (in the case of  $W = 3$ ) will *guarantee* a core. We believe this finding can account in part for the popularity of such supramajoritarian rules. We have also shown that, even if the dimensionality of the space is relatively high, then (as with the UN Security Council example) veto rules or other special features may create either collegial voting rules or weighted voting games whose spatial array is such as to give rise to a core. These results significantly ameliorate the pessimism that seemed warranted by the generic instability results for simple-majority rule.

Finally, we have suggested ways in which the structural features of voting games can be used to provide a non-arbitrary classification of party systems as unipolar, bipolar, and so on. Also, we have identified hypotheses about electoral dynamics and cabinet longevity that are based on the spatial array and relative strength of political parties and the conditions of a structurally stable core, which suggests a new and promising line of research on a topic that has been much studied but is still not well understood (Grofman n.d.; Schofield n.d.[a]).

## Notes

A portion of this research was conducted while Schofield was the Sherman Fairchild Scholar in the Division of Humanities and Social Sciences, California Institute of Technology. The work has benefited from conversations with Charles Plott and earlier collaborative research with Richard McKelvey. We are indebted to the staff at the Word Processing Center, School of Social Sciences, University of California, Irvine for typing earlier drafts of this manuscript; to Cheryl Larsson for preparation of the figures; and to Dorothy Gormick for bibliographic assistance. This research was partially supported by NSF Grant no. SES 84-18295 to Schofield, and no. SES 85-06376, Program in Decision and Management Science, to Grofman. It was prepared for presentation at the International Conference on European Cabinet Coalitions, European

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1. If the convexity and compactness assumptions are dropped, then a "local core" is obtained. See Kramer and Klevorick 1974 and Schofield 1985 for the technical details. That readers grasp the exact nature of the compactness and convexity assumptions is not at all critical for understanding what follows. Readers may act as if all results refer to the familiar Cartesian coordinate space, thereby treating distance between alternatives as if that distance satisfied those properties it would satisfy in ordinary life.

2. Wilson and Herzberg (1984) in their experiments on five-voter spatial games used both simple-majority rule and a four-fifths rule. In both cases the majority of the outcomes was either within the minmax set (10 of 18 cases for the four-fifths rule, 8 of 18 for the three-fifths rule) or very near to it (an additional 6 of 18 cases for the four-fifths rule and an additional 7 of 18 cases for the three-fifths rule). Moreover, in the Wilson and Herzberg (1984) experiments, of the few outcomes that were not in or near the minmax set, half occurred as outcomes that were imposed when time ran out.

In the Fiorina and Plott (1978) experiment of Game 3, while only 3 of 15 observations fall directly within the minmax set, if we enlarge the bounds of the minmax set slightly, we can form a polygon in which 13 of the 15 observations fall. Even the remaining two observations are not so far away from the minmax set. Indeed, the mean outcome (45,62) in this voting game is within the minmax set (albeit barely). Because Fiorina and Plott (1978) counted success only when an outcome was inside the minmax set, they were not impressed with the fit of the minmax set to their data (cf. Ferejohn and Fiorina 1975). Although they concluded that "the minmax set does best among the models considered" (Fiorina and Plott 1978, 592), they then went on to say that was "only because of very weak competitors." However, there were 14 of these so-called "weak" competitors and we believe that *visually*—the most powerful statistical test known—the Fiorina and Plott experiments show the minmax set to fit the data quite well (see Fiorina and Plott 1978, 589, Fig. 8).

Also, we should note that other explanations of the location of outcomes in the Fiorina and Plott (1978) experiments on games without a core have been offered by Margolis (1982) and Grofman et al. (1987). A variety of models to predict expected outcomes in such games have been proposed. See Fiorina and Plott (1978) and Straffin and Grofman (1984) for relatively nontechnical reviews.

Wilson and Herzberg (1984) also ran experiments on the same five-voter two-dimensional game that we discussed, but now with one player (Player 2)

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given a veto. Though Wilson and Herzberg (1984) do not treat the game this way, it can be reexpressed as the weighted voting game (5;3,1,1,1,1). This game is collegial (i.e., there is a veto player who is in all minimal winning coalitions). Giving Player 2 a veto power shifts the location of the core away from the center of the space and to the ideal point of that player. In the Wilson and Herzberg (1984) data, 5 of 11 outcomes fall near the core point and another 3 outcomes lie in the inner polygon just below it. While the fit of the core is far from ideal, even here it appears to have considerable predictive power. We conjecture that the reason that the veto player does not obtain his ideal point as outcome is that motion toward the veto player requires continued shifts in the composition of the winning coalition.

3. For games where players are equally weighted there is reason to believe that the area of the minmax set will shrink as the number of voters increases (Demange 1982; cf. Feld, Grofman, and Miller 1985).

4. Of course, there are decision costs to searching out new alternatives; thus we may never get to the minmax set. The work of Kramer (1977) provides one plausible agenda process that will lead us toward the minmax set. Kramer (1977) proposed that at each iteration of a sequential decision process actors would seek out the alternative that defeats the status quo by the greatest margin. Kramer's discussion is couched in terms of two-candidate competition, but it can just as easily be restated as a process directed toward finding consensus (i.e., one seeking outcomes that beat the status quo by the largest margin).

5. An exception would be where the group is operating under a de jure unanimity rule. Here the group pressure would probably act so as to exert pressure on "deviants" from the group consensus to reduce the effective rule to prevent minority veto power. This appears to be true in juries (Grofman 1981).

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# PRINCIPALS, BUREAUCRATS, AND RESPONSIVENESS IN CLEAN AIR ENFORCEMENTS

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**A** principal-agent perspective has been employed in recent studies to rediscover the importance of democratic hierarchies in shaping public bureaucratic outputs. I test the robustness of the hierarchy model for explaining outputs from an agency that has often been cast in the image of bureaucratic independence, the Environmental Protection Agency. Examining the effect of the Reagan presidency on EPA outputs for clean air, Box-Tiao models are constructed to explain shifts in the vigor of air pollution enforcements between 1977 and 1985. The analysis shows that the influence of elected institutions is limited when an agency has substantial bureaucratic resources and a zeal for their use. Moreover, under these conditions, bureaucracy can even move outputs in directions completely opposite from what a model of hierarchy would predict. The implication is that for some agencies it is necessary to give greater consideration to the agent in explaining implementation outcomes through time.

**H**ow responsive are U.S. public bureaucracies to elected political institutions? This important question of democratic control has often drawn contrasting answers. Some appraisals have emphasized the imperviousness of the "fourth branch of government" to political control, while others have accentuated an electoral-administrative hierarchy as molding policy outputs in demonstrable ways.

Bureaucratic power does lend strong centrifugal tendencies to U.S. government. Rourke (1984) described the sources of agency power relative to elected institutions as consisting of four factors: expertise, constituency, vitality, and leadership. Expertise confers bureaucratic power through superior knowledge of a problem or policy. Constituency goes to the core of political relationships through the ability of bureaucracy to mobilize

political support or curb political opposition. Vitality refers to the commitment of bureaucratic personnel to job, program, or organization. Leadership is an intangible that relates to the other three variables. A good leader will bring greater expertise to an organization, effectively mobilize constituencies, and improve personnel commitment to make the organization more vital. It is the blend of these features that Rourke viewed as enabling public bureaucracies to operate in relative independence of central political authority.

Bureaucracy has been described frequently by others as a device that frustrates political direction. Elected leaders and their appointees have been seen as overly dependent on career bureaucrats. Hence, they can set the tone and image of an agency, but they can seldom change its direction (Kaufman 1981). Bureaucrats

have more knowledge, experience, inter-governmental ties, and time than political superiors. Thus, they can resist and wait out efforts at change (Heclo 1977). And bureaucrats have a near monopoly on information about agency resources and requirements; therefore, they can maneuver to maximize budgets and slack resources (Niskanen 1971, 1975). According to these views, the ability of elected political institutions to alter bureaucratic outputs is minimal.

In contrast with these images, scholars of a quantitative persuasion have recently shown that central political institutions do make a difference in the outputs of public bureaucracies. Implementation behavior has been observed as varying consistently across time with the ideology of presidential administrations (Moe 1982; see also Beck 1982), the ideology of congressional oversight committees (Weingast and Moran 1983), and the features of the courts (Moe 1985). Model terms to represent Congress and the president are now commonly included in quantitative studies purporting to explain variations in public policy outputs, both national and federal (cf. Chubb 1985; Scholtz and Wei 1986).

Empirical evidence demonstrating a response from public bureaucracies to the major democratic institutions has generated considerable excitement, raising expectations that a theory of control can be validated based on hierarchy and an economic view of organizations (Moe 1984). The framework has been labeled by academics the principal-agent model of political-bureaucratic relations. According to the principal-agent model, central political institutions (principals) bound and mold the preferences of public bureaucracies (agents) situated lower in a policy hierarchy. The ability of central political institutions to do this depends on at least three factors: the presence and effective use of political tools of control, the resources and receptivity of the

bureaucratic agents to whom the tools are applied, and the presence of crosscutting relationships that alter principal-agent responses.

Generally, the principal-agent framework casts relations between democratic principals and bureaucratic agents as ordered. Elected institutions design public bureaucracies and provide the staples for their maintenance. Although bureaucracies have interests of their own that sometimes motivate circumvention of central direction, their shirking tendencies can be overcome. Elected institutions appoint leadership, manipulate personnel, control organizational structure, provide vital resources, employ oversight mechanisms, and exercise direct authority to evoke a response from bureaucratic agents. That is, democratic institutions are able to alter the preferences of bureaucracies systematically, making the outcomes of the implementation policy process more predictable.

### The Research Focus and Vehicle

I will test the adequacy of a principal-agent framework in explaining outputs from an agency that should (if any does) conform to the model of bureaucratic autonomy: the Environmental Protection Agency. EPA provides a formidable test for the model since it traditionally has possessed all of the features which vest an agency with power. The purpose of the analysis will be to specify more precisely the conditions under which democratic hierarchy effectively shapes implementation outcomes.

Not only is the research vehicle for this evaluation different from that of previous studies of principal-agent relations, but the methods are also different. They are different in two fundamental respects. First, they are distinctive in the form of the data. Others have examined these relations with longitudinal methods using

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## Clean Air Enforcements

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data divided by annual intervals (e.g., Chubb 1985; Moe 1982; Scholtz and Wei 1986; Weingast and Moran 1983). Using time series impact assessment, nonlinear probability, or pooled regression methods they have found relationships between elected political institutions and bureaucratic outputs that suggest the importance of hierarchy. This research will also use longitudinal methods, but unlike previous work the data will be divided by monthly intervals to "zoom in" more closely on the policy process. Finely divided data are expected to yield better resolution for sensing causal sequences relevant to hypotheses.

Providing greater longitudinal resolution enables the analysis to differ from previous work in another more important respect. The purpose will be to learn more than simply *whether* public bureaucracies respond to elected political institutions. It will also be to discover *when*, *how*, and *why* public bureaucracies respond to elected political institutions. More specifically, the following questions are addressed here: Does the timing of public policy change suggest that bureaucratic agents respond to political principals acting independently or acting in tandem? When public bureaucracies do respond, is the response initiated by symbolic events or by the actual application of political tools? Which political tools are most effective in evoking bureaucratic responses? How fast and with what magnitude do policy responses occur following policy initiatives? Are the responses to political initiatives always predictable for a given set of principal-agent relations? And finally, how much of the total variation in public bureaucratic behavior can be explained by considerations of hierarchy? By viewing in micro the sequence, magnitudes, and durations of changes in the implementation policy process across an interesting period of time, a better handle should be obtained on how well a principal-agent model can explain out-

comes in an alleged democratic implementation hierarchy.

The method of analysis will be the time series quasi experiment (Campbell and Stanley 1966; Cook and Campbell 1979); that is, the response through time of a particular implementation bureaucracy to a particular set of policy initiatives will be determined. The focus will be on how EPA's implementation bureaucracy responded to the policy initiatives spawned by the election of 1980. The specific policy for analysis will be EPA's treatment of the Clean Air Acts. The 1980 election was followed by almost 2½ years of concerted efforts by a determined presidential administration to subdue EPA clean air enforcement efforts. This policy and these initiatives provide a rigorous test for both the bureaucratic autonomy and the principal-agent perspectives. A well-entrenched program was besieged by very strong efforts toward retrenchment at a time when conditions were seemingly ripe for change. The response to the policy initiatives holds the promise of providing answers to some theoretically interesting questions.

### The Clean Air Implementation Bureaucracy

What were the features of the clean air implementation bureaucracy prior to the policy initiatives following the 1980 election? National clean air regulation was initiated in 1970 with the passage of the Clean Air Act Amendments. The 1970 legislation (and amendments in 1974 and 1977) made responsibility for clean air the federal responsibility of the Environmental Protection Agency and counterparts at the state level. This analysis will focus only on EPA.

EPA was created by executive reorganization under an order that consolidated 15 different environmental programs into one agency to deal with the environment

as a "single, interrelated system." The structure of the organization broke from the previous trend of placing new regulatory functions in an independent multi-headed commission situated outside the executive branch. EPA was created as an executive agency with a single administrator appointed by the president. The major subdivisions of EPA were divided into areas such as enforcement, planning, research and development, hazardous material, and management. Within these divisions were offices that dealt with the major programs to be administered by EPA. In 1980, clean air implementation was handled specifically by the Office of Air, Noise, and Radiation with some tasks delegated to other functional areas. Most clean air regulation was performed by the 10 regional EPA offices. These offices contained technical specialists who performed the actual work of air pollution regulation, as well as administrators who oversaw federal clean air programs.

In spite of being created as an executive agency, EPA grew through the 1970s as an organization in substantial independence of executive authority. This independence was enabled by a number of factors. Environmental laws had clear, goal-oriented legal mandates that bound bureaucratic options. Environmental issues received the frequent attention of the media, keeping the matter salient to publics and politicians alike. Program constituencies, both aggregate and diffuse, prodded the agency to maintain vigorous environmental outputs. Many of the personnel of the organization were themselves zealous supporters and highly expert in the environmental technologies that developed through the 1970s. The agency was closely watched by several different congressional committees who viewed environmental programs as their special bailiwick, facilitating a more vigorous enforcement of the law. Because of these powerful supports, a tradition had even emerged whereby presidents

selected EPA administrators to assuage constituency interests rather than for effective executive administration. By 1980 EPA had grown into an organization that literally represented the environment.

These supports were manifested through the provision of copious financial support for maintaining clean air enforcement outputs. At the time of the 1980 election, the EPA operating budget was at an all-time high of \$1.35 billion (excluding sewer grants and superfund), with clean air being the largest environmental program (Office of Management and Budget 1981). In 1980, about a fourth of the total budget, \$326 million, was spent directly on clean air with additional support coming from functionally supportive offices. A clean air staff of about 1,875 persons was maintained in central and regional offices for performing inspections, air quality testing, surveillance, litigation development, adjudication, and administration. At the time of the 1980 election, these personnel performed about 6,300 clean air enforcement actions annually, and monitored state agencies that performed about 21,600 clean air enforcement actions annually (Wood 1987).

These national investments in clean air had impacts in the nation that were not viewed favorably from all quarters. In the decade of the 1970s, industry spent over \$80 billion on air pollution control technology (Council on Environmental Quality [CEQ] 1984, tbl. A-19), but some estimated the cost to the economy to be much higher (Weidenbaum 1979; Arthur Anderson & Co. 1979). By 1980, EPA and the states had gained substantial regulatory compliance from the more than 200 thousand air pollution sources in the United States. The Council on Environmental Quality (1981) estimated that only 27 hundred sources nationwide were not being adequately controlled. The Clean Air Acts and EPA had changed the environmental landscape but had generated considerable political opposition in

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### The Reagan Policy Initiatives

Ronald Reagan made it clear from the beginning that his administration would not favor vigorous environmental regulation. Generally, he campaigned on a theme that decried the evils and inefficiencies of "big government," endorsing a shift in responsibility for many federal programs to the states and localities. "Regulatory relief" for business and industry was a major part of the Reagan agenda with environmental policies especially targeted because of their alleged large impact on the economy.

More specific to his intentions for EPA, Reagan's campaign rhetoric betrayed an insensitivity to the need for strong environmental regulation.<sup>1</sup> Between the election and the inauguration, extensive plans were laid down for reforming EPA and its programs.<sup>2</sup> That the actual administration objective was to reduce the enforcement effort was later a subject of considerable ideological debate, with the administration stating that it simply wanted to bring greater efficiency to the regulatory process. But this claim was hard to maintain after congressional staffers revealed documents demonstrating that in over 70 instances Reagan's EPA administrators told OMB in plain English that they were reducing the effort to stop pollution (U.S. Congress, House 1982, 26; see also U.S. Congress, Senate, Committee on the Environment and Public Works 1981, 59). Whatever the intentions, it is clear that several events occurring early in the Reagan presidency ought to have initiated change in EPA outputs.

The Reagan inauguration itself was a symbolic event that signaled to both EPA and pollution sources that change was eminent. It was followed immediately with decisive applications of administrative tools for bringing "regulatory relief" to industry. An antienvironmentalist attorney, Ann Gorsuch (later Burford), was nominated as EPA administrator. At the

Division of Air, Noise, and Radiation, Kathleen Bennett, a former paper industry lobbyist, was given responsibility for clean air. Many other key positions within the organization were filled with administration loyalists who were antagonistic toward the agency mission. A policy analyst who worked for the Reagan transition team admitted later that "hit lists" of EPA personnel had been developed to identify those most likely to resist the changes at EPA.<sup>3</sup> Personnel rules were used to place those so identified in positions where they were least likely to impede reform (Kirschten 1983, 732-36; see also U.S. Congress, Senate, Committee on the Environment and Public Works 1981, 10). Through careful personnel manipulations, the White House created an administrative structure that should have been more receptive to relaxation of environmental regulation.

These early personnel initiatives were augmented by reorganizations that also aimed at changing environmental regulation. Decision making was centralized from the regional offices giving the Reagan appointees more control over enforcement activities. Additionally, the enforcement division was kept in a continual state of disarray, being reorganized every 11 weeks after the Gorsuch confirmation (Gotttron 1982). Previously, referrals of environmental cases to the Justice Department had averaged about 200 annually. During the first nine months of 1981 only 50 cases were referred by EPA for prosecution, with only 12 of these coming after the Gorsuch confirmation (U.S. Congress, Senate, Committee on the Environment and Public Works 1981; Wenner 1984). The message was clear that the administration did not favor and would not support strong pollution control regulation.

All of these initiatives brought some striking changes in EPA's organizational character. There were drops in personnel

morale and there was an exodus of program zealots and professionals. Even before the confirmation of administration appointees in May 1981, the total EPA staff had been reduced by 11%. Much larger reductions were planned for the future (Andrews 1984). It was estimated that the turnover rate at EPA during these times was about 2.7% per month, leaving a staff of conservers to run the organization (Gotttron 1982). One observer remarked, "What was once a robust dynamic entity has shriveled to a gray shadow of its former self, wracked by internal dissension, run by people with little expertise in environmental issues, and dogged by a paranoia that virtually brought it to a standstill" (cited in Crandall 1982, 29).

If the EPA bureaucracy was an agent responding to initiatives promulgated singularly by an executive principal, then enforcement activity should have dropped following the Reagan inauguration and definitive applications of administrative tools. The president removed all vestiges of executive constituency from EPA programs, making it clear that he wanted them subdued. He used his formidable personnel powers to install an administrative hierarchy responsive to his wishes. He reorganized agency decision making to facilitate cooperation with his plans for "regulatory relief." And all of these initiatives resulted in an organization less able to maintain the traditional posture of bureaucratic autonomy. But these were not the end of EPA's woes.

A second related set of initiatives should also have caused retrenchment in EPA environmental outputs. Nine months after the Reagan inauguration, large budget reductions became effective for environmental programs. Congress, at the president's urging, consented to reduced fiscal supports for both EPA and the states. At EPA the operating budget was reduced by 24% for fiscal year 1982 (U.S. Congress, House 1982). These cuts

had results that were specific to clean air. Between 1980 and 1983 constant dollar expenditures for air pollution regulation and monitoring declined by 42.3% (CEQ 1984, tbl. A-19). The number of personnel authorized for clean air activities declined by 31% (U.S. Congress, House. Task Force on Energy and the Environment 1982). Cuts were spread throughout the agency, decreasing EPA's ability to engage in all basic regulatory functions, as well as provide ancillary support services to EPA programs.

If the EPA bureaucracy was an agent responding to multiple political principals, Congress and the executive, then there should have been a response to the lower budget allocations beginning with fiscal year 1982. This response should have occurred for two reasons. The first and most obvious reason was that EPA no longer had the resources for conducting a vigorous environmental program. For clean air in particular, it took money to provide the personnel and equipment necessary for air pollution monitoring and abatement activities. A second equally important reason that there should have been a response was that the self-interested bureaucrats at EPA wanted to preserve the organization and its programs. Continued vigorous environmental regulation could only result in additional future budget reductions and further cripple the agency. Hence, it had become in the agent's interest to shirk responsibility for clean air regulation. A reduction in enforcement vigor was, therefore, a predictable response.

From the election and through the first 26 months of the Reagan administration, an outright assault was made on environmental programs. Congress, though certainly not antienvironmental, was more Republican than at any time since the 1950s. Additionally, it was a weak check on policy change because of the magnitude of the 1980 Reagan victory. This early reticence declined with time. EPA

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activists conducted a rear-guard action through leaks to the press and contacts with Congress. Between October 1981 and July 1982, EPA officials were called to testify before congressional committees more than 70 times to explain why they were not implementing the law (Gotttron 1982). The EPA administrator, Ann Burford (previously Gorsuch), was called on at least 15 of these occasions. Matters came to a head with the toxic waste scandal when it was alleged that she was not only failing to execute the law but may also have been encouraging pollution sources to violate the law. The turmoil surrounding these matters resulted in the resignation of Ann Burford in March 1983 and, in the same month, the nomination of William Ruckelshaus as the new administrator. The Reagan administration's outright assault on environmental programs had come to a halt.

This third event, the Burford resignation (and nearly simultaneous nomination of William Ruckelshaus as a replacement), should have resulted in a positive response in the vigor of EPA's implementation efforts. The resignation was a symbolic event that signaled administration acquiescence to congressional and public demands for renewed environmental programs. Equally important, many of the obstacles that had confronted effective regulatory administration were removed by the changes that followed. There was a house cleaning at EPA with most of the initial Reagan appointees dismissed and replaced with competent professionals. Within a short time Ruckelshaus had improved morale at EPA and even convinced the administration to seek restoration of some of EPA's budget. These reinstallations of bureaucratic supports should have resulted in a gradual return of EPA to its former prominent position in promoting clean air.

### Operationalizing Bureaucratic "Outputs" with Data

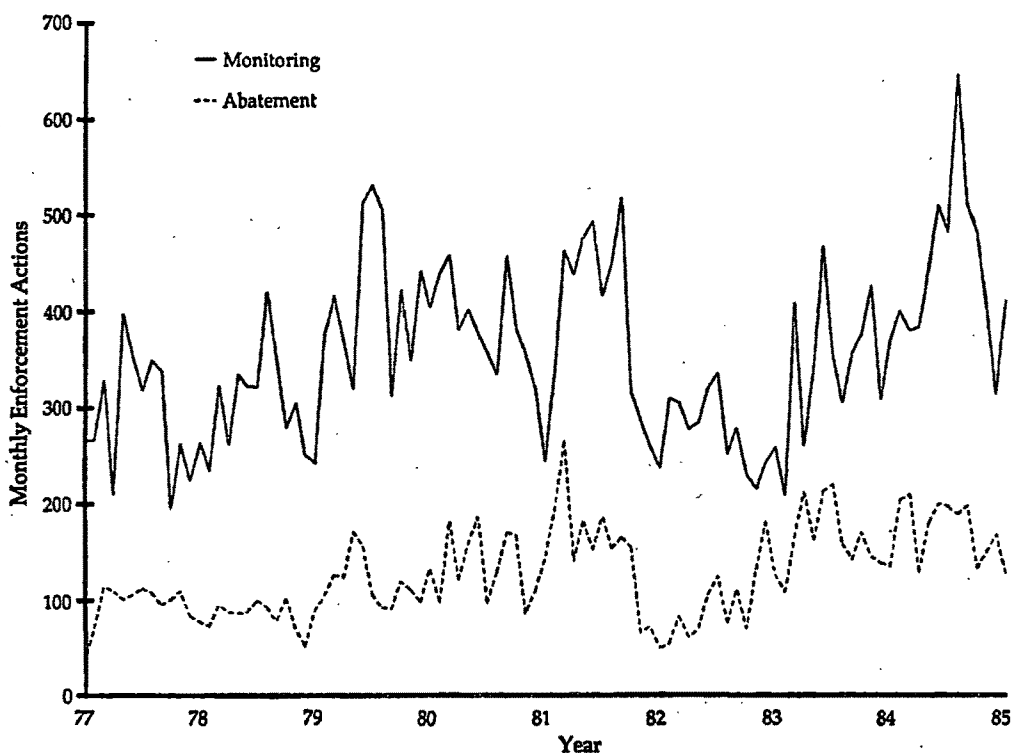
The Reagan inauguration and the fiscal year 1982 budget were powerful and discrete events that should have depressed policy outputs at EPA. The Burford resignation was a similarly powerful and discrete event that should have invigorated policy outputs at EPA. Because these stimuli were strong, it seems reasonable that some specific clean air bureaucratic outputs should have covaried with their incidence through time.

To evaluate the hypothesized covariations, the term *outputs* must be operationalized for EPA clean air implementation. Measurement of a vague concept like *outputs* can be problematic. In a general sense, bureaucratic outputs consist in all activities conducted to accomplish an organizational mission. In a restricted sense, however, *outputs* can be defined as those activities done to promote a core agency technology.<sup>4</sup> For purposes of this article, the term *outputs* is used in the more restricted sense to refer to interactions—between EPA regulators and air pollution sources—intended to secure regulatory compliance. It is these "in-the-trenches" interactions between the key regulatory actors that provide the truest measure of what policy is. Conveniently, these sorts of activities are quantifiable.

Outputs are measured by counting the number of enforcement interactions between EPA and emission sources for discrete monthly intervals with observations to be analyzed in serial form. Records of enforcement interactions have been kept since the early 1970s in EPA's Compliance Data System and were secured with a Freedom of Information Act request.<sup>5</sup> The obtained Milestone Report on enforcement actions is used as a data base for the response measurements that follow.

Two different measures of enforcement interactions are reasonable, based on dif-

Figure 1. EPA Monitoring and Abatement Activity (1977-85)



ferent elements of the enforcement process. Monitoring activities are enforcement actions taken to assure that air pollution sources remain in compliance with the law. Abatement activities are enforcement actions taken to bring noncompliant air pollution sources into compliance with the law. The monitoring activity time series observations consist of the sum of all EPA inspections (except those for case development), compliance monitor reports, compliance tests, and routine surveillance actions. The abatement activity time series observations consist of the sum of all EPA notices of violation, administrative orders, consent orders, consent decrees, case development inspections, cases referred for litigation, and meetings for formal and informal negotiations. The two time series include data from January

1977 to January 1985. This period centers the interval containing the hypothesized policy interventions yet still retains a sizable number of preintervention and post-intervention observations.<sup>6</sup> The time series monthly intervals correspond to regular monthly reports from EPA regions of specific actions taken to ensure compliance.

The intent of the two series is to capture the vigor with which EPA performed its enforcement functions through the period of interest. Monitoring activity is very important in causing air pollution sources to remain in compliance. The specter of being found out is usually sufficient to prevent most pollution sources from deviating too far from the law. Abatement activities, while involving fewer actions, attack the worst pollution prob-



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lems and also hold great illustrative significance for pollution sources. Each measure presents a different view of the enforcement policy process through time, which should reveal the responsiveness of EPA outputs to the hypothesized policy interventions.

Graphs of the two time series are presented in Figure 1. A cursory analysis of the pictures is interesting, but of little help when the complexity of raw time series data is considered. These data, like most other social science time series, contain extraneous components that make intuitive analysis difficult. Both time series may have seasonal components, trends, and stochastic variations unrelated to the hypothesized policy interventions. To rid the series of these undesired components and to facilitate effective analysis, methods of statistical control and modeling are necessary.

### A Method for Measuring the Response

A well-developed and methodologically proven way to evaluate the effects of interventions on social processes in equilibrium is time series impact assessment (Box and Tiao 1975). These methods require the identification and estimation of mathematical components that separately describe the stochastic and deterministic variations in the series. That is,

$$Y_t = f(I_t) + N_t$$

where

$Y_t$  = monitoring or abatement activity time series

$I_t$  = intervention event(s) at time  $t$

$N_t$  = noise model for stochastic processes and trends

Modeling the series offers both an excellent way to control for the independent effects of series noise, as well as an elegant

description of the series response to events of interest.

### The Noise ( $N_t$ )

The best way to discover the character of extraneous noise active in a time series is empirical modeling. We care little for the causes of uninteresting variations in the data, only for the effects of certain events on series behavior. In this regard the family of models developed by Box and Jenkins (1976) called *ARIMA models* are useful.

The underlying logic behind the Box-Jenkins method is that most of the unwanted information found in data gathered across time is caused by spurious relationships originating from within the time series (autocorrelation). That is, the value of any observation at time  $t$  is almost always a function of the value of observations at time  $t - n$ . Because previous observations contain the effects of both external disturbances and autocorrelation, the observed time series will not present a true picture of the response at a given time  $t$ . Relevant information may be masked. Equally important, spurious effects may cause unwarranted conclusions about the hypotheses of interest. It is important, therefore, that prior to the evaluation of intervention components, time series "noise components" should be independent for each observation, normally distributed around a mean, and of constant variance (i.e., a white noise process). Box-Jenkins modeling achieves this condition through empirical identification and estimation of autocorrelation effects.

### The Interventions ( $I_t$ )

The initial selection of model intervention components is the operationalization of predictions about how the series ought to respond to the interventions. Where the discovery of the series noise structure

is a more or less atheoretical endeavor, the design of intervention components should be guided by a priori notions. That is, perceptions of the timing, magnitude, and duration of the hypothesized interventions must be considered.

There are a large number of complex forms that the response to an intervention can take. Box and Tiao (1975) suggest that many intervention responses can be modeled using transfer functions to describe the dynamic transfer from the intervention to the dependent series. An intervention, from this perspective, may be conceptualized as an exogenous input time series that acts on the dependent series in binary fashion, switching to the alternate level at the time of the intervention to disturb the dependent series equilibrium. Transfer functions are terms that describe the independent effect of the input series ( $I_t$ ) on the dependent series ( $Y_t$ ). It is important to consider both the form of the input series and the substance of the function operating on that series when selecting model intervention components.

The full Box-Tiao model requires, in addition to the noise model, a specification of input series and transfer functions for each discrete intervention that affects the dependent series. The initial model specification for both enforcement activity time series should, therefore, include an intervention component to represent each of the three policy interventions discussed in the preceding section. The final models should include only effective interventions since the goal of Box-Tiao modeling is to produce a parsimonious yet theoretically complete explanation for the variance in the time series process.

### The Responses to the Reagan Policy Interventions

The methods described above were used to construct models of the time series

for both EPA monitoring and abatement activities.

#### Monitoring Activity

The modeled responses of EPA monitoring outputs to the Reagan policy interventions are represented in the following equation:

$$Y_t = \omega_{o1}I_{t1} + \omega_{o2}I_{t2} + \omega_{o3}I_{t3} + \frac{a_t}{(1 - \phi_1 B)} + \mu$$

where

- $Y_t$  = EPA monitoring activity from 1977 to 1985
- $\omega_o$  = intervention component transfer weights
- $I_{t1}$  = Reagan inauguration
- $I_{t2}$  = fiscal year 1982 budget reduction
- $I_{t3}$  = Burford resigns
- $\phi_1$  = autoregressive noise parameter
- $a_t$  = white noise disturbances
- $B$  = backshift operator such that  $BY_t = Y_{t-1}$
- $\mu$  = mean

Parameter estimates and diagnostic statistics for the equation are contained in Table 1. The equation and parameter estimates are used to generate model-predicted values for monthly EPA monitoring activity which, along with actual values, are contained in Figure 2. The graphed predicted values present an accurate view of the movements that occurred in monitoring activities after each of the policy interventions.

How did EPA monitoring activity respond to the Reagan policy interventions? All three of the hypothesized policy interventions evoked a response in EPA monitoring outputs. But not all of the responses were in the hypothesized direction. Following the Reagan inauguration there was, surprisingly, a zero-order

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**Table 1. The Impact of the Reagan Presidency on  
EPA Clean Air Act Monitoring Activity**

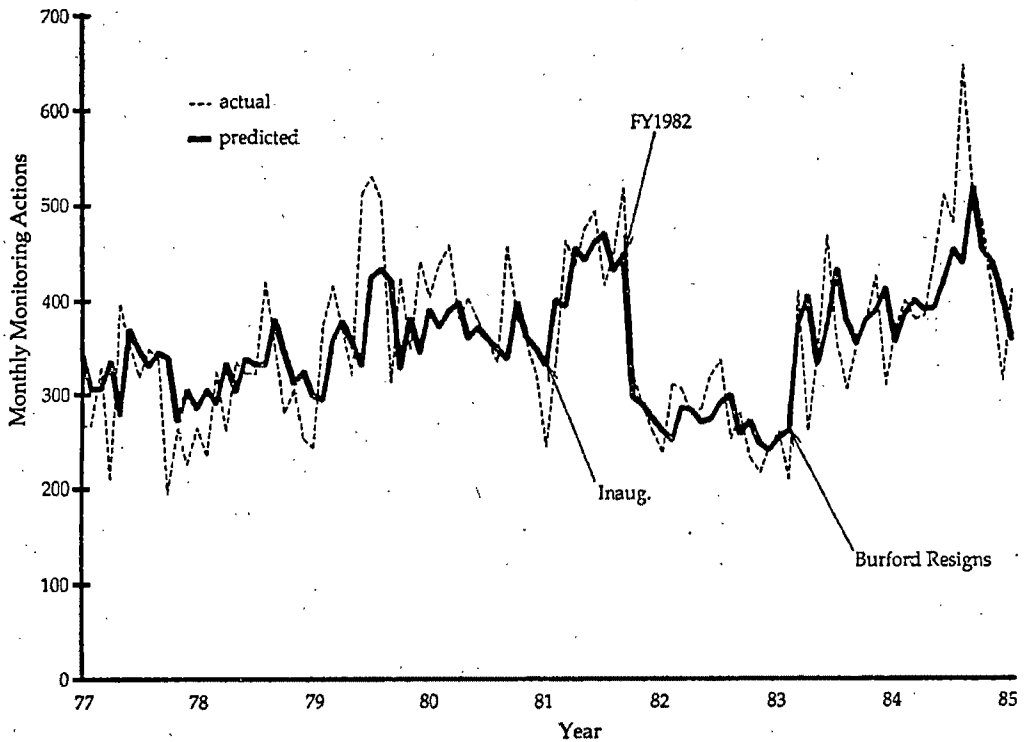
Model Component	Parameter	Monitoring Activity <sup>a</sup> Estimate	t Statistic
Reagan inauguration ( $I_{t1}$ ) <sup>b</sup>	$\omega_{o1}$	105.47	2.62
FY 1982 budget ( $I_{t2-1}$ )	$\omega_{o2}$	-185.45	-4.17
Burford resigns ( $I_{t3}$ )	$\omega_{o3}$	137.87	3.95
First-order autoregressive	$\phi_1$	.48	5.35
Mean	$\mu$	343.87	19.60

Note: Measures of fit: residual mean square (noise only), 5068.10; residual mean square (full model), 4225.39 (% change = 16.67); autocorrelation of residuals,  $Q = 18.06$  with 22 degrees of freedom (no significant lag in first 24).

<sup>a</sup>For the monitoring activities model, all intervention series ( $I_t$ ) were step inputs.

<sup>b</sup>No transfer function  $\delta_1$  parameters were significant, indicating very abrupt responses.

**Figure 2. Model-Predicted EPA Monitoring Activity (1977-85)**



*increase* in EPA monitoring activity of about 105 monitoring actions per month. This increase constituted a change of about 30% over the average number of monthly monitoring actions that had characterized the previous Democratic administration. This means that following the Reagan inauguration, the EPA bureaucracy bucked the administration and used its slack resources to substantially increase surveillance of pollution sources.

After the implementation of the fiscal year 1982 budget, EPA no longer maintained the high level of monitoring activity. The number of monthly monitoring actions dropped by about 185, a decline of 41% from the number being conducted from February through September of 1981. This reflected a decline of 23% from the average number in the previous Democratic administration. The fiscal year 1982 budget had both reduced EPA resources for air pollution monitoring and displaced bureaucratic interests toward avoiding further reductions. Congress, through the budget, had signaled tacit consent to the new directions at EPA. Accordingly, EPA responded to signals from dual political principals by maintaining monitoring activity at very low levels until the Burford resignation in March 1983.

Following the Burford resignation, there was an immediate zero-order jump in the number of monthly monitoring actions of about 138, an increase of about 50% from the depressed level that had existed. The change reflected an increase of 16% over the pre-Reagan average number of monthly monitoring actions; that is, following the resignation, monitoring levels were actually somewhat higher than they had been during the supposedly friendlier Democratic administration. The committed bureaucrats who remained at EPA had again found slack resources to restore and intensify their surveillance activities, signaling a renewed EPA inter-

est in detecting noncompliant pollution sources.

The causal mechanisms that enabled the jump in monitoring activity following the Burford resignation are not patently clear from an analysis of the data. Public support, both diffuse and aggregated, for environmental regulation had been strong throughout the period of the policy interventions. It had become obvious many months earlier in congressional hearings that legislative consent to an emasculated environmental policy was not present, but there was no increase in funding for EPA activities until October 1983. Nevertheless, a response occurred only after the Burford resignation. The logical interpretation is that EPA bureaucrats, long subdued by a hostile taskmaster but short on resources, engaged in a celebration. The zealous but risk-averse bureaucrats at EPA viewed the resignation as a cue that it was permissible for them to resume vigorous regulation of the environment without fear of retribution. They moved decisively to reestablish regulatory control of air pollution sources.

Increased monitoring activity was one mechanism EPA used to reestablish its core technology following the Burford resignation. But there were limits to what could be accomplished in enforcements with more surveillance. Moreover, because EPA resources were limited for so long, there were deficiencies in the quality of monitoring activities that could be conducted immediately. For one thing, they initially consisted of nonresource intensive activities such as readings of compliance monitors and more numerous tests. There was no significant increase in the personnel or equipment available for compliance inspections until October 1983 when modest budget increases were implemented.<sup>8</sup>

Increased monitoring activity was not the only device used by EPA regulators for reestablishing the clean air program following the Burford resignation. More-

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**Table 2. The Impact of the Reagan Presidency on EPA Clean Air Act Abatement Activity**

Model Component	Parameter	Abatement Activity <sup>a</sup> Estimate	t Statistic
Reagan inauguration ( $I_{t1}$ )	$\omega_{o1}$	84.83	3.47
	$\delta_{11}$	.93	14.00
FY 1982 budget ( $I_{t2-1}$ ) <sup>b</sup>	$\omega_{o2}$	104.80	-3.70
	$\delta_{12}$	.87	10.64
Burford resigns ( $I_{t3}$ )	$\omega_{o3}$	63.83	3.50
	$\delta_{13}$	.99	60.26
First-order autoregressive	$\phi_1$	.25	2.41
Mean	$\mu$	111.42	18.94

Note: Measures of fit: residual mean square (noise only), 1257.09; residual mean square (full model), 914.23 (% change = 27.27); autocorrelation of residuals,  $Q = 17.05$  with 22 degrees of freedom (no significant lag in first 24).

<sup>a</sup>For the abatement activities model, all intervention series inputs ( $I_t$ ) were pulsed.

<sup>b</sup>For the abatement activities model, the FY 1982 intervention series was lagged one month.

over, the model described below for EPA abatement activities suggests that changes in monitoring levels were only a secondary device.

### Abatement Activity

The modeled responses of EPA abatement outputs to the Reagan policy interventions are represented in the following equation:

$$Y_t = \frac{\omega_{o1}}{(1 - \delta_{11}B)} I_{t1} + \frac{\omega_{o2}}{(1 - \delta_{12}B)} I_{t2-1} + \frac{\omega_{o3}}{(1 - \delta_{13}B)} I_{t3} + \frac{a_t}{(1 - \phi_1 B)} + \mu$$

where

- $Y_t$  = EPA abatement activity from 1977 to 1985
- $\omega_o$  = intervention component transfer weights
- $\delta_1$  = transfer function rate of change weights
- $I_{t1}$  = Reagan inauguration
- $I_{t2}$  = fiscal year 1982 budget reduction

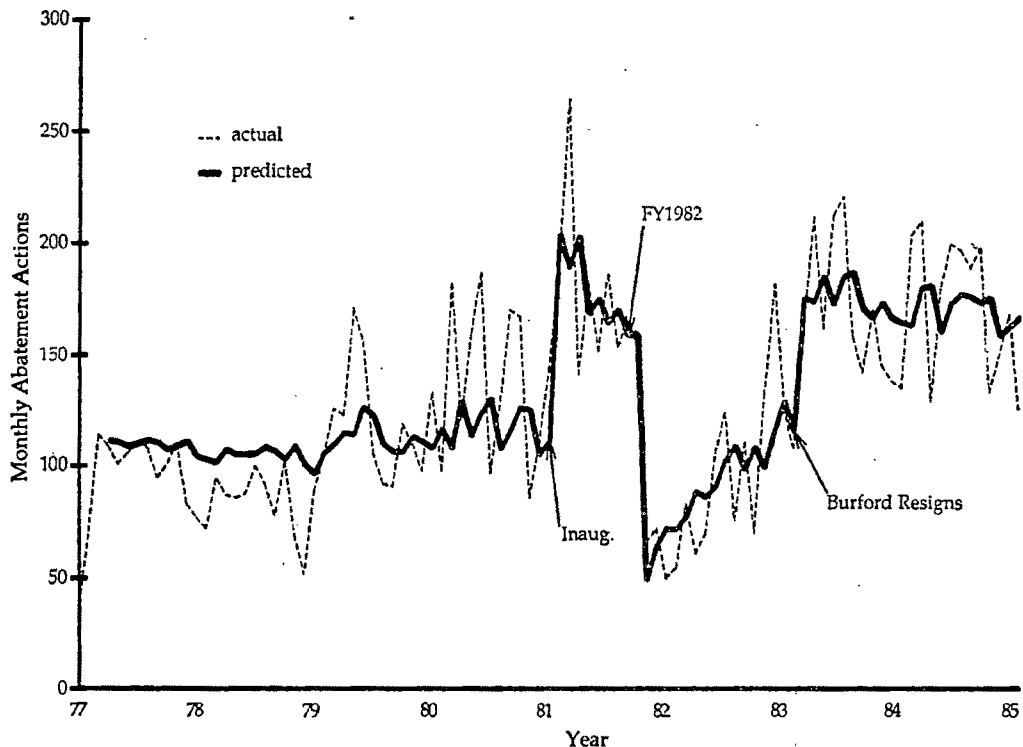
- $I_{t3}$  = Burford resigns
- $\phi_1$  = autoregressive noise parameter
- $a_t$  = white noise disturbances
- $B$  = backshift operator such that  $BY_t = Y_{t-1}$
- $\mu$  = mean

Parameter estimates and diagnostic statistics for the equation are contained in Table 2. The equation and parameter estimates are used to generate predicted values for monthly EPA abatement activity that, along with actual values, are contained in Figure 3. The graphed predicted values present an accurate view of the movements in EPA abatement activities after each of the policy interventions.

The interventions and polarities of responses that were effective for the preceding monitoring activities model were also characteristic of the abatement activities model. But the abatement activities model was distinctive in the much larger magnitude of the responses and the presence of significant first-order model intervention parameters ( $\delta_1$ ).

What do these modeling results say about EPA clean air abatement activities after the Reagan policy interventions?

Figure 3. Model-Predicted EPA Abatement Activity (1977-85)



Following the Reagan inauguration there was an abrupt and very large increase in the number of monthly abatement actions. The model shows that EPA conducted about 85 more abatement actions than usual in the first month after the inauguration, a rise of about 76% over the number that had characterized the previous Democratic administration. The full magnitude of the increase was temporary, immediately entering a gradual decline toward the preintervention level. The rate at which EPA abatement activity moved downward from the peak is reflected in the  $\delta_{11}$  parameter for the Reagan inauguration model term ( $I_{11}$ ). With first-order transfer functions, the  $\delta_1$  parameter is constrained between the interval  $\pm 1$ , with values near unity indicating a very slow rate of change. Accordingly, the

model shows that EPA abatement activities would have remained vigorous, falling only very slowly, if there had been no change stimulus at fiscal year 1982.

Following the fiscal year 1982 budget, EPA abatement activity declined by about 105 abatement actions per month, a drop of 69% from the existing numbers. The drop sent EPA abatement outputs to a low that was 56% below the average monthly levels of the previous Democratic administration. The decline in activity was delayed one month from the hypothesized time, occurring in November 1981. The delay hints at the transitory character of the bureaucratic response for abatements. The impermanence of the change is confirmed by observing the gradual resurgence of abatement activities following the large initial negative

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response. The rate at which abatement activities returned following the fiscal year 1982 budget cut is described by the  $\delta_{12}$  parameter for the intervention ( $I_{12-1}$ ). Significantly, by the time of the Burford resignation in March 1983, the number of monthly abatements had been restored to a level about equal to that which prevailed during the previous Democratic administration.

After the Burford resignation, EPA abatement activities increased abruptly by about 64 actions per month to rise 52% above the existing level. This change placed the average number of monthly abatement actions at 58% above the numbers that prevailed under the earlier Democratic administration. The response to the Burford resignation was modeled as a temporary change. But the rate of decay back toward the preintervention level was exceedingly slow, as reflected in the large value of  $\delta_{13}$  for the intervention term ( $I_{13}$ ). The implication is that for a long time following the Burford resignation, an EPA bureaucracy with limited fiscal support used abatements, a non-resource-intensive activity (i.e., little extra effort is required to begin issuing more notices of violation or administrative orders) as a wedge for bolstering EPA clean air primacy. And they used them intensively since the numbers were maintained for the remainder of the series at levels higher than for any similar period in the history of the agency.

Considering the EPA abatement activity model as a whole, the model suggests that clean air abatement actions were manipulated by EPA bureaucrats at crucial times to boost regulatory control. Specifically, after the Reagan inauguration the number of EPA abatement actions increased when EPA bureaucrats wanted to reverse the notion that non-compliant behavior was acceptable. They increased again when EPA bureaucrats wanted to do the same following the Burford resignation. Moreover, since abate-

ment outputs increased steadily after the fiscal year 1982 budget reductions, it is reasonable to conclude that bureaucratic inertia had an effect even during a period of limited resources and severe political constraints.

### Implications for a Theory of Hierarchy

The statistical analyses described above are more than simply an exercise in policy modeling. My expressed purpose is to test the adequacy of a principal-agent framework in explaining outcomes through time in an implementation policy process. The vehicle through which relations are being analyzed, EPA clean air policy, provides a rigorous test for the theory. The preintervention bureaucracy was archetypical of a case where the agent ought to have been able to operate in relative independence of executive authority. But the intervention stimuli were very strong, guaranteeing large responses. The findings reported above demonstrate clearly that considerations of hierarchy, although important, have obvious limitations for explaining outcomes in some implementation policy processes.

For clean air, a principal-agent model would predict that the Reagan administration, given the most Republican Congress since the 1950s and extraordinary political influence, should have been able to shift the preferences of the environmental bureaucracy. All of the available tools of administrative control were applied toward moving EPA away from vigorous implementation of the law. But the data analysis shows that in the end EPA's revealed preferences were completely opposite from what the model predicted. After the Burford resignation and continuing for the duration of the data, enforcements were pursued more vigorously than at any time in the agency history and in a manner inconsistent with the

ideological dispositions of elected political institutions. Moreover, throughout the observed policy process there were serious failures of explanation for considerations of hierarchy.

A model of hierarchy does not explain the increase in EPA enforcements that occurred in the months following the Reagan inauguration. In spite of the considerable effort the Reagan administration applied toward reining in enforcements at EPA, alleged bureaucratic agents seem to have acted alone, contrary to administration wishes, to increase enforcements well above the numbers that had existed during the previous Democratic administration. Nor does hierarchy explain why, after the fiscal year 1982 budget, the number of abatement activities began climbing from their depressed level to return to pre-Reagan-era levels by the time of the Burford resignation. The proclivity of EPA regulators to regulate seems to have been a factor negating the administration's ability to keep clean air enforcements to a minimum. A model of hierarchy does not explain the magnitude and decisiveness with which enforcement vigor returned following the Burford resignation in March 1983. After this intervention, abatement activities moved to a much higher level than characterized the earlier Democratic administration. Furthermore, a model of hierarchy does not explain why EPA abatement outputs remained at the higher level to 1985 with bureaucratic supports which had not grown significantly over those of the past.

These limits on the ability of hierarchy to explain implementation outcomes do not mean that principal-agent relations are irrelevant. They did make a difference in EPA's clean air outputs, since for both measures of enforcement activity there were drops following the fiscal year 1982 budget reductions. But they were only temporary, and there is no evidence in the causal sequence that the changes were an effect of executive authority operating

directly on the EPA organization. Reagan's appointments, personnel changes, reorganizations, removal of executive constituency, and other initiatives intended to bring the organization into line were seemingly ineffective in initiating "regulatory relief" for industry. Executive authority was only made effective through the power of the purse. That is, Congress mediated the relationship between the executive and the bureaucracy by giving its tacit approval to administration policies with large budget reductions.<sup>9</sup> When that tacit approval was removed by the forced resignation of the EPA administrator in March 1983, executive authority returned to a condition of impotence. EPA followed a new course, secure in the knowledge that there would be no future budget reductions comparable to those in fiscal year 1982. Outputs jumped to and remained at levels higher than would have been predicted given the ideological predilections of the Reagan administration. The image produced by these findings is that of a hierarchy effective only when there is a consensus of multiple principals (a "coarchy"), with bureaucracy very important in substantive outcomees.

One possible explanation for the failure of the model to explain clean air outputs adequately is that there were multiple principals operating on EPA behavior. This hypothesis would suggest that clean air outputs were responsive to a supportive Congress rather than to the executive. But the congressional responsiveness hypothesis likewise does a poor job of explaining important variations in clean air outputs. At the time of the Reagan inauguration, when enforcements increased sharply, there was no stimulus sent by the Congress or its committees to cause the increase. The more conservative Congress was, in fact, reticent when faced with a president who had won by an electoral landslide. Similarly, when abatement outputs began increasing from their



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depressed levels after fiscal year 1982, it hardly seems likely that a Congress that had just approved large budget reductions was responsible for the movement. Furthermore, when enforcements increased dramatically following the Burford resignation, the timing and magnitude of the changes do not suggest that a shift in congressional influence was wholly responsible. The 1982 congressional election did dilute the perception of a mandate that had prevailed since the 1980 Reagan victory. But there was no significant alteration of the oversight committees, and there was no increase in EPA appropriations until fiscal year 1984. The decisiveness of the changes in March 1983 suggests that EPA bureaucrats, while perhaps enabled by intense external interest, took advantage of the situation to increase enforcement outputs to unprecedented levels. Moreover, the proposition that a Republican Congress would spur enforcement activity to levels more vigorous than in any period in agency history is fully inconsistent with previous research on these relations. The congressional responsiveness hypothesis is, then, an unsatisfactory explanation for the variations through time in EPA clean air outputs.

Why does a principal-agent model fail to explain the longitudinal variations in EPA clean air outputs? Simply put, the model as previously applied does not give sufficient weight to bureaucratic interests in shaping policy outputs.<sup>10</sup> The ability of elected principals to shape outputs depends not only on the effective application of political tools of control, but also on the resources and receptivity of bureaucracy to behavioral modification. A failure of control can result at either the principal or the agent end of relations. Elected institutions may manipulate political controls poorly and fail to evoke a response (Waterman 1987). Likewise, a bureaucracy imbued with substantial resources and a willingness to use them may also frustrate political direction.

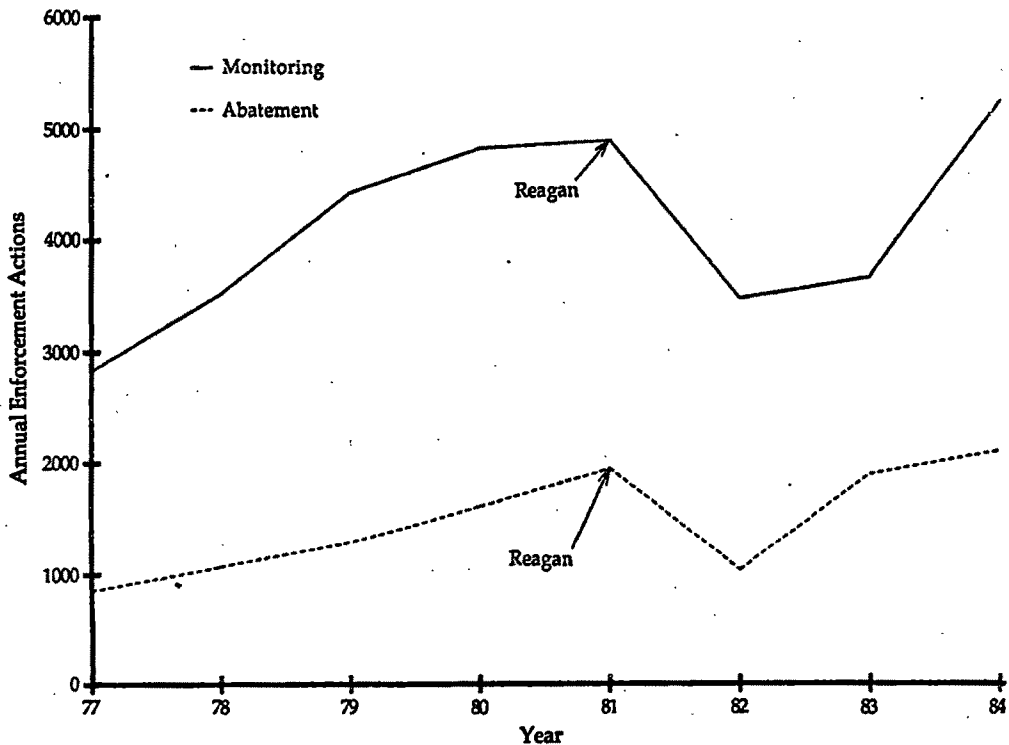
What has been illustrated here is a limit of political control, a case where manipulation not only was unsuccessful but backfired to move outputs in opposite directions.

Significantly, EPA bureaucrats acted at all times to protect their interests in promulgating the agency core technology. Sometimes this entailed restraining clean air enforcements to avoid further budget reductions. At other times it entailed moving outputs upward to preserve control over regulated air pollution sources. In the latter cases, the changes at EPA came to warrant largely and decisively the conclusion that bureaucracy was an important independent influence. The basal implication is that bureaucracies are more than receptors of policy stimuli emanating from an electoral administrative hierarchy. Rather, they are themselves responsible for much of the variation and substance of public policy through time.

### A Methodological Concern for Future Research

An important methodological consideration flows from the findings presented here. This research is different not only because of the objects taken for analysis but also because of the way in which the data are analyzed. Previous quantitative examinations of the relation between central political institutions and bureaucratic outputs (Chubb 1985; Moe 1982; Scholtz and Wei 1986; Weingast and Moran 1983) employed longitudinal designs with data divided by annual intervals. Annual data reveal only the tendencies of a policy process and mask the details that should flow from serial analysis.<sup>11</sup> This research employs a longitudinal design with finely divided intervals to capture an empirically rich dynamic underlying the process tendencies. Dividing a time series by small intervals allows

Figure 4. EPA Monitoring and Abatement Activity (1977-85)



observation in micro to more readily detect the sequence, magnitudes, and durations of policy changes. The insight that a knowledge of these factors gives is invaluable for providing answers about when, how, and why changes occur in political behavior. As an illustration of this point it is informative to compare the implications that flow from the clean air enforcement data aggregated annually (see Figure 4) with those coming from the same data divided by monthly intervals (see Figures 2 and 3). A critical difference between this research and previous research lies, then, in what is being explained: tendencies or multiple outcomes.

A caution should be issued of the danger in making inferences from longitudinal data that have poor resolution. Not only is the underlying dynamic obscured, causing the analyst potentially

to miss important movements, but the prospect is enlarged for making incorrect judgments due to problems of history (Campbell and Stanley, 1966, 39; Freeman 1983, 337). In the clean air case, for instance, if the data were aggregated annually, rather than monthly, the results of this analysis would not have differed markedly from previous research extolling the importance of electoral-bureaucratic hierarchies. The tendency of both time series was for outputs to be depressed in the early years of the Reagan presidency. Most of the time series variance was contained in the drop in enforcement after 1981.<sup>12</sup> Because of the magnitude of the changes and the low data resolution, the other variations in the series would have been lost. The conclusion would have been that elected principals were effective in altering the prefer-

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ences of bureaucratic agents at EPA. This conclusion, based on the process tendency, would have been parsimonious but not descriptive of what really occurred.

In observing a policy process longitudinally, it is important to consider carefully what can be resolved by a particular set of time series data, tendencies or multiple outcomes. Research goals should be set accordingly within limits imposed by the data. Where the data are selectable, choice of time series intervals should be made in awareness that decisions are akin to those that typically must be made between parsimony and substantive accuracy. Data with low resolution allow explanations of process tendencies, but may miss movements important to theory. Data with high resolution allow better explanation for the many variations occurring in policy through time but may require many variables to account for a complex process. Where it is not possible to select the data for analysis, research conclusions should be tempered by an awareness of the limits imposed by these considerations.

### Conclusion

This research bridges a gap between two contrasting bodies of theoretical literature on the relation between elected political institutions and public bureaucracy. The tradition that casts public bureaucracy as a pervasive influence, making policy inertial, is found partially correct since bureaucracy made a significant difference in EPA clean air outputs. On the other hand, the emerging tradition that casts the outputs of bureaucracies as predictable through consideration of democratic hierarchies is also found partially correct since elected principals were

able (if only temporarily) to shape policy outputs. The centrifugal influence of bureaucracy and the centralizing influence of hierarchies are both important, requiring consideration in a theory of political control. The extremes of the two views may be reconcilable under a principal-agent framework if more emphasis is given to the agent side of relations in future research. But it must be recognized that public bureaucracies are not simply instruments of the constitutional organs of government manipulated through behavioral incentives.

Theories of bureaucracy must make clear the dual capacity in which administrative institutions operate. Bureaucracies translate current events and ideologies into change, making government responsive to popular initiatives. They also manifest past events and ideologies, thereby insulating government from the continual progression of passing stimuli. They are reflections of both policy in the present and in the past. In this dual role, they operate as *agents of the law* who, by virtue of delegated authority, are transformed into quasi principals. Consequently, they can themselves be an important source of the variation and substance of implementation outputs through time.

This does not mean that democratic controls have failed. Public bureaucracies are legitimate principals charged with the obligations of providing responsiveness and stability. Like elected institutions of government, they have representational tasks. They represent all of those static coalitions from the past that successfully had their policy ambitions transformed into law. Bureaucracy derives its legitimate claim to principality from the laws and regulations that flowed from past coalitions. It often has inherent interests in faithfully preserving the law. Radical changes in direction seldom come unless new laws are passed. At all times, responsiveness depends on the outcomes of bi-directional interactions that may or may

not demonstrate a hierarchical aspect. Bureaucratic principals may even compete, at times, with elected principals for control of policy to move outputs in unanticipated directions. Put simply, bureaucracies are subject to limited manipulation by elected institutions, but their responsiveness is bounded by a legitimate representational task. And they do some manipulating of their own.

## Notes

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1. Consider, for example, the following materials paraphrased from Kraft (1984, 35-43): Early in the campaign, Reagan stated that air pollution in the United States had been "substantially controlled." In another speech he claimed that trees and plants were responsible for more pollution than all of U.S. industry and went on to call for reform of the Clean Air Acts to aid the steel industry. On another occasion he said that if EPA had its way, "you and I would have to live in rabbit holes." He referred to the oil slicks off the California coast as having been considered healthful at one time in U.S. history. But the most publicized of these gaffes was the Mount St. Helens statement in which Reagan claimed that the volcano emitted more sulfur dioxide than had come from all automobiles in the last 10 years.

2. During the transition period, plans were formulated by the conservative Heritage Foundation that described EPA as a hotbed of economic inefficiency and advocated "the most comprehensive and far-reaching programs of regulatory reform ever undertaken by any administration in the nation's history" (Cordia and Hinrich 1981; for a good summary see Kraft 1984). Not all observers agree that these recommendations were the ones that drove the early administration of EPA (but see William E. Schmidt, "Denver Lawyer's Role in EPA Decisions Is Focus of Inquiry by Congress," *New York Times*, 26 February 1983; see also B. Drummond Ayres, Jr., "Conservatives Bid Reagan Cut More," *New York Times*, 22 January 1983; and Russell W. Peterson,

"Laissez Faire Landscape," *New York Times Magazine*, 31 October 1982). Nevertheless, there is striking similarity between what was contained in the foundation's suggestions and what actually occurred at EPA.

3. Stuart Taylor, Jr., "Ex-EPA Aide Says He Drew Up 'Pro and Con' Lists on Personnel," *New York Times*, 17 March 1983.

4. The term *core technology* is borrowed from Thompson (1967), who develops a theory of organizations based on the concept.

5. The Freedom of Information Act Petition was submitted to the EPA Office of Air, Noise, and Radiation, Stationary Source Division. The data were retrieved in the form of a printed computer listing of enforcements from all 54 states and territories within the 10 EPA regions.

6. There were 102 observations, with the inauguration falling on the 49th and the Burford resignation falling on the 75th.

7. The use of backshift notation is conventional shorthand in statistical time series analysis. For a short and very simple discussion of backshift algebra see McCleary and Hay 1980, 45-48.

8. In a separate analysis of compliance inspections only, there was found a significant first-order step intervention in October 1983 when the first Ruckelshaus budget was implemented. About \$165.5 million was restored to EPA's operating budget at this time, of which an unknown amount found its way into promoting clean air.

9. Note that since political influence was manifested through the budget, a shared political tool, there are some real difficulties in defining just which principal was responsible for altering bureaucratic behavior. Who made the difference: the executive, Congress, or both? The problem involves issues of theory that cannot be resolved by statistical analysis (or any other form of analysis). It also involves difficulties for statistical analysis in parameter interpretation since neither executive nor congressional influence is independent. One solution to this problem could be to use a single-model term from the multiplication of the two variables. This specification captures the substance of the relation described below as *coarchy*.

10. Bendor and Moe (1985, 1986) do explicitly include bureaucracy in their dynamic model of these interactions. At this point, however, no one has effectively operationalized agent interests in empirical modeling of these relations.

11. The discussion in this section is applicable whether the data are analyzed with regular time series methods, pooled methods, or nonlinear probability models with variables divided by annual intervals.

12. It is worth a special note to caution about those longitudinal methods (e.g., pooled regression) that do not force the analyst to look at the data. A model term may be theoretically satisfying, explain

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substantial amounts of variance, be statistically significant, and still miss what actually occurred.

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## CONTROVERSY

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## DO U.S. SENATORS MODERATE STRATEGICALLY?

*Do U.S. senators adjust their policy positions or voting behavior—engage in “strategic moderation”—in their quest for reelection? In the June 1986 issue of this Review, Gerald Wright and Michael Berkman sought to demonstrate that Senate incumbents moderate their ideological positions as elections near. This endeavor was part of their larger effort to show the importance of policy issues in the selection of members of Congress. Robert Bernstein takes the view that the claims about strategic moderation rest on methodological flaws. But Wright and Berkman argue that most investigators agree on the general direction of senatorial candidate behavior. The controversy turns on conception and interpretation of analytical results.*

To support their argument “that policy issues play an important role in the selection of members of Congress,” Gerald Wright and Michael Berkman attempt to show that Senators engage in “strategic moderation,” that is, that Senators shift to more moderate positions in the last two to three years of their terms in order to increase their prospects for reelection. In so doing, Wright and Berkman enter an ongoing debate on whether there is empirical evidence of strategic moderation (Elling 1982; Thomas 1985). I think measurement flaws in their research leave that debate unresolved.

### Do Senators Engage in Strategic Moderation?

To demonstrate that senators “moderate later in their terms to increase their election appeal” (p. 575), Wright and Berkman compare the mean policy conservatism of senators up for reelection in 1982 with the mean policy conservatism of similarly situated senators not up for reelection. That way of measuring “strategic moderation” has two serious flaws:

(1) the mean conservatism score for a group is not a measure of how moderate group members are; and (2) a static 1982 comparison cannot distinguish between senators who became more moderate as the election approached and those who were moderate throughout their careers.

Regarding the first flaw, a mean rating of neutral on a conservative-liberal index for a group of senators can reflect moderation by every senator in the group, but it can also reflect counterbalancing extremists. If one candidate has a score of +5 (most conservative on the index used by Wright and Berkman), and another a score of -5 (most liberal), the candidates' party gets a score of 0 (perfect moderation); but that party score does not reflect any “strategic moderation” by party candidates.

If one is using a conservatism scale, a better measure of the moderation of each senator is the absolute value of the difference between each senator's score and the scale score for neutrality (Elling 1982). The mean of those absolute values can then be used as a summary measure for how moderate a group of senators is.<sup>1</sup>

Regarding the second flaw, a group of senators running for reelection can be



more moderate than a similarly situated group not running for reelection without that relative moderation being "strategic"; that difference in moderation might always have been characteristic of the groups. A better measure of strategic moderation is to compare moderation scores from different years to see the extent to which those running and those not running shift positions as the election approaches. Since Wright and Berkman argue that strategic moderation occurs in the last two to three years before an election, a comparison of third-year and sixth-year moderation scores will disclose any strategic moderation that may occur.<sup>2</sup>

The over-time comparison should really highlight strategic moderation. Not only will those running for reelection be *more* moderate than they were three years earlier because of strategic moderation, but those not running will tend to be *less* moderate than three years earlier, when about half of them had faced reelection.

### Measuring Strategic Moderation

For reasons detailed by Elling (1982, 79), moderation in each year is probably best measured by deviation from the midpoint of one of the standard measures of senators' ideology (Americans for Democratic Action or Americans for Constitutional Action [ACA] ratings). The ACA ratings, because they do not penalize for absences, are preferable and are used in the analysis below. To control for changes in the construction of the ACA scales, it is necessary to observe *relative shifts* in moderation scores by both those running for reelection and those not running for reelection.

While there may be some debate over which is the best control group for comparison with those running for reelection, the best case can be made for those senators not running for reelection *but from the same states* as those running for reelection. That comparison eliminates

problems stemming from comparison with a group of senators whose constituencies differ geographically, ethnically, occupationally, and so on from the constituencies of those running. Limiting the control group to senators serving during the entire period also eliminates the possibility that changes in moderation scores might be caused by turnover. Table 1 compares changes in moderation scores for those running for reelection with same-state senators not running for reelection for those states in which the two senators were the same throughout the time period.<sup>3</sup>

Table 1 shows very limited evidence of strategic moderation. As their terms neared their end, Democrats standing for reelection in 1982 did moderate their positions substantially relative to their control group. However, Republicans standing for reelection in both 1982 and 1984 showed only slightly more moderation (or less extremism) than did their control groups, and Democrats running in 1984 showed less moderation (or more extremism) than did those not running.

Given that the relative changes in moderation scores should overstate any evidence of strategic moderation and given the sometimes weak or contradictory findings, there is little support for the hypothesis that senators moderate their positions as reelection time approaches.

### Why There Is So Little Evidence of Strategic Moderation

It should not be surprising that there is so little evidence of strategic moderation. The argument for strategic moderation is based on an oversimplification of theories derived from spatial analysis. Even if one assumes that senators *shift* ideologies in order to enhance reelection prospects, it does not follow that senators will *moderate* their positions unless the senators assume that the median voters in their

## Do Senators Moderate Strategically

**Table 1. Mean Moderation Scores for Senators Running for Reelection and Same-State Senators Not Running**

Time Period	Democrats		Republicans	
	Running	Not Running	Running	Not Running
<b>For 1982</b>				
Moderation (1982)	14.2	27.1	21.5	27.2
Moderation (1979)	27.3	32.7	27.1	30.5
Change (1979-82)	-13.1	-5.6	-5.6	-3.3
Number of cases	13	11	8	10
<b>For 1984</b>				
Moderation (1984)	21.9	25.9	32.3	36.3
Moderation (1981)	15.6	20.7	20.6	21.7
Change (1981-84)	+6.3	+5.2	+11.7	+14.6
Number of cases	11	11	15	15

districts are (1) more moderate than they are and (2) likely to be closer to their future opponents' ideologies than to their own. Such would never be the case for conservative Democrats expecting to face even more conservative Republicans or for liberal Republicans expecting to face even more liberal Democrats. If they are already positioned between the median voter and the potential opposition, movement towards the center would be expected to cost votes. If the median voter is positioned between them and the potential opposition, or beyond the potential opposition, only a shift towards a more extreme position would be expected to gain votes. Thomas (1985) empirically verified that argument by finding that conservative Democrats and liberal Republicans do not moderate as elections approach.<sup>4</sup>

Strategic moderation would not even be expected to enhance the election prospects of all liberal Democrats and conservative Republicans. However, restricting comparisons to just liberal Democrats and conservative Republicans does increase the probability of finding evidence of some strategic moderation. Table 2 shows the results of those restricted comparisons

for senators running in 1982 and 1984 and their appropriate control groups.

As theory would suggest, the evidence of strategic moderation is somewhat stronger when the sample is restricted to those most likely to expect to profit from such moderation. Those running shifted to more moderate positions relative to their respective control groups for both parties, both years. The *N*s are small, as is the relative shift by Democrats running in 1984. However, the results do suggest some strategic shifting to more moderate positions by liberal Democrats and conservative Republicans.

### Are Moderates More Likely To Be Reelected?

The hypothesis that senators will moderate their positions is in part based on the hypothesis that moderates are more likely to get reelected. While Wright and Berkman do not test that hypothesis directly, they present an empirically derived equation that indicates just the opposite: that senators running for reelection would gain the most votes by taking extreme positions.

**Table 2. Mean Moderation Scores for Liberal Democratic and Conservative Republican Senators Running for Reelection and Same-State Senators Not Running**

Time Period	Democrats		Republicans	
	Running	Not Running	Running	Not Running
For 1982				
Moderation (1982)	13.0	26.5	23.0	27.0
Moderation (1979)	28.3	35.0	33.0	30.3
Change (1979-82)	-15.3	-8.5	-10.0	-3.3
Number of cases	13	11	8	10
For 1984				
Moderation (1984)	20.6	29.0	36.9	42.4
Moderation (1981)	19.4	26.8	26.4	24.6
Change (1981-84)	+8	+2.2	+10.5	+17.8
Number of cases	11	11	15	15

Wright and Berkman present an equation (number 2) showing that the likelihood of a person casting a Republican vote ( $V$ ) is a function of the *product* of the person's ideology ( $I$ ) and the candidate's ideology ( $C$ ).<sup>5</sup> Once other variables are controlled,  $V = .054 IC$ , for the Republican candidate's ideology, and  $V = -.038 IC$ , for the Democrat's ideology.<sup>6</sup>

In their equation,  $I$  takes the value of  $-1$  for liberals,  $0$  for moderates, and  $1$  for conservatives. Thus  $.054 IC$  (or  $-.038 IC$ ) is always  $0$  for moderates. For moderates, the equation indicates that the probability of a Republican vote (or a Democratic vote) is completely unaffected by the extremism of the candidate.

For liberals and conservatives, however, the candidate's ideology does affect the probability of a Republican vote. Candidate ideology is coded from  $+5$  (most conservative) to  $-5$  (most liberal). If a Republican candidate adopts an extreme conservative position, the candidate increases the probability of a Republican vote by  $(.054)(1)(5)$  for each conservative voter and decreases the probability of a Republican vote by the same amount for each liberal voter.<sup>7</sup>

If there are more conservatives than liberals in the district, the Republican candidate maximizes the number of Republican votes (maximizes the sum of  $.054 IC$  over all voters) by taking the most extreme conservative position. If liberals outnumber conservatives, the Republican candidate maximizes his or her votes by taking the most extreme liberal position. Similarly, the Democratic candidate maximizes the number of Democratic votes by taking either the most extreme conservative or liberal position (thus maximizing  $-V$ ).

If the Wright and Berkman equation is correct, candidates receive no electoral advantage from moderating their positions. Instead, they gain by taking extreme positions. This unexpected finding may be an artifact of the measures and analysis employed; there may be no electoral advantage in "strategic extremism." However, it may correctly reflect a substantially greater salience of ideology to conservative and liberal voters than to moderate voters. In either case, Wright and Berkman's individual-level analysis shows no evidence of electoral advantage to "strategic moderation."

## Do Senators Moderate Strategically

### Strategic Behavior by Senators

Neither logic nor evidence supports the theory of widespread strategic moderation. The logic of spatial analysis suggests that moderation would be "strategic" only for some liberal Democrats and conservative Republicans. Analysis of senatorial behavior does find limited evidence of strategic moderation among just those groups of senators. However, evidence uncovered by Wright and Berkman at the individual level casts doubt upon whether moderation is in the interest of even those senators. Strategic "shifting" may prove rewarding, but that shifting need not be to a more moderate position.

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Robert Bernstein raises some interesting questions in his thoughtful comments on our study of the dynamics of candidate behavior and voter decision making in U.S. Senate elections. His analysis focuses on two points. The first concerns our conclusion that senatorial incumbents moderate as the election nears. The second concerns whether it is electorally rational for them to do so. On the first matter, Bernstein simply has interpreted the term *moderation* differently than we intended. However, this is of little theoretical consequence since we agree with him on the actual patterns of candidate behavior. On the second matter we argue that Bernstein has misinterpreted our probit equations. A proper test of the hypotheses implied by his extremism thesis demonstrates the soundness of our initial conclusions: moderation, not extremism, provides more votes for senatorial candidates.

### Strategic Behavior: Candidates

Bernstein sees an "ongoing debate" over the nature of changes in incumbents'

ideological positions over the election cycle. We do not see such a debate. We note some inconsistency in the way different authors have hypothesized about strategic shifts, and some differences in the use of terms. However, the work by Elling (1982), Thomas (1985), ourselves, and Bernstein all show the same thing: there is a tendency for incumbent senators to move away from their party's ideological pole and in the direction of the opposition as the election nears. Our purpose in the initial portion of our article was to provide a summary of overall candidate strategic behavior using a new data source, the CBS News–*New York Times* 1982 Congressional Poll. We saw this as building on the studies that have used roll-call behavior to assess strategic positioning. Our chief contribution here was to determine that challengers seem unable to undertake the same sort of positioning as incumbents. This is the important fact in our context of our argument.

Some confusion came from our use of the term *moderation*. Bernstein follows Elling's (1982) use of the term to mean movement toward the midpoint of the senatorial ideological spectrum. We used the term to mean the Downsian behavior of Democrats moving to the Right or Republicans moving to the Left to get closer to their states' median voter. For conservative Democrats and liberal Republicans such shifts are away from the middle of the senatorial spectrum. Thus, based on his conceptualization of the term, Bernstein argues we do not present evidence for strategic moderation. We feel our usage was clear in context, but we would probably use the term *strategic shifts* if we were writing the article today.<sup>8</sup> The important point is that once this simple terminological difference is clarified, our analysis does not suffer if this behavior is called *strategic shifts* rather than *strategic moderation*. There is no dispute on the empirical findings, and these are consistent with our theory of candidate issue strategies. Further, since

we agree on the actual patterns of candidate behavior, Bernstein's methodological suggestions about measuring how candidates shift do not bear on our conclusions.

### Strategic Behavior and Election Outcomes

Bernstein's second point is potentially much more important. He argues that our data show that movement *toward* the ideological extreme is a profitable electoral strategy: "Candidates receive no electoral advantage from moderating their positions. Instead, they gain by taking extreme positions." We argue that movement *away* from one's party extreme and toward the opposition and the state median voter is the vote-maximizing strategy. Four points bear on this rather substantial disagreement.

First, Bernstein states that we do not test the hypothesis that moderation gains votes. On the contrary, we take up more than a full page (p. 582) with this question. In the "Candidate and Economic Effects on the Outcome" section we show how our individual-level findings can be used to look at the aggregate consequences of candidate strategy. We provide data to support our conclusion that moderation, *here* in the sense of middle-of-the-road issue positions, does help candidates in the general election.

Second, most of his analysis rests on an inappropriate equation. Equation 2 has vote on the left side and only party, ideology, incumbency dummies, the candidate positions variables, and terms for the interaction of voter and candidate ideologies on the right. Equation 4, which includes the central variable of presidential evaluations, is a better specification of the process of candidate choice than Equation 2 on both theoretical grounds and in terms of a superior fit.<sup>9</sup> Hence, we

include presidential evaluations in the analysis below.

The third, and probably the most serious, of Bernstein's errors occurs when he calculates the slope coefficients for candidate strategy. Using the coefficients from Equation 2, he omits a key component, erroneously believing that because  $b_4$  and  $b_5$  do not reach statistical significance, they should be treated as 0. It is important to realize that no significance test changes the best estimate of candidate effects based on Equation 2: the slope for Republican candidate ideology is  $(b_4 + b_7I)C_R$ , and the slope for Democratic candidate ideology is  $(b_5 + b_8I)C_D$ . Dropping  $b_4$  and  $b_5$  has the effect of saying that candidates' strategies are of no consequence among moderates, and this proposition provides the basis for Bernstein's extremism thesis.

Our Equation 2 does not provide an appropriate test for the hypothesis that candidate strategy has no effect among ideological moderates. However, such a test is straightforward: using the same data analyzed in our article, we select only moderates and run a probit analysis of vote on the candidates' ideological positions, (with party identification, incumbency, and Reagan approval as controls). Our expectation is that as Republicans get more conservative, they move away from the moderate voter and decrease the probability of a GOP ballot; and as Democrats get more conservative, they move toward the moderate voter, hence they also decrease the probability of a GOP ballot. Thus, the coefficients for both candidate variables should be negative. This is what we find. The probit coefficients are  $-.020$  and  $-.018$  for the Republican and Democratic candidates respectively, with  $t$ -values of  $-3.8$  and  $-3.5$ . The foundation for Bernstein's argument—that among moderates "the probability of a Republican vote (or a Democratic vote) is completely unaffected by the extremism of the candidate"—is simply wrong.

Finally, there is a problem with Bern-

## Do Senators Moderate Strategically

**Table 3. Simulated Election Outcomes in Twenty States  
Based on Alternative Senate-Candidate Strategies**

State	Simulated Candidate Strategies		Difference	Number of Cases
	Liberal Democrats v. Moderate Republicans	Moderate Democrats v. Conservative Republicans		
California	52.9	47.5	5.4	2,883
Connecticut	50.8	45.1	5.7	1,945
Maine	39.2	33.8	5.4	1,515
Michigan	36.6	31.4	5.2	823
Minnesota	49.2	43.8	5.4	1,181
Mississippi	38.0	32.6	5.4	953
Missouri	49.4	43.8	5.6	2,034
Montana	39.5	34.2	5.3	1,391
Nebraska	48.5	43.0	5.5	1,177
Nevada	43.9	38.5	5.4	1,124
New Jersey	49.6	43.9	5.7	1,997
New Mexico	50.4	44.9	5.5	1,128
New York	35.2	30.1	5.1	2,218
Ohio	37.1	32.0	5.1	931
Rhode Island	44.3	38.4	5.9	1,254
Tennessee	33.4	28.5	4.9	829
Utah	63.1	57.9	5.2	1,533
Virginia	51.9	46.4	5.5	935
West Virginia	32.1	27.4	4.7	633
Wyoming	58.5	53.0	5.5	870

Note: Percent voting Republican is estimated based on col. 1 of Table 6 (Wright and Berkman 1986) with liberal, moderate, and conservative candidate issue positions simulated at values -5, 0, and 5, respectively.

stein's interpretation of probit coefficients; and this contributes, we believe, to his conclusion. His analysis for the extremism hypothesis rests, in part, on the pattern of liberal and conservative voters being more responsive than moderates to differences in candidate issue positions.<sup>10</sup> However, differences in the slopes within probit do not necessarily translate into like differences in the probabilities of a Republican or Democratic vote. To illustrate, let us consider only the moderates and conservatives in a district; and let us assume for sake of convenience that they are equal in number. Finally, assume we estimate that a given shift by the Republican candidate to the Right yields a probit coefficient of +.2 for conservative voters and -.1 for moderates.

Bernstein would infer from this that more votes would be gained from conservatives than would be lost among

moderates by the rightward shift. And on the basis of such logic he argues against the moderation thesis. However, the higher positive coefficient for conservatives does not necessarily mean a rightward move would have the desired electoral consequences. On the one hand, conservatives as a group are already disposed to vote for the GOP candidates, thus they may have a prior expected probit score ( $Z^*$ ) of, for instance, 1.0 (which is the equivalent of a .64 probability of voting Republican). On the other hand, the average moderate may be undecided with a  $Z^* = .0$  (probability of .5). Then the rightward shift by the candidate would change the  $Z^*$ s to 1.2 for conservatives and to -.1 for moderates. In probabilities, however, because of the nonlinear relationship between the  $Z^*$ s and their accompanying probabilities, this is an increase in the probability of voting

Republican of only .023 for conservatives while the same probability changes  $-.04$  for moderates. The rightward shift would have decreased the net proportion of all these voters expected to vote Republican, even though in terms of probit coefficients conservatives appear more responsive to candidate issue positions.

The point here is that to assess the likely effects of candidate strategy adequately we need not only to consider the coefficients from the appropriate specification but also to translate these into mean probabilities among the constituencies of interest. We did so in the article for a typical undecided voter. Here we consider the consequences of alternative candidate strategies for 20 state electorates.

Our test again uses the CBS News-*New York Times* 1982 exit-poll data sets. We simulate election outcomes by substituting different candidate issue positions in the individual-voter prediction equation. We use the predicted probit scores to obtain probabilities of a GOP vote at the individual level, and then calculate state means of these probabilities to obtain the expected proportion voting Republican in each state under each strategy set.

Two elections are simulated for each state: the first has a liberal Democrat running against a moderate (middle-of-the-road) Republican, while the second has a moderate Democrat opposed by a conservative Republican. The results are shown in Table 3. Notice that for each and every state—including such conservative states as Utah and Mississippi—the estimated GOP vote (mean estimated probability) is higher in the simulation where the Republican is the moderate. Likewise, of course, the Democrat does better in each case running as a moderate opposed by a conservative. The extremism hypothesis finds no support in these data.

In summary then, in spite of a terminological confusion, our theory and analysis hold up quite well. Candidates do behave in a manner consistent with our premise

that they believe issues count in their elections. Further, we find no support for Bernstein's extremism thesis: our data show that moderates do respond to candidate strategies, and because of this, his deductions about candidates having to pay attention only to ideologues is incorrect. Further, when we look at voters in the 1982 elections, candidates running as centrists did better in our simulation than ideological extremists. This provides still further evidence for the electoral advantage of "strategic moderation" in contests for the U.S. Senate.

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## Notes

Robert Bernstein thanks Harvey Tucker, Norman Luttbeg, and Cynthia Bernstein for their comments on earlier drafts of this critique.

1. The Wright and Berkman measure and the absolute-value method would give the same answer for the senators in a given party *only* if all Republicans were conservative and all Democrats liberal. As it is, the Wright-Berkman score reflects the relative balance of liberals and conservatives in a party rather than the moderation of senators from that party. For example, the strikingly low conservatism score of 1.0 that they get for the 11 Republicans running for reelection primarily reflects the fact that 6 of those 11 Republicans were liberals (Weicker, Durenberger, Danforth, Heinz, Chafee, and Stafford). (Note: the *N*s for the second column of Table 2 in Wright and Berkman are reversed.)

2. Thomas (1985) also compares third year with sixth year. Earlier than the third year would mean too much turnover in the control groups; later might understate strategic moderation if it does occur.

3. Wright and Berkman use senators from all states in their control group. That control group does not moderate as much as the same-state senators for 1982 ( $-5.1$  for Democrats and  $-.5$  for Republicans), thus strengthening the case for strategic moderation. However, that control group moderates more than the same-state control group for 1984 ( $5.0$  for Democrats and  $13.7$  for Republicans), thus weakening the case for strategic moderation.

4. Thomas found, for a small sample, that conservative Democrats and liberal Republicans actually tended to move away from the center. The four conservative Democrats in this sample who were running for reelection did move slightly away from the center over the time period (but there were no

## Do Senators Moderate Strategically

conservative Democrats in the control group with which to compare them). The six liberal Republicans running for reelection moved slightly towards the center, while the one liberal Republican in the control group moved slightly away from center.

5.  $C$  was not significant by itself;  $IC$  was.

6. .054 and  $-.038$  are maximum likelihood estimates and not regression coefficients, but that does not alter the analysis.

7. .054  $CI$  cannot be read directly as a probability, but is best read as a  $z$ -transformation of the probability. Still, maximizing .054  $CI$  will maximize the probability of voting Republican.

8. We discuss the problem of strategic moderation in clear Downsian terms: "Candidates . . . would like (if only for purposes of getting votes) to appear moderate to the general electorate. . . . Candidates cannot easily shift positions toward the median state voter in the short-run without risking both alienation of those core supporters and charges from the opposition of being two-faced and inconsistent" (p. 569). Our operationalization should have left no ambiguity: "If there is electoral pressure toward moderation in the general election, we expect Democrats running for reelection to be more moderate (i.e., conservative) than nonelection year Democratic incumbents, . . . while incumbent Republicans running will be more moderate (i.e., liberal) than nonelection year Republican senators" (p. 574).

9. Equation 2 was estimated to demonstrate the effects of strategic behavior on ideological voting. Some of the workings of voter ideology would be obscured by including presidential evaluations.

10. Bernstein's comment here does raise an interesting problem, the differential salience of candidate strategies to different voters. Ideological identifiers are shown in our analysis to be more responsive than moderates. However, this only scratches the surface of possible variability among voters. We suspect that similar differences may occur by levels of partisanship, incumbency, and voter sophistication or interest. These differences will be explored in a future paper.

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## Forthcoming in June

The following articles, controversies, and research notes have been tentatively scheduled for publication in the June 1988 issue:

Alan I. Abramowitz. Explaining Senate Election Outcomes.

James Alt, Randall Calvert, and Brian D. Humes. Reputation and Hegemonic Stability: A Game Theoretic Analysis.

David Austen-Smith and Jeffrey Banks. Elections, Coalitions, and Legislative Outcomes.

Ivor Crewe and Donald D. Searing. Margaret Thatcher, Anthony Downs, and the Theory of Ideological Change.

Darell Dobbs and Robert Grafstein. Political Culture and Political Preferences. A Controversy.

James L. Gibson. Political Intolerance and Political Repression During the McCarthy Red Scare.

Bernard Grofman and Scott L. Feld. Rousseau's General Will: A Condorcetian Perspective.

Douglas A. Hibbs, Jr. and Christopher Dennis. The Politics and Economics of Income Distribution Outcomes in the United States.

Paul K. Huth. Extended Deterrence and the Outbreak of War.

David Laitin and Aaron Wildavsky. Three Theses on Culture. A Controversy.

Manus Midlarsky. Rulers and the Ruled: Patterned Inequality and the Onset of Mass Political Violence.

George A. Quattrone and Amos Tversky. Contrasting Rational and Psychological Analyses of Political Culture.

H. Mark Roelofs. Liberation Theology: The Recovery of Biblical Radicalism.

Kenneth D. Wald, Owen E. Dennis, and Samuel S. Hill. Churches as Political Communities.



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## REVIEW ESSAYS

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**The Revolution within the Revolution: Workers' Control in Rural Portugal.** By Nancy Gina Bermeo (Princeton: Princeton University Press, 1986. xix, 263p. \$28.50).

**Politics in Contemporary Portugal: Parties and the Consolidation of Democracy.** By Thomas C. Bruneau and Alex MacLeod (Boulder, Lynne Rienner, 1986. xix, 236p. \$30.00).

**Portugal in the 1980s: Dilemmas of Democratic Consolidation.** Edited by Kenneth Maxwell (New York: Greenwood, 1986. xiv, 254p. \$39.95).

Although political scientists who study Western Europe have traditionally concentrated their efforts on Britain, France, Germany, and Italy, there has been in recent years a certain shift in focus to the smaller and newer democracies of the region. Despite this change, Portugal's new democracy has yet to receive much scholarly attention. This is understandable. Portugal's location on the extreme south-western edge of the continent removes it from the mainstream of European events and its small size reduces its international importance. Moreover, it is overshadowed by Spain, which, because of its size and more widely known language, culture, and history has, comparatively speaking, drawn much more scholarly attention. Although understandable, this neglect of Portugal is unfortunate. Having recently made the transition from authoritarian rule to democracy, Portugal offers fertile ground for the testing of new hypotheses and the invalidating or supporting of existing ones in a large number of important research areas, such as authoritarian-regime breakdown, military intervention, democratic-regime instauration, regime consolidation and institutionalization, rural rebellion, party-system formation, constitution making, and the like.

It should be noted that although Portugal has been ignored by Europeanists, it has received some limited attention by Latin Americanists. Writing in the early 1970s, a certain group of these scholars sought to explain

Portuguese political life from the perspective of the Latin American political experience. The general thrust of their work was to show that Portugal, like countries in Latin America, fell outside the range of the general frameworks of political development because its political process and institutions were fundamentally different from those in the rest of the less-developed world. At the same time, it was argued that Portugal could not be comprehended from the perspective of Western Europe's advanced industrial societies. The central organizing concept of these Latin Americanists was *corporatism*, which was seen as the key feature of Ibero-Latin American political systems and gave them their distinctive political life. This work, which eventually spawned a vast outpouring of studies of corporatism in northern European countries, was, however, not very fruitful for explaining the Portuguese case. The corporatist framework tended to distort the reality of Portugal's political life because it exaggerated the importance of structures of interest intermediation that were in fact little more than paper organizations and insignificant for the political process during the Salazar-Caetano dictatorship.

It should also be noted that Portugal received some considerable attention immediately after the overthrow of the dictatorship by the Armed Forces Movement (MFA) on 25 April 1974. This event generated an outpouring of works seeking to explain the "Revolution of the Carnations" and subsequent events. Unfortunately most of these works were produced in large part by journalists and scholars who knew very little about Portuguese history, society, and politics. Consequently, they did not have the historical and sociological perspective necessary to understand well the events that took place. Sound scholarship on the period of Portugal's transition to democracy is, therefore, extremely limited, being confined, until the publication of the above volume by Nancy Bermeo, to the papers presented at two meetings organized by the International Conference Group on Portugal and published in two volumes of proceedings:

*Contemporary Portugal: The Revolution and Its Antecedents*, edited by Lawrence S. Graham and Harry M. Makler (Austin: University of Texas Press, 1979) and *In Search of Modern Portugal: The Revolution and Its Consequences*, edited by Lawrence Graham and Douglas L. Wheeler (Madison: University of Wisconsin Press, 1983). Two additional works need to be mentioned in this regard, Thomas C. Bruneau's *Politics and Nationhood: Post Revolutionary Portugal* (New York: Praeger, 1984) and my *Portugal's Political Development: A Comparative Approach* (Boulder: Westview, 1985).

The three books under review here are, therefore, welcome additions to the small body of sound scholarship that has been produced on Portugal. Unlike the corporatist literature, these works do not view Portuguese political life as unique and fundamentally different. Nor were they written by individuals lacking a historical and sociological perspective on Portugal. These books were produced by acknowledged and respected experts on Portuguese political and social life who have studied the country for an extended period of time. They generally seek to understand Portugal by considering it from within the broad tradition of comparative scholarship and in doing so have brought Portugal into the comparative research tradition on Western Europe, the tradition to which the country properly belongs by virtue of its location, history, and political and social experience.

Of these books, Nancy Bermeo's is the most theoretically ambitious. It deals with the rural upheavals that took place in southern Portugal (the Alentejo) in the wake of the collapse of the dictatorship. The book is divided into three parts. The first deals with the land occupations, the second with the management of the collective farms produced by these land seizures, and the third with Portugal's rural rebellion within the context of national-level politics. The primary goal of the volume is to take issue with two distinct but related bodies of theoretical literature based on the Portuguese experience. The first is that body of sociological thought which holds that rural proletarians are incapable of challenging property relations unless they are organized and led by outside groups, specifically radical political parties or unions. The second is that which argues that worker control does not neces-

sarily lead to more equalitarian decision making within the production unit or to a higher level of productivity.

With respect to the first of these bodies of theory, Bermeo's evidence shows conclusively that the land seizures in the Alentejo do not support the idea that rural proletarians cannot act on their own. By carefully analyzing the way land occupations occurred, Bermeo shows that Portugal's southern rural labor force seized land not under guidance from outside radical forces as has usually been surmised but on their own initiative. She argues that these rural proletarians were impelled to act by the social and economic conditions (grossly unequal land tenure, secularization, military conscription, and seasonal labor migration) of the Alentejo. According to Bermeo, rural workers seized land when changes in Lisbon released the grid of authority that had bound fast the economic and social order of southern Portugal. She shows that the outside radical force usually considered as having instigated the land seizures, the Portuguese Communist party (PCP), did not lead the peasants onto the land as their vanguard but followed them.

In the second part of the book, Bermeo shows that worker control is associated with greater equalitarianism, more rank-and-file participation taken by the cooperatives, and a greater unit productivity. Although she finds that the cooperatives are equalitarian and grant numerous opportunities for participation in the decision-making process, the tendency of most workers is to defer to the elected managerial hierarchy. This, she argues, is not due to the lack of interest but to structural constraints such as illiteracy and the lack of organizational skills and, therefore, of confidence among collective-farm workers. Bermeo also finds that worker control is clearly associated with increased levels of political participation and activism in those regions of the Alentejo where they are most in evidence.

In the third part of the book, Bermeo does a masterful job of showing how politics took precedence over class conflict after the collapse of the dictatorship and how this influenced the outcome of the land seizures. She shows that the rivalry between the PCP and the Socialist party (PS) for control of the political situation at the national level led the PS to turn *against* the land seizures and cooperatives, and the PCP *towards* them. In order to defeat the PCP,

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the PS collaborated with the Center and Right to dismantle the collectives, which gradually became a bastion of PCP support outside of the industrial zones of Lisbon, especially after 25 November 1975 when the Communists lost all hope of gaining control of the state apparatus.

The most interesting and theoretically most important aspect of the book is the data that Bermeo has provided on the attitudes of rural proletarians, based on in-depth interviews conducted with over one hundred such individuals. These interviews reveal that the rural workers in the Alentejo seized land for instrumental reasons; that is, they occupied land to protect their livelihoods in the face of uncertainty created by the collapse of the dictatorship. Most of those interviewed reported that they wanted to work unused land and wanted to pay rent for its use. These responses give rise to a theoretical question bearing directly on the author's thesis; that is, does the seizure of land by rural proletarians who want to work unused or underutilized land and *pay rent* for its use constitute a challenge to the basic property-owning relations of the region? If it does, then the author's thesis that rural workers can take independent *radical* action is supported. If it does not, then the question of how rural proletarians who have acted independently but for instrumental reasons eventually produced a radical form of agricultural organization, the collective, needs more discussion. Bermeo, who recognized this problem, argues that Portugal's rural workers were radicalized by forces inherent in the "history" and "sociology" of the Alentejo. Could it not be, however, that outside forces played some role in this process, either individual workers who had been radicalized during their personal migration to Lisbon or the PCP as it followed the workers onto the land? On this point, more research is needed, which Bermeo recognizes.

The book by Thomas Bruneau and Alex MacLeod is a study of the political system installed in Portugal after 1974. The authors argue that Portugal does not need to be treated as a unique case (if ever it needed to be) and can be analyzed using "concepts" and "categories" common to the study of other West European democracies. The general purpose of the book is to show how Portugal's four major political parties, the PCP, PS, the Social Democrats (PSD) and the Christian Democrats

(CDS), not unlike parties in other West European successor democracies, have captured and divided among themselves the body politic. According to Bruneau and MacLeod, this domination of political life by the parties has been very detrimental to democracy because it has politicized every nook and cranny of the Portuguese political system and has brought on severe *imobilidade* and extreme governmental instability.

The book can be divided into two parts. The first describes the emergent party system as a whole and each of the major parties separately. A wealth of useful information is presented on the organization, electoral strategy, bases of support, degree of grass-roots penetration, leadership quarrels, and factionalism in each of the parties. This part of the book also contains a discussion on Portuguese interest groups, which are found to be few in number and not very important for the political struggle because any influence they do have is mediated by the political parties through whom they must act.

The second part of the book discusses the negative consequences of excessive party domination. A chapter on the 1976 constitution and revisions made to it in 1982 shows that this document can be seen as a temporary truce among rival political parties who need to co-exist within the same political system and not a fundamental agreement among them on the political and economic framework for the country. A chapter on the parliament shows how seats in that body do not represent constituents but are, in effect, at the disposal of the parties who won them. A chapter on the media shows how the state-owned broadcasting network has been colonized by the parties. Finally, a chapter on nationalized enterprises, a very large sector of the Portuguese economy, demonstrates how the appointment of managers and directors has more to do with party loyalty than technical expertise.

The image of the Portuguese political system conveyed by Bruneau and MacLeod, then, is that of political fragmentation and stagnation owing to excessive politicization. While the authors have done a superb job of describing this overpoliticization, they do not offer the reader much in the way of explanation. Here and there the book contains references to other West European political systems that have experienced similar political fragmentation and



tantalizing references to degrees of "institutionalization" and the "personalization of politics," but there is no sustained systematic discussion of these concepts or cases and how they can help explain the present state of Portuguese political life. Despite this shortcoming, this is by far the best general treatment of the current political system in Portugal that has been published to date.

The third book, edited by Kenneth Maxwell, brings together the writing of a group of experts on Portugal that includes a former Portuguese prime minister and a former minister of industry, as well as Portuguese and non-Portuguese academics and journalists who analyze the current political, economic, and social situation in Portugal. Maxwell introduces the book with a brief but thorough survey of events since the collapse of the dictatorship a decade ago. This is followed by a journalistic account of daily life in contemporary Portugal by Diane Smith, a long-time resident who reports for the London *Financial Times*. These two chapters set the stage for the six substantive chapters which follow.

The first three of these chapters deal with Portuguese foreign relations. In one, Rainer Eisfeld, a West German political scientist who has published widely in the West European social-science literature on post-1974 Portugal, examines the structural problems associated with Portugal's accession to the European Economic Community (EEC). Eisfeld shows that the rationale for Portuguese membership was primarily political not economic; that is, membership was a way of assisting in the consolidation of democracy. He also suggests that membership will not only change Portugal in fundamental ways but will also change the EEC itself because Portuguese membership, along with that of Spain and Greece, has expanded the size of the community's less developed regions considerably, while its organizational structure, originally designed for advanced industrial countries, has remained the same. Bernardo Futscher Pereira, a doctoral candidate at Columbia, analyzes Portugal's relations with Spain. He shows how ancient Portuguese antipathy toward and distrust of Spain have impeded harmonious relations between these two Iberian nations in various policy areas: Spain's application for membership in NATO, trade relations, and fishing rights. António de Figueiredo, a dis-

tinguished Portuguese journalist, analyzes Portugal's relations with Africa and explains why the ties broken by the withdrawal from Angola, Mozambique, and Guinea (Bissau) have been retied with such rapidity. These three pieces are an important addition to the literature on Portugal because so little has been written about the country's foreign relations.

The second three of the six substantive chapters deal with domestic problems. João Cravinho, a former minister of industry, surveys Portugal's present economic crisis. He argues that the crisis is not due to temporary, short-term or misguided government policies but is caused by deeply rooted structural aspects of Portugal's economic organization. Governmental instability aggravates the crisis because it inhibits the development and implementation of a coherent long-range plan for economic modernization.

The chapter on Portuguese society by Boaventura de Sousa Santos, a legal sociologist, answers the question implicit in Cravinho's chapter: If the Portuguese economy is in such a bad state, with no hope for improvement in the immediate future, why has there not been a greater manifestation of discontent among the Portuguese population? In other words, why has life proceeded with such normality in the face of such obvious economic deterioration? According to Cravinho, the effects of the crisis in the economy are mitigated within Portuguese society by the actions of certain "compensatory mechanisms," some historical and some of relatively recent vintage, having risen since 1974. The most important of these compensatory mechanisms is the agricultural system found in northern Portugal. In that region, where live about 60% of the population, countless small family-owned farms supplement industrial wages and provide alternative food supplies. Other mechanisms identified by Sousa Santos are the "ruralization" of cities (the coming to the fore of rural habits temporarily suppressed by recently arrived city dwellers), a nationwide rent freeze (presently being phased out), and a vibrant underground economy. The author points out that while some compensatory mechanisms, such as small farms, may help the population weather the economic crisis, they ironically contribute to the maintenance of the structural problems pointed out by Cravinho inherent in Portugal's economy

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and make modernization an even more difficult task.

In the final chapter, Pinto Balsemão, a former prime minister, examines the factors that have led to a high degree of political instability in Portugal since 1974 (10 governments since 1976, not one of which has survived more than two years). Balsemão focuses on the constitution; the electoral law, which specifies proportional representation; and the semipresidential nature of the regime. Like Bruneau and MacLeod he sees the constitution as the embodiment of a temporary political truce among competing political parties and not a practical attempt to solve differences democratically. This has produced in Portugal what Balsemão calls the "institutionalization of the temporary," which makes it impossible to generate long-term coherent policies for solving problems and undermines the authority of the state. He argues that reforming the system will be very difficult because of "situationism," which refers to the control of Portuguese politics by a small political class and their clientele, who would lose power and privilege if fundamental changes were made. Maxwell's book is the best overview of the foreign and domestic policy problems faced by Portugal today. It is a must for scholars and policymakers interested in Iberia generally and Portugal specifically.

A general conclusion that can be drawn from these three books is that the political system that emerged in Portugal in the wake of the collapse of the dictatorship is the result of a temporary truce among fiercely hostile political-party elites who share a commitment to democracy at the ideological level but do not see it as practical means of resolving differences. This lack of practical commitment has generated a very high degree of political instability and aggravated economic and social problems during the last decade and will continue to do so for the foreseeable future. Stable democracy will only emerge in Portugal when its political class honestly faces the issues that divide it and devises effective democratic procedures for resolving them. This, of course, is not a situation unique to Portugal or the West European political experience. It might be useful to examine the experience of other West European successor democracies that have either escaped this kind of politicization or learned to live with it and even prospered. If this were done, our understanding of Portu-

gal's future as a democracy would be greatly enhanced.

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### **Capitalism, Socialism, and Technology: A Comparative Study of Cuba and Jamaica.**

By Charles Edquist (London: Zed Books, 1985. 196p. \$27.25 cloth; \$6.95 paper).

**Democratic Socialism in Jamaica: The Political Movement and Social Transformation in Dependent Capitalism.** By Evelynne Huber Stephens and John D. Stephens (Princeton, NJ: Princeton University Press, 1986. 480p. \$55.00 cloth; \$14.50 paper).

**Class, State, and Democracy in Jamaica.** By Carl Stone (Westport, CT: Praeger, 1986. 175p. \$32.05).

The failure of the development process in Jamaica, as throughout the Third World, has resulted in the social and economic marginalization of large numbers of people. An unemployment rate hovering around 30%, a level of production that in per capita terms is only three-quarters what it was 15 years ago, and a huge foreign debt are only a few examples of what this failure means for the Jamaican people.

This ongoing crisis—how to understand it and what to do about it—is the thread that links the three books under review here. Each provides important theoretical insights about the political economy of underdevelopment and makes a significant contribution to the literature on Jamaica as well. Evelynne Huber Stephens and John D. Stephens look to the Jamaican case to explore the possibilities of a transition to socialism that both truly transforms social and economic relations and deepens democracy. Charles Edquist focuses on one sector, the sugar industry, identifying the causes and consequences of Jamaica's failure to modernize sugarcane field operations. But he contrasts this to the more positive experience in Cuba and in so doing raises an important set of theoretical issues bearing directly on development experiences in the Third World. For Carl Stone, the issue is not so much change as stability; he analyzes the conditions that have allowed Jamaicans, in contrast to many people in the Third World, the relative luxury of a

strong and stable liberal democratic order.

The Stephensens focus on the important 1972–1980 period in Jamaica. During this time Michael Manley and his People's National Party (PNP) government adopted a democratic-socialist ideology and attempted to transform Jamaican reality along those lines. While this effort did produce certain fundamental changes, it also was associated with a sharp deterioration of social and economic conditions and the ultimate rout of the PNP in the general elections in 1980. The Stephensens' analysis uses this experience not only to provide what will become the standard reference for any discussion of this important case but also to develop perhaps the most incisive analysis to date of the nature of a democratic-socialist development path in the Third World.

One key conclusion that emerges is that this generally negative experience *does not* demonstrate the nonviability of the democratic-socialist path. Rather, it serves to highlight the serious policy errors made by the Manley regime. These strategic and tactical errors, for the most part committed before 1976, effectively limited the regime's options in the ensuing period and initiated an irreversible process of political and economic decline.

The possibility of carrying out an effective political strategy ensuring a democratic transition to socialism, the Stephensens argue, founded on two important areas of policy failure. First, the 1976 foreign-exchange crisis would have been less severe, and overall economic performance much improved if the regime had not squandered its scarce resources, especially those generated by the tax levied on bauxite producers in 1974, on distributionist measures. Rather, the emphasis should have been on productive state-sector investment projects and strong program implementation. This, along with other modifications in PNP policy, would have reduced interclass hostility, reduced capital flight, and retained key professional and managerial resources within the country. They also argue that the Manley regime should have kept a greater distance vis-à-vis Cuba and engaged in less antiimperialist, anticapitalist rhetoric. Both areas of policy error contributed to a failure to fulfill a crucial strategic component of the democratic-socialist model, the need to construct a political compromise or entente between segments of the capitalist and managerial classes and the democratic-socialist

class alliance (working class, peasantry, lower middle class, and urban marginals).

Though they emphasize these contingent issues of policy choice, the Stephensens do not ignore the structural constraints imposed by dependent capitalism on any country seeking to transcend its clutches. Indeed, as they show, many of these factors, such as the chronic balance-of-payments problem and excessive import dependence and export concentration that the Manley regime inherited, were especially formidable in the Jamaican case. Nor do they ignore other historical and country-specific constraints, such as Jamaica's geopolitical importance to U.S. imperial interests or the dominance within Jamaica of imported patterns and values. They just do not see these as determinative in this instance.

Here the Stephensens enter onto difficult theoretical terrain. On one hand, they argue that the democratic-socialist model requires some accommodation with segments of the capitalist class ensuring at least toleration of the program. On the other hand, they claim that it also required a process of "social and political mobilization and ideological definition" (p. 299). There is only a fine line, however, between building a class-based socialist movement and not overly antagonizing elements of foreign and local capital.

This fine line means that achieving both strategic objectives is exceedingly difficult. The difficulty—and the tendency for an accommodationist approach to end up as a process of co-optation of the masses—is magnified, given the concrete realities of the transition. In Jamaica these included media largely controlled by capital, close ties between foreign and local capital, and a sociopolitical system open to, and heavily influenced by, metropolitan values and interests. The more conservative segments of the capitalist class had enormous resources to influence public debate and consciousness. Their sensitivity was heightened in such a resource-poor society as Jamaica, with a history of severe class divisions and tensions. In such a situation, anything that empowered and mobilized peasants and workers was likely to be perceived as anticapitalist and threatening. In this situation the Democratic Socialist party, committed to a political compromise with segments of capital, was at a clear disadvantage.

The likely outcome of this situation is a set

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of policies that might have a better economic result than those actually adopted in the Jamaican case, but without the same class orientation. Let me illustrate with an example. Between 1975 and 1977, the three largest sugar estates in Jamaica were transformed into 23 sugar-worker cooperatives. This was a controversial decision at the time, opposed by many both within and outside the administration. This decision and the subsequent difficulty experienced by the cooperatives became a key propaganda issue used against the Manley regime. It is likely that a democratic socialist regime more sensitive to economic performance and accommodationist goals, as the Stephensens suggest, would not have moved so rapidly to establish cooperatives. A state farm approach, favored by many at the time, was the likely alternative. What would have been the class orientation of such a decision? From the sugarworkers' point of view, the social relations of production resulting from a state enterprise, without a workable model of worker participation, would not have differed significantly from the old plantation model. Second, the economic decisions of such a bureaucratic entity, given the very difficult socioeconomic realities of the estates, would almost certainly have been much harsher for the workers than under cooperatives. The end result would have been more akin to the situation following nationalization of the sugar estates in Guyana: far less empowerment for the workers and far more empowerment for state bureaucrats.

A second major difficulty with the Stephensens analysis revolves around the agricultural sector. Despite their recognition that "a (if not *the*) key role in reducing dependence certainly has to be played by agriculture" (p. 274), neither their theoretical nor their empirical discussions do justice to the agrarian question in the transition to socialism. A crucial issue here is the importance, and possibility, of modernizing the export sector. (I will come back to this shortly in my discussion of the Edquist book.) Equally important is the need to eliminate rural poverty and rural-urban migration on food imports. In a situation such as Jamaica's, with a fundamental problem being the enormously inequitable distribution of resources, primarily land, in the rural parts, it is inconceivable that there can be a meaningful move to solving these problems without

land reform. While there can be no gainsaying the political, social, and economic difficulties of such a path, there is no other way. Although mentioning the crucial role of the agricultural sector, the Stephensens do not adequately develop this point. Partly for this reason, they gloss over the meager efforts of the PNP in this regard. The "land reform" that occurred during this period (except in the case of the cooperatives, which were largely forced on the government) hardly deserves any such label.

It is important to consider this further. Any meaningful effort at land reform in the Jamaican context, any effort to redistribute power in the rural areas significantly, must contest terrain controlled by the foreign and local bourgeoisie. The local rural bourgeoisie, moreover, are heavily intertwined with urban elites. As Stone shows, the old rural oligarchy has been largely replaced—much of the locally owned estates are held by urban entrepreneurs or rural elites with modern orientations, many tied to the PNP itself. In other words, a meaningful effort at land reform would almost certainly do much to disrupt the entente between capitalist elements and the democratic-socialist class coalition. But a failure to carry forward such an agrarian strategy risks making a mockery of any socialist transition or the effort to improve socioeconomic conditions for the masses in Jamaica.

Charles Edquist does not focus on land reform in his book but he does emphasize the importance of agricultural modernization. The land reform that occurred in Cuba between 1959 and 1963 was a key condition for the technological transformation of its sugar industry. This sort of technical change is fundamental to the development process: "If modern techniques are not introduced in Third World countries, these countries will continue to be left behind and the vicious circle of underdevelopment will continue to operate" (p. 164). What are the conditions that facilitate the introduction of modern techniques and what are the socioeconomic consequences of technological choice? In answering these questions, Edquist develops a theory of technological innovation and compares the differential response of capitalist Jamaica and socialist Cuba to technological opportunities in the harvesting of sugarcane. His theoretically informed analysis makes this an illuminating study of why and how development does or

does not occur in the Third World. It is also the best discussion to date of the modernization of Cuban agriculture.

Cuba's is a successful story of technological modernization. By 1985, almost two-thirds of the sugarcane crop was reaped by machine. The positive consequences of this achievement cannot be overemphasized. Harvest mechanization resulted in

1. the transference to thousands of workers of the skills to operate and repair complex farm machinery
2. the freeing up of the labor power of around 200 thousand Cuban workers
3. the establishment of a nationwide system for the maintenance and repair of harvesters
4. the acquisition by many Cuban technicians of valuable design and manufacturing experience
5. the development of a new industry, with the construction in 1977 of the largest cane-harvester manufacturing plant in the world.

In Jamaica, however, the capitalist social relations of production precluded efforts to mechanize the cutting process. Why? First, a situation of high unemployment greatly increased the political and social costs of any effort to mechanize the harvest. Second, the parameters of decision making in a capitalist economy also constrained technological change. The developmental fruits of the mechanization process can take decades to ripen; significant costs need to be borne in the interim. In Cuba, for example, it took about 15 years before harvesting machinery was really operational. Could a capitalist Jamaica undertake such an investment? In Jamaica such investment decisions were characteristically undertaken by firms, based on cost-benefit ratios calculated from the firm's point of view only. These firms, moreover, characteristically adopted only a short-term perspective, but the benefits of mechanization tend to accrue in the long run. Further, in the vertically integrated operations characteristic of Jamaica, a high degree of the value added in the production of sugar occurred in the processing and marketing of sugar, not in the farm production of cane. Hence there were fewer incentives to undertake expensive investments in mechanizing the cane harvest.

Third, the Jamaican state, at least until the 1970s, exhibited little interest and had few resources with which to initiate or coordinate the extensive effort needed to mechanize harvesting operations. In the 1970s, as part of its democratic-socialist program, the PNP *did* greatly expand the government's role in the sugar industry. But a labor-displacing mechanization policy was largely precluded by its close ties to one of the major trade unions and its dependence on workers for political support. This issue, however—the role of a democratic-socialist regime in fomenting a process of agrarian change and modernization—is not developed well in either the Stephens or Edquist books. It is a crucial area for further analysis.

If the Manley regime neither succeeded in its ambitious goals of social transformation in Jamaica, nor initiated a meaningful process of agrarian development, one important result did ensue. Jamaica's democratic political order weathered enormous political, social, and economic turbulence without significant damage. Jamaica, Carl Stone argues in *Class, State, and Democracy in Jamaica*, is a rarity in the Third World: socioeconomic instability and dependence are combined with a stable liberal democracy. Moreover, according to Stone, it is durable: "the flexibility and adaptability that have evolved . . . suggest that the democratic trends are likely to grow rather than diminish in Jamaica in the future" (p. 191). Alas, the conditions of socioeconomic deprivation for the masses also appear to be durable. The liberal democratic order appears incapable of ameliorating the conditions of a majority of Jamaicans, those very Jamaicans whose acquiescence to the political order is a condition of its stability. I foresee major tensions and disorder in the future of Jamaican democracy, rather than the growth that Stone predicts.

What, according to Stone, is the basis of Jamaica's "political success"—its development of a bourgeois political system in the face of a class system that appears inhospitable towards it and of a high capacity for managing the "certain element of turbulence" that rocks this political status quo from time to time? First, the gradual and consensual process of decolonization took place at a time of striking economic growth in Jamaica. This growth, moreover, effectively established a new, urban, and entrepreneurial elite as the dominant element

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in the economy. But, according to Stone, the economically privileged strata remained relatively detached from the main source of political power in Jamaica—the two political parties and the “outstanding” group of middle-class professionals who control them. This political ascendance of a stratum influenced but not controlled by the economic elite has allowed it to balance class conflict in Jamaica.

So the relatively autonomous political parties and the political system they control have played an effective role in “accommodating” and “reflecting” the interest of the masses; the latter in turn have tied their fate to the parties, largely eschewing other mechanisms of political expression. This exchange—political support for some welfare benefits—is nothing other than a system of political clientage. While recognizing some of the “weaknesses and deficiencies” in such a system of political representation, Stone appears surprisingly unconcerned about its manipulative nature.

While Stone is correct in arguing that there have been changes in the racist and oppressive plantation system that dominated Jamaica up to the 1940s, surely he exaggerates the transformation when he heaps accolades on the “highly competitive social system” that has developed. This is a society long identified as having one of the most skewed patterns of income distribution in the world.

It is also surprising that this political sociology of democracy in a Third World

country closely linked to U.S. economic and political interests could effectively ignore a number of key actors. Multinational capital and U.S. political elites have important interests in Jamaica; their penchant for manipulating democratic processes in dependent Third World countries is well known. The reader will not find these issues discussed or even recognized here. The actual and potential role of the Jamaican military-police establishment is also neglected in Stone's analysis. Yet defense now claims a share of the government's operating budget close to twice what it was in the early 1970s, with the absolute amount growing almost 20-fold in this period. Stone's discussion would have been greatly enriched if he had attempted to confront, rather than ignore, these issues.

The need to transcend dependent capitalism in the Third World and the difficulties involved in doing this, the implications of the Cuban agricultural development model for other countries, and the conditions for, and implications of, liberal democracy are all fundamental problems of empirical and theoretical importance. These three books share a concern for, and an awareness of, these issues. Both Caribbeanists and non-Caribbeanists interested in development or comparative politics will find them stimulating and insightful.

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**Liberation and Its Limits: The Moral and Political Thought of Freud.** By Jeffrey B. Abramson (Boston: Beacon Press, 1986. xi, 160p. \$14.95 cloth; \$7.95 paper).

**Political Ensembles: A Psychosocial Approach to Politics and Leadership.** By Graham Little (New York: Oxford University Press, 1985. viii, 223p. \$29.95).

It is refreshing to see two books whose methodological perspective begins from the utility of psychoanalysis in the study of politics and political life. Both Abramson and Lit-

tle argue that concepts like *the unconscious*, *repression*, *self*, *Eros*, and *psychosexuality*, can be useful in delineating the structure of political life. Both agree the study of politics requires a perspective that takes seriously the depth character of the self, the fact that unconscious structure and process affect the action and values of politics. Psychoanalysis as a way of looking at the world moves political analysis beyond its concern with the rational, the formal, the statistical and behavioral.

Little seeks a broad theory of interpretation that relies on both sociological and psycho-

analytic insights to explain political phenomena. Abramson focuses directly on Freud's clinical and philosophical formulations, specifically the concept of eros and the relationship between eros and liberation.

Both projects are ambitious. Little devises a theory of political form or "ideal" types; what he calls *Structure*, *Group*, and *Ensemble*. He grounds these types in psychological category: thus, *Structure* brings with it self-versus-other relations, the strong leader, a more authoritarian or conservative kind of politics. *Group* encourages an emphasis on solidarity, self-in-other, less focus on private interest and individual will and rights. The *Group* leader shows more sensitivity to what the group is, what it thinks; he or she emphasizes collective goals and aims. The real-world equivalent would be leftist or socialist politics. *Ensemble* describes self-and-other, with a form of leadership Little calls "inspiring," where issues, personality, relationship, and progress define the dialogue between leaders and followers. The *Ensemble* leader is more of a participatory type, with an optimism about the resolution of issues and a sense of the future that brings adventure, change, possibility, and transformation. (Little for example sees Gary Hart as an *Ensemble* leader, although the *Ensemble* coalition, because of its hopefulness, may find itself brought to disillusionment and despair.)

Psychoanalytic theory helps to elaborate and draw out these political formations and types of leadership. For *Structure*, Freud's drive theory and his view of paternal authority, renunciation, and guilt may be considered the appropriate model. For *Group*, effective interpretation derives from the British (psychoanalytic) object relations theorists: Melanie Klein, Wilfred Bion, and D. W. Winnicott. And for the *Ensemble* relation, a participatory politics of cooperation, friendship and "play," in the sense of the spontaneous distribution of political energies, Little turns primarily to the psychoanalytic thought of Heinz Kohut and the sociological theory of Victor Turner.

Several assumptions guide both works: psychoanalysis presupposes a certain skepticism regarding human action and belief; it embraces the demystification of illusion; it presumes the embeddedness of the human self in conflict and desire. It supports change and the liberation of the self from internal and

external constraint, although as a theory it is ambivalent on what liberation or restraint signifies and involves. Psychoanalytic insight is more than the dreary and pessimistic conclusions of *Civilization and Its Discontents* or *Totem and Taboo*. Nor does Freud or modern psychoanalysis necessarily support a rigid view of psychic determinism, either as clinical or political analysis. Both Little and Abramson see psychoanalysis as an interpretive method that looks for and encourages transformation in the individual and the polity, that broadens and humanizes the horizon of what politics means as a collective action. Yet they argue that the unconscious possesses a tremendous power and significance. Its presence should not be overlooked or underestimated, since to be human is to be more than a "rational actor." Little observes, "The freest man is also the most vulnerable to the unconscious, the most enlightened, to the darkest forces. This is Freud's respectful reply to Nietzsche" (p. 176).

Both arguments take issue with the Hobbesian notion that "nature" and "human nature" precede the self into the social world. Rather, the internal (the "intrapsychic") and the external (interpersonal) operate dialectically; "the individual is neither wholly himself nor wholly the construction of other people or society" (Little, p. 167). Unconscious fantasy, sexuality, longing, concern, envy, rivalry, hate: what occurs and transpires in the realm of *desire* frames the relation between self and politics: "Desire and rivalry boil under the cool hand of structure. Envy jostles equality and brotherly love in the *Group*. Hope and exquisite sociability waver on the abyss of narcissism, delusion and despair in the *Ensemble*" (p. 66)—an interesting observation of *Ensemble* in the context of the unraveling of the Hart campaign.

Eros in Abramson's view binds communities; in this respect, it is a creative, life-sustaining dynamic: "Much of what Freud notably taught about health and happiness depends on his appreciation of this link between human eros and human sociability" (p. xi). In Little's terms, this concept of Eros would probably fall under the *Ensemble* political type and the "inspiring" leader.

Yet Eros, Abramson argues, may become, particularly in groups, a form of oppression. He describes impediments to a politically expansive Eros and criticizes the Hobbesian

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ment of studies in the areas of mass media, public opinion, and education. The major strength of Bélanger's discussion, in fact, rests in its ability to organize a great deal of social theory and research in the behavioral as well as Marxist traditions and to indicate the extent to which scholarship in what are conventionally viewed as competing streams often seeks to explain similar phenomena.

On a more practical level, what is the utility of Bélanger's proposed framework? This question is addressed most directly in a concluding chapter entitled, "A Structural View of Feminism; or, What Can the Framework Tell Us about Feminism." The contemporary women's movement is mentioned initially in chapter 2, where the author suggests that social conflict follows from "a refusal to accept controls" and, in the case of the feminist movement, from "opposition to controls which currently remain in men's hands" (p. 55). This formulation is expanded at the end of the book in an effort to explain how this particular social movement takes issue with established forms of social control, notably masculine domination of Western culture as it is reinforced through traditional sex roles in the family, educational system, labor force, mass media, and so on.

From the perspective of political science, Bélanger offers a fairly innovative view of how male control of social regulation (i.e., governance) has led to two waves of feminist activism in a number of different cultures. While the forms of feminist mobilization have varied, from middle-class Protestant reformism in Anglo-American systems in the early twentieth century to socialist and Marxist variants in contemporary European states, Bélanger argues that the challenge confronting women's movements is quite similar across time and place; that is, the threat of institutionalization and deradicalization opens a crucial set of questions for researchers who "may wonder whether the movement became deradicalized because it evolved progressively toward institutionalization, or whether, on the contrary, it became institutionalized because it resigned itself to deradicalization" (p. 197). Since this type of problem is rarely posed in general discussions of politics and society, *Framework* is notable for considering women within its parameters. Readers must ask, however, whether the real-world issue here is not better

identified as the resiliency of traditional control and conditioning mechanisms rather than as the strategic failures of an ongoing mobilization process (i.e., feminism).

In short, Bélanger's framework holds promise for political scientists and sociologists interested in social movements, but the starting point of the approach—social control—remains crucial in explaining how mobilization is frequently insufficient in the face of well-entrenched socialization processes.

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**Cities of Gods: Faith, Politics, and Pluralism in Judaism, Christianity, and Islam.** Edited by Nigel Biggar, Jamie S. Scott, and William Schweiker (Westport, CT: Greenwood, 1986. viii, 244p. \$39.95).

The revival of public concern with religion and its relationship to politics has stimulated a parallel revival of interest on the part of the academic community in the intersection between those two major forms of collective human expression. This is an abrupt change after a long period in which religion was relegated to the realm of "primordial culture," whose impact was seen as essentially residual and lessening. This new interest takes two forms: empirical studies of the relationship between religion and politics and studies of the impact of religious ideas on political life and thought. The latter, in particular has led to a revival of political theology, which today is probably an arena for more creative thinking than political philosophy.

*Cities of Gods* apparently tries to combine both aspects of the relationship. In doing so it produces a rather confused result. The confusion begins with its title, which suggests a certain detachment and relativism about the nature of the divinity, when in fact the only religions discussed are the three monotheistic faiths that not only share belief in the same God but insist that there is only one God as the cardinal element in their respective belief systems. Moreover, the hint of detached scholarly objectivity is not at all reflected in the book, whose authors are very much believers and very much engaged in their religious communities. This is not to suggest that the articles are



merely subjective. Most tend to confront their traditions quite candidly, but, with one or two exceptions, they are written by committed people.

The confusions continue in part because the three religious communities discussed are so different and nothing is done by either the editors or the authors to find a common basis of comparison. Thus Judaism and Islam are correctly presented as fully political religions, that is to say, they require a shared political community for their fulfillment; while Christianity is presented as detached from the polity, though committed to trying to guide it, often from an opposition stance ("the Protestant principle"). At the same time, Judaism is wrongly presented as finding its political expression only through the Jewish state of Israel with no expectations of a Jewish political life in the Diaspora, at least not since the Emancipation. In contrast, Islam is presented as both political and ecumenical, struggling with the development of an Islamic political community for hundreds of millions of people living in many different states. Since much of this is true, it greatly complicates the problem of comparison but nothing is done to deal with these complications. If anything, the treatment of Judaism and Islam must conform to the language of Christianity, which is usually the case in such collections, distorting their own authenticity.

The best chapters in this book are those dealing with Islam. In "Islam and Political Action: Politics in the Service of Religion," Fazlur Rahman provides the best short exposition of the relationship between Islam and politics in theory and practice I have come across. The following two articles by Charles J. Adams and Marvin Zonis build on Rahman and add appropriate nuances and modifiers. Only the last chapter by Khalid Bin Sayed is more in the nature of a radical tract than political thought or analysis. It is useful as a document exemplifying contemporary Islamic radicalism. The point that all four authors make, which deserves careful attention by the West, is that even Islamic modernists are not Western liberals. They see themselves as functioning fully within the Islamic tradition, which is not a liberal tradition, even when they make a strong case that it encourages democratic republicanism.

The four articles on Judaism are correct, yet,

in this reviewer's opinion, seriously flawed. Manfred Vogel is not off base when he sees "the state as essential expression of the faith of Judaism," but his discussion of the subject is so convoluted as to be of little help to a political scientist. Substantively, his error is in his use of the term *state*. The Jewish political tradition clearly rejects the notion of a reified state—until modern times there was no generic term for *state* in Hebrew. A proper polity in that tradition is the *edah*—congregation (in the original sense of the term).

Robert M. Seltzer makes liberal pluralism the touchstone of his belief system and measures Judaism in terms of whether or not it conforms, rather than analyzing Judaism on its own terms; whereas Mitchell Cohen's article, "Pluralism and Theocrats: The Conflict between Religion and State in Israel" is simply deficient. Not only does he show a lack of understanding of the many nuances and complexities in the relations between religion, state, and society in Israel, painting a highly inaccurate picture in stark black and white, but he has not troubled to familiarize himself with the by now rather abundant literature that documents and analyzes those nuances. John Pawlikowski is a sympathetic observer, as his title indicates, but he has not overcome the problem of thinking about the subject in Christian terms.

The weakest part of the book is part 2, on Christianity. Although Martin Marty's chapter is up to his usual standard, the others show all the confusions of contemporary liberal Christianity when approaching questions of religion and politics. On the one hand, they want to develop a political theology; but on the other, they do not want to acknowledge the legitimacy of the exercise of power other than under the most benign circumstances, where politics is essentially a matter of civil cooperation with no hint of the possibility of coercion. Contemporary liberal Christianity does not have the stomach to hold on when the going gets rough and this is abundantly evident in these chapters. The chapters also demonstrate what happens intellectually when there is the kind of severed connection between religion and polity which is characteristic of the contemporary West, particularly the United States, where the gap between the religious and political communities is so great that the religious community does not bear any responsibility for the

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consequences of political acts, their own or others.

The Islamic section is best perhaps because it is least influenced by Christian intellectual frameworks and terminology, trying rather to see Islam in its own context. The section on Judaism never quite overcomes the fact that the writers are all U.S. citizens involved in religious thought, used to writing in Christian language. This is a constant problem of Jewish-Christian dialogue in the United States and it is one that influences the comparative study of politics and religion as well, as this book amply illustrates. It is to be hoped that those concerned with the effort will devote substantial time to breaking through the limits of language so that truly comparative studies can be developed. In the interim, this book is a good example of where the subject now stands. That is both its strength and its weakness.

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**Rousseau and the Republic of Virtue: The Language of Politics in the French Revolution.** By Carol Blum (Ithaca, NY: Cornell University Press, 1986. 302p. \$24.95).

**Rousseau, Nature, and History.** By Asher Horowitz (Toronto: University of Toronto Press, 1987. 273p. \$30.00).

**The General Will before Rousseau: The Transformation of the Divine into the Civic.** By Patrick Riley (Princeton: Princeton University Press, 1986. 274p. \$27.50).

Without overtly disagreeing with each other, these three recent books give strikingly different pictures of Rousseau. The differences stem in large part from decisions about the proper perspective in which to view Rousseau: whether he should be considered in the light of an earlier tradition or in terms of those for whom he prepared the way; whether he should be read for his place in the history of political thought or for his influence on political practice. Each approach carries its own set of hazards.

Many traps await those who attempt to trace influences on thinkers. To his credit, Patrick Riley avoids the very seductive trap of turning earlier thinkers into "proto-Rousseauians" (p. x). Riley carefully explores the rich

background of dispute about the *general will* in the works of Arnauld, Pascal, Malebranche, Bossuet, Fénelon, Bayle, and Montesquieu to show what this notion meant in the century before Rousseau. By doing so he reminds us that, for Rousseau and his predecessors, fundamental political problems were inseparable from theological questions.

Riley traces the origin of the term *general will* to interpretations of St. Paul's remark at 1 Timothy 2:4, "God wills that all men be saved" (pp. 4-5, 9, 68 n. 8). From this passage come questions about how anyone can be damned and whether salvation comes from one's own efforts or by divine grace alone. The general will was born in 1644 with Antoine Arnauld's claim that God's general will, which is that all sorts of people be saved, does not conflict with his particular selection of a few. Thus the *general will* begins as an explanation of God's justice.

Contention over the divine general will ran through the succeeding generations. The pivotal figure in this debate is Nicholas Malebranche who opposed the Jansenism of Arnauld and Pascal and was later opposed by Fénelon and Bossuet. Malebranche turned emphasis away from salvation to the orderliness of nature, which follows general laws issuing from God's will. He attempted a union of Christianity with the new Cartesian physics, in which the roles of both particular providence and miracles were reduced so much that it was objected that "Malebranche does not really offer nature *and* grace at all: he offers just nature and grace vanishes" (p. 71). Although Riley only occasionally alludes to the high political stakes involved (cf. pp. 6-7), he outlines these theological disputes with painstaking attention to detail.

Next Riley describes the "secularization" (pp. 52, 79, 101, 171) of the general will by Bayle and Montesquieu. He explains Bayle's distinction between generality and particularity in his account of human justice and refers to both Montesquieu's analysis of general natural causes that influence human society and his "conversion of the general will of God to the general will of the citizen" (p. 250). Riley does not decide whether this conversion simply uses or subverts the theological term. In describing this "secularization" he reasonably objects to excessive claims about Hobbes's importance (pp. 177-80). He observes that

Hobbes did not use the term and that Hobbes's sovereign is a particular. Nonetheless, Hobbes does call the law "the general providence" exercised by him (*Leviathan*, chaps. 26, 30). The sovereign's will, like God's, has a general application.

Finally, Riley suggests that the notion of *generality* is the key to "the overall coherence and unity in Rousseau's thought" (p. 258) and points to elements of Malebranchism in Rousseau's fictional characters. However, he could also call attention to the importance of particularity in Rousseau's descriptions of the state of nature (where lack of any sort of general knowledge contributes to contentment), the customs necessary for healthy political orders, and his own life. Reconciling generality with a more fundamental and natural experience of particularity is the crucial issue. Rousseau does present the general will as an imitation of the "immutable decrees of the divinity" (Geneva manuscript of the *Social Contract*), but he claims that the inaccessibility and ineffectiveness of these decrees make this "imitation" necessary. Thus Rousseau's doctrine of the general will is based on a criticism of earlier versions of the doctrine. Riley's book sheds considerable light on the earlier versions against which Rousseau presented his thought.

While Riley looks at the traditional character of Rousseau's language, Asher Horowitz insists upon looking forward to a very different language. Horowitz argues that Rousseau's traditional language conceals his radical novelty and that understanding his depth and complexity requires translating him into the language of Marx and Marcuse rather than that of Malebranche (p. 86). Accordingly, he offers a "left Freudian reading" of Rousseau (p. 4). Rendering Rousseau in such new language invites the question of whether something gets lost in the translation.

Horowitz's "imaginative reconstruction" (p. 9) of Rousseau centers on the historical character of human existence. His account of history in Rousseau stresses labor that "distinguishes human beings from all other forms of being" and is "the primordial form of historical activity" (p. 31; see also pp. 73, 76). Rousseau, however, gives labor a less dominant, though still important, role. In *Emile*, Rousseau immunizes his student against the effects of division of labor, but he presents anger, religion, submissiveness to authority, and sexual desire

as equal or even more fundamental dangers. Moreover, in the *Second Discourse* he identifies "perfectibility" as the distinctive human characteristic (see Horowitz, p. 73). Perfectibility manifests itself in labor, but also in fear, sexual desire, and other factors that bring humans together and separate them, thereby generating history.

From his focus on labor, Horowitz argues that Rousseau identifies at least three "distinct socio-economic forms," each based on a predominant type of labor: "primitive society, traditional or pre-capitalist society, and bourgeois or civil society properly so-called" (p. 90). These categories replace Rousseau's own of savage, barbaric, and civilized societies. Whereas Rousseau claims that nascent society is durable and that most peoples remain in this condition, Horowitz stresses its fragility in the face of technological developments (p. 99). Horowitz's interpretation has a certain validity, but it changes Rousseau's emphasis considerably. Horowitz also insists on the "relatively high degree of technological development and division of labour" in the most advanced societies (p. 114). This formulation may obscure the fact that republican Rome, Athens, Persia, and ancient Egypt all belong in this category of civilized community. Thus, although Horowitz distinguishes Rousseau from Marx on several points, he consistently blurs significant distinctions between the two in his effort to contrast Rousseau with his predecessors.

Using his framework, Horowitz gives novel interpretations of *Julie* as a doomed flight from the tensions of bourgeois society, the *Social Contract* as an ultimately self-contradictory community based on bourgeois premises, and *Emile* as a transcendence of bourgeois society. The most interesting and valuable of these is the discussion of Rousseau's reservations about the community of the *Social Contract*. Although Horowitz could show that these reservations extend to all civilized communities and explain the significance of *Emile*'s political education; nevertheless, he succeeds in demonstrating the superiority of *Emile*'s reconciliation of nature and civilization to the denaturing required for citizens.

Horowitz could apply his general argument about history more radically to the question of the foundations of Rousseau's knowledge. While he argues that Rousseau links philoso-

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phy to history and alludes to the *Confessions* as an example of historical method (p. 29), he dismisses the autobiographical writings as "apologetic introspection and mystical experience" (p. 166). Nevertheless, in these works Rousseau explains how he acquired his understanding of both nature and society in spite of being affected by society. Horowitz does not investigate this point, but his analysis allows the reader to pose this important question.

Unlike Horowitz and Riley, Carol Blum focuses attention on Rousseau's personality. Her book concerns "the fictional construction labeled 'Jean-Jacques Rousseau,'" not the philosopher (p. 13). Blum uses this "cultural artifact," which consists of what Rousseau's readers made of him and his books, to answer the much debated question of his influence on the French Revolution. Although she frequently warns that the main protagonists of her book, Saint-Just and Robespierre, simplified Rousseau's thought, ultimately she considers them "authentically faithful to a central core of the master's teaching" (p. 277). Blum demonstrates that an image of Rousseau cast a powerful spell on the most radical of the revolutionaries and, indeed, on their contemporaries.

Blum elaborates on Jean Starobinski's claim that while Rousseau was not the theoretician of the revolution, "the Terror seems to me to be, on the political level, homologous with what unfolds on the mental level in the autobiographical writings of Rousseau" (quoted pp. 18, 218). In Blum's elaboration, the revolutionaries' image of Jean-Jacques "combined claims of moral superiority with a covert erotic (if infantile) invitation" (p. 50; see also p. 109). Crucial to his seductive appeal was an equation of "the virtuous, the natural, and the asocial" (p. 57). This combination led those infected by it to glorify their own innocence and to condemn the outside world. Especially in Robespierre, the identification with Jean-Jacques led to an obsession with martyrdom and willingness to encourage assassination and terror. In short Blum argues that Rousseau gave the revolution its model of revolutionary psychology.

Blum presents ample evidence that people like Robespierre were powerfully affected by an image of Rousseau. Nevertheless, their (or her) equation of the virtuous, the natural, and the asocial distorts Rousseau's position. Rousseau consistently presents virtue in almost all

of its senses as a political and unnatural characteristic. Accordingly, Blum might have made more of the occasions during the Revolution in which conflicting interpretations of Rousseau were set against each other. The more moderate revolutionaries may have been affected by Rousseau as much as the extremists were and at least as true to his thought.

In spite of the unsympathetic description of Rousseau's personality and inaccuracies on a few points, Blum points to an often neglected practical and theoretical issue. His activity as a novelist and composer shows that Rousseau wished to affect his audience and he certainly intended to inspire identification with himself. As Blum says, "Rousseau's life is a *problem* almost exclusively because of its literary representation by Rousseau" (p. 77). As she further points out, Rousseau used his "life" to overturn the respectability of the rich, the powerful, the intellectuals, religious authorities, and others. He presented a simplified popular image of himself as well as a complex theoretical teaching. Blum's book clarifies a part of the effect of this popular image. More about both this popular effect and Rousseau's deeper importance could be found in how readers such as leading poets of France, Germany, and England viewed Rousseau in the 50 years after his death. No doubt scholars will continue to ponder this thinker, his influence, and his effect.

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**Shi'ism and Social Protest.** Edited by Juan R. I. Cole and Nikki R. Keddie (New Haven: Yale University Press, 1986. x, 325p. \$40.00 cloth; \$12.95 paper).

To begin with the positive, the introduction by Cole and Keddie provides an excellent, balanced account of the multiple forces of Shi'ism and of the factors—historical, social, economic, and political (i.e., Iranian-based inspiration or assistance)—that have prompted increased political activity and social protest among Shi'i groups throughout the Middle East. Moreover, all of the articles are informative and well documented, though not all of the same quality. Cole and Keddie are to be congratulated for finding contributors thor-

oughly acquainted with the area, capable of using primary sources in the different language areas studied, and willing to look at their particular domain in a new light.

Here, however, is where the faults of the book become most apparent. Despite the claims by the editors that "the purpose of this volume is to help fill gaps in our knowledge about the various Shi'i communities who have recently shed quietism for political activism, from Lebanon in the west to Afghanistan in the east" (p. 2), four of the volume's 11 chapters fail to address this general theme. The four are Fred Halliday's account of Iranian foreign policy since the revolution (chap. 3), Nahid Yeganeh's and Nikki Keddie's discussion of how female sexuality has been viewed in Iran prior to and after the revolution (chap. 4), Rudi Matthee's description of changes in the way publications from Egypt's Muslim Brotherhood and Islamic Left movements viewed Iran's revolution (chap. 10), and Muriel Atkin's survey of the various postures taken by Soviet scholars in the last 25-30 years towards Shi'ism and Shi'i political activity (chap. 11). Excellent and interesting as each of these essays is in itself, none sheds light on the basic theme of the book as identified by the editors.

The seven chapters that do address this theme fall into three groups. One is represented by Richard Cottam's subtle characterization of the Iranian Islamic Republic as an authoritarian populist regime and his argument that Muhammad Reza Shah's regime became vulnerable by failing to offer any outlet for the desire among growing numbers of Iranians to participate in governmental activities (chap. 2). Another, the development of Shi'i protest in the Persian-Arab Gulf (that is, in Iraq, Saudi Arabia, and Bahrain) finds expression in R. K. Ramazani's integration of Khomeini's concept of security in the Gulf with case studies of Shi'i political activity in each of these states (chap. 1), Hanna Batatu's detailed analysis of two Shi'i groups in Iraq (chap. 7), and Jacob Goldberg's narrative about the treatment of Shi'is in Saudi Arabia (chap. 9). The third, Shi'i social protest elsewhere, is addressed with respect to Lebanon by Helena Cobban (chap. 5) and Augustus R. Norton (chap. 6) and with respect to Afghanistan by David B. Edwards (chap. 8).

Clearly, there is duplication of effort here.

The articles by Cobban and Norton are so similar that one wonders why both were included or why each author was not asked to focus on different issues. Nonetheless, Cobban's article is especially important for the prognoses she offers with respect to Lebanon's political future, and Norton's for his critical analysis of how Shi'i protest has developed in Lebanon in the last 40-odd years. One also wonders why Ramazani was not urged to provide case studies of gulf states other than Iraq and Saudi Arabia and why his punctilious quibble with Batatu over the precise founding date of the Islamic Call was not excised (compare p. 41, nn. 8-10 with pp. 191-92, nn. 30-36).

Other indications of editorial laxity are the excessive variation in the length of the articles and in the terminus ad quem for research in them. More serious, however, are the conflicts between authors. Cobban and Norton, for example, offer different translations for the military movement of which Amal is the acronym (pp. 144, 167) and vastly different figures of casualties resulting from the March 1978 Israeli invasion of Lebanon (pp. 146, 168). The editors and Batatu appear to ignore each other's explanation of the difference between the Akhbari and Usuli schools within Twelver Shi'ism (pp. 7-8, 187, n. 16) and Muriel Atkin pays no heed to Norton's insistence that Sayyid Musa al-Sadr was known as Imam (pp. 293, 160-61). Even the map placed at the head of the volume lacks utility, insofar as it fails to locate the many lesser-known places cited by the various authors and even some of the better-known places, such as Basra.

In sum, this is a collection of excellent articles more or less addressing a similar theme. Each is interesting and important in itself, but few come together to form a coherent whole.

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**Women and War.** By Jean Bethke Elshtain  
(New York: Basic Books, 1987. xvi, 288p.  
\$19.95).

*Women and War* is a work that deconstructs influential narratives about war in the Western tradition in an effort to undermine falsities in their teaching—especially false dichotomies

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that limit our sense of political choice and thus by implication teach the inevitability of war. Elshtain examines works as divergent as Greek tragedy, Kantian philosophy, and popular movies from *Shane* to *Rambo*, and connects their messages to the readiness—even eagerness—for war carried in the currently dominant theory of realism, which she strongly critiques.

Elshtain divides her book into two parts. In part 1, "Armed Civic Virtue," she examines the multiplicity of images of the armed warrior as an exemplary figure in works of political theory from Homer, Plato, and Aristotle to Lenin and also examines various "just-war" theories, arguing that they are often a prop for, rather than a brake on, "armed civic virtue."

In part 2, "Life Givers/Life Takers: History's Gender Gap," Elshtain sets out to lay to rest the message of one of the "dominant discourses" about war: that women are peace loving and life preserving, while men are violent and bellicose. That discourse has perpetuated a variety of false conceptions about men and women—their natures, social roles, and political voices—that have served to reinforce realist justifications of war. Men have been taught by war narratives from the Greeks to the present to understand virtue as readiness to fight for one's nation, to accept the bearing of arms as the highest act of citizenship. The same narrative tradition teaches women that their virtue is to decry violence but nonetheless to praise male valor and courage, to support war with home-front duties, to comfort the wounded, to mourn the dead.

Mounting a series of counterexamples, a parade of bloodthirsty women and peace-loving men, Elshtain urges the separation of fact from fiction. Most importantly, she insists, we must distinguish the real from the dogma of realism that depicts war as inevitable, even "natural," when in fact it stems from the decisions of elites whose own thinking may be beclouded by false images and fantasy.

Her deconstruction of the war-peace and male-female dichotomies leads Elshtain to argue that those who oppose the destructiveness and inhumanity of war should stop proposing "peace" as the alternative to "war." The image of peace in most narratives is bucolic, devoid of conflict, apolitical, and ultimately

false in its promise of harmony. By contrast, drawing on the work of Hannah Arendt, Elshtain seeks "wartime enthusiasms without war" and new narrative structures in which collective violence is not counterposed to "peace" but to "politics" (pp. 231, 237). Specifically, she proposes a politics of "chastened patriotism," whose practitioners would be urged to accept differences, understand limits, practice restraint, and know when to forgive: the tenets of a growing body of feminist theory about war.

One problem with the book is that while it examines the dynamics by which false conceptions of male and female natures encourage the recourse to war, Elshtain does not develop a larger causal theory. Why have our dominant narratives made it seem that war is inevitable? What, if any, is the relationship between patriarchy and war? Elshtain does not directly address these questions. Nonetheless, she demonstrates the power of narrative to shape political thinking and advances a compelling argument: questions about gender must be brought to the study of international relations and great-power politics to free the discipline from its conventional (and false) realist paradigms.

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### **A History and Theory of Informed Consent.**

By Ruth R. Faden and Tom L. Beauchamp  
(with Nancy M. P. King) (New York:  
Oxford University Press, 1986. xv, 392p.  
\$29.95).

To consent is to agree, to be of the same mind. Dictionaries, always misleadingly clear about what words mean, elaborate *consent* as the voluntary acceptance or allowance of what is done or planned by another. All lexical accounts classify *consent* as an act of reason or deliberation.

Faden and Beauchamp explore the history and theory of informed consent in a variety of social practices, with special emphasis on bioethical issues. The book is structured along three lines. The first is an examination of those moral and legal concepts that illuminate informed consent. The second is a delineation of a history of informed consent in selected areas

of biomedical traditions. The third consists of the authors' theory of informed consent drawn from the lessons gleaned from philosophy, law, and history.

One of the better chapters in the book (written mainly by Nancy King) is a study of consent and the courts (chap. 4). We are provided with a helpful sketch of the mechanisms that historically introduced informed consent into medicine. The authors point out that medical malpractice is in general based on beneficence, which identifies the rules for proper medical care that protect the well-being of patients. But recent negligence cases do rest on a failure of informed consent, and are developed on the acceptance of patient autonomy. In a practice where physician and patient share the same goals, a legal concern for self-determination has "jolted" medical practitioners into accepting the rights of patients to accept or refuse therapy.

The (ironically) coercive introduction of informed consent from outside biomedical practices helps explain why research on human subjects is more nearly guided by informed consent than is clinical medicine. Put simply, the state has compelled research scientists (via regulation) to obtain informed consent from their subjects while merely urging physicians "to adopt a model of shared decisionmaking" (p. 224). But even the mandatory character of consent requirements in research is both recent (dating only to the mid-1970s) and a reaction to the well-known moral failures represented by Milgram's research at Willowbrook, Tuskegee, and the Jewish Chronic Disease Hospital. The authors remind us that biomedical practitioners have not taken the initiative in implementing informed consent.

In the context of this legal and political history the authors proceed to explain informed consent in terms of its embedded concepts. Autonomy is disentangled as a cluster of intentions, understandings, and freedom from controlling influences (p. 238). Each of these three components of autonomy is elaborated, with the usual distinctions recognized among coercive, manipulative, and persuasive influences. The authors admit that autonomy requires independence both from others and from oneself and that the latter is impossible to fix in the absence of a theory of the self.

The theory of informed consent developed in the book relies on a distinction between

informed consent as autonomous authorization (sense<sub>1</sub>) and legally or institutionally effective (valid) authorization from a patient or subject (sense<sub>2</sub>). The former more nearly expresses a moral sense of informed consent, while the latter is represented in conventional rules that regulate those who seek consent. The authors show how these two senses of informed consent are different and sometimes contrary interpretations and that they provide a more robust view of the concept than standard definitions based on disclosure, comprehension, voluntariness, and competence. On the basis of this richer understanding of consent and relying on historical materials, the authors urge a shift from disclosure to effective communication in the practice of informed consent.

The book is a helpful and often authoritative account of informed consent that moves comfortably between empirical and philosophical approaches. The empirical material is secondary, representing no field work. But it is useful to have political and legal policies and decisions collected in narrative form. It would be even more useful to have some recent data on clinical medicine to document accurately how consent is pursued by health professionals. Missing from the book is any extensive treatment of the hard and important cases of no-code and partial-code practices in medicine. Living wills receive no mention at all. The issues of consent emerging from such cases invite us to survey some of our deeper values on life and its qualities, and how life-and-death decisions are and ought to be made in liberal societies. One leaves the present work disappointed that the considerable skills possessed by Faden and Beauchamp were not brought to bear on consent in critical care medicine.

Also missing in the work is a sensitivity to alternative communities of discourse that might provide radically different accounts of informed consent. Religious communities, for example, map the embedded concepts in consent within ontologies different from, and hostile to, those informing medical communities. The present work touches on these issues with brief discussions of false beliefs (pp. 253-54) and the need for intrasubjective components in identifying materially relevant propositions in consent decisions (pp. 309-10). But the authors do not explore the intriguing possibilities that

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all or most of our criteria for informed consent, including that which specifies competence, are captives of competing discourses. The authors are content to elaborate informed consent within the conventions of medical and legal discourses.

One also wishes that more speculative philosophy, especially recent work on primitive terms in the discourses the authors do address, could have been introduced. The work of Parfit (and others) on individual identity might enlighten our understandings of the self assumed in theories of autonomy. Can serial selves give informed consent, for example? Does autonomy require nonreductionist selves?

The book is an important work within the traditional areas of empiricism and philosophy that increases our appetites for different and perhaps more fanciful explorations of informed consent.

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**The Spirit of Revolt: Anarchism and the Cult of Authority.** By Richard K. Fenn (Totowa, NJ: Rowman & Littlefield, 1986. vii, 179p. \$27.95).

The problem of modern society, argues Richard Fenn, is that it mistakes what passes for a rationalized social order as an indication of the secularization of society. This misperception is the outgrowth of the priestly paradigm of secularization, which locates the voice of reason in a priestly class. In the modern world the priestly class consists of state and institutional authorities who seek to make institutions, especially the modern state, immune from popular and spontaneous action (p. 3). The result is that the state is identified as the ultimate seat of reason, which must control social conflict through ritualization to maintain order.

From the perspective of an alternative paradigm, what Fenn calls the prophetic paradigm, the modern state represents only a partial secularization of society. The representation of the state as the source of reason and necessary order results in fact in the "cult of authority" consisting of the self-identification of individuals with the state. It feeds on the fears, anxie-

ties, and helplessness that people experience in increasing, manufactured complexity (p. 90) of society. It further maintains itself through the ritualization of social conflict and participation. Ritualization, Fenn argues, displaces social conflict without really resolving it (pp. 143-49). The result is a lack of conclusive action, a "sham process of manufacture from which social facts never emerge complete and finished. . . . No decision is final and no grievance finally resolved" (p. 66). The problem is not only that participation is a fraud because it is ineffective. In addition, the "illusions of security" it promotes "can lead an entire community to the brink of self-destruction" (p. 128).

As an alternative to the pathology of the modern state, Fenn claims to offer an ideal that gives shape to "the possibilities for genuine freedom and unfeigned solidarity" (p. 19). Anarchism, the "emancipation of humankind from all forms of order except those arrived at by continuous and spontaneous acts of free consent, requires that the state be replaced with more flexible, simpler, and smaller forms of collective organization" (p. 89).

Fenn recognizes that the anarchist project involves various ambiguities. For example, it may require the emergence of heroic individuals. But the flip side of the anarchist hero can be contempt for the rest of humanity (pp. 150-53) that tempts the anarchist to authoritarian action and must be resisted. Nevertheless, the direction of the modern state, its failure to resolve social conflicts, and the disaffection that ritualized social action may create make anarchism the only alternative that guarantees free, uncoerced communities of autonomous social actors.

Fenn's argument speaks to many of the concerns of contemporary political theory. His claim that the connection between personal identity of citizens and the state as a cult of authority is a challenge to those political theorists who have attempted to outline the foundations of legitimacy of the state based on a connection to autonomous personal identity. Because it speaks to many issues in contemporary political theory, several questions remain (in addition to the traditional challenges of how anarchism is to be realized in practice). First, Fenn's discussion implies that in the modern world the overriding threat to autonomy is the state. But alternative diag-



noses of modernity locate the inefficacy of political action in a distorted relationship between the state and the economy that is part of an increasingly interdependent economic world. Such accounts are important for they raise the possibility that constraints on autonomy are not inherent in the modern state as Fenn would have it and that perhaps the modern state can be reconstructed as the embodiment of individual and communal autonomy. Second (and connected to the first point), though it may not be a "gross oversimplification to argue that complexity in social life is imposed by elites to perpetuate their domination" (p. 90), it is an oversimplification. Whether a public sphere of small communities characterized by what Fenn calls *limited obligation and spontaneous political action* would be capable of coping with that complexity is problematic. Third, one wonders if Fenn's impatience with "manufactured" social facts and inconclusive political action is a protest against the social construction of reality and the ambiguity that many theorists argue is inherent in modern politics and a precondition for freedom and autonomy in complex societies.

Fenn's book serves as a reminder of the potential dangers of the connections between personal identity and the modern state. And in considering the anarchist ideal it may be important to keep in mind Kropotkin's claim that "mental timidity in constructing an ideal is certainly a criterion of mental timidity in practice" (quoted p. 154). But it is also important to keep in mind that political ideals that are unsustainable or unrealizable can issue in social despair and political cynicism that are the resources of authoritarian political arrangements.

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**Private Vices, Public Benefits: Bernard Mandeville's Social and Political Thought.** By M. M. Goldsmith (Cambridge: Cambridge University Press, 1985. ix, 183p. \$29.95).

Three of the six chapters in *Private Vices, Public Benefits* have been published already—as two somewhat shorter articles—in easily accessible journals. Those earlier articles were

significant contributions to a revival of scholarly interest in Mandeville that began in the mid-1970s and has resulted in other book-length studies by Paulette Carrire, Richard Cook, Thomas Horne, and Hector Monro. There are two principal novelties in this volume. The first (in chap. 3) is an account of the *Fable of the Bees* showing that Mandeville understood the development of social institutions as the unintended consequence of human interdependence. The other (in chap. 4) is a detailed presentation of Mandeville's political views as those of a lifelong, if often "independent," Whig.

Goldsmith's chief objective in this book is "to show what Mandeville was doing in writing what he did" (p. ix). Yet readers interested in developing a better historical understanding of Mandeville's cynical, often bawdy humor will be disappointed. Indeed, that is a well-known aspect of his work Goldsmith is determined to play down. What he offers instead is a portrayal of Mandeville as a theorist whose thoroughgoing affirmation of human appetites and passions is best understood as a criticism and rejection of an ideology of "virtue" prevalent in Britain during the half century following the Glorious Revolution. According to Goldsmith, that ideology was a fusion of Christian and civic republican ideals of virtue that led to the formation of societies designed to promote Christianity and reform manners and (for Mandeville) also found its chief expression in Richard Steele's *The Tatler*. In this setting, it is clear why Mandeville's views were so scandalous for some of his contemporaries. For in defending natural human inclinations to pursue private goods and pleasures, Mandeville sought to justify what many took to be nothing more than "vice." Mandeville was also troubling because (unlike Hobbes) he acknowledged that individuals enjoyed the company of their fellows but not for reasons proponents of "virtue" could accept. Mandeville stressed that man's sociability was rooted in self-love, not natural benevolence. Men live in society because of their vanity, because they seek the flattery and admiration of others, and because they believe others are possibly aids to satisfying their material desires. Indeed, the main source of social interdependence is pride, what Mandeville often called *self-liking*. By nature men are indolent, yet they overcome their instinctive

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indolence because they are also self-seeking. To pursue "virtue" in the face of these obvious facts, Mandeville argued, would be ruinous to the progress of society. A few individuals may come to find virtue as a result of extraordinary self-restraint or self-denial; but "virtue" here is attained at the cost of other benefits, such as sociability or the common pursuit of wealth. Mandeville thought that the vast majority of men would not pay such a cost. Hence, our "public benefits" must be rooted in our "private vices," and those who would impose "virtue" on man's private passions propound dangerous nonsense.

Despite some repetitiveness in his analysis, Goldsmith's chief contribution to a fuller understanding of these views is to show how they took shape as a direct response to an ideology of virtue that informed much of British opinion after 1688. But there are problems with this approach and in Goldsmith's attempt (in chap. 5) to link Mandeville's views with "the spirit of capitalism." The main problem with Goldsmith's approach is that it still leaves one wondering how the *Fable of the Bees* ever achieved such a great *succès de scandale*. It may be true that Mandeville began his thinking within an established British ideology of "virtue," but it simply does not follow that an attempt to measure the power and virtuosity of his thinking should remain largely confined within that ideology. Such an approach leads to a neglect of other motives in Mandeville's thinking and sheds little light on the question of why Mandeville was to be both the foil and the inspiration of subsequent eighteenth century thinkers (especially David Hume and Adam Smith) whose work was at best marginally related to the concerns of the proponents of Christian and civic virtue.

Tying Mandeville to the "spirit of capitalism" is certainly one way of addressing these issues. But Mandeville's endorsement of money-making (pp. 134-37) does not make him an advocate of "capitalism." It is now widely accepted that in their understanding of eighteenth century "commerce" mercantilist thinkers shared many important assumptions in common with their supposed "free-trade" rivals. Chief among them was an analysis of human psychology that remarked on the social utility of ambition and the unintended public good that would spring under certain circumstances from ambition. Mercantilist and free-

trade thinkers alike, then, agreed that it was unnecessary to sacrifice individual self-interest to the larger demands of "virtue." The issue that decisively divided them was the mercantilist contention that enlightened political authority could ensure that the widespread pursuit of self-interest resulted in the good of the nation as a whole. In the eighteenth century debate about the proper role of government in commercial societies, there can be little question that Mandeville stood with the mercantilists. Hence, his role in promoting the "spirit of capitalism" ultimately requires a more complex analysis than one will find in this book.

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**Marxism and Totality: The Adventures of a Concept from Lukács to Habermas.** By Martin Jay (Berkeley and Los Angeles: University of California Press, 1984. xi, 576p. \$29.50 cloth; \$14.95 paper).

Both politically and philosophically Marxism is not a very popular theme in Western societies today. There are many reasons for this situation, some having to do with the emergence of novel or post-Marxist perspectives, but the neglect carries a price. Martin Jay's study reminds us of the rich intellectual resources stored up in the Marxist tradition. Unlike Kolakowski, Jay does not presume to offer a survey of all the "main currents" of this tradition, nor does he approach his topic from a position of radical disaffection. Instead, his book offers a sympathetic, but non-sectarian treatment of one major strand in this legacy, namely, "Western Marxism"—a strand strongly indebted to the "philosophical critique that began with Kant and German Idealism" and which, on the whole, was resolutely opposed "not only to the fatalistic economism of the Second International, but also to the voluntarist vanguardism of the Third" (p. 2). In seeking to distill the central concerns of this strand, the book does not present a panoramic survey, but rather relies on one central category—the concept of totality—as a guiding "compass to help us traverse the vast and uncharted intellectual territory that is Western Marxism." Although acknowledging the array

of other key concepts (like praxis, value, or dialectics), Jay argues that "the issue of totality has been at the center of the Marxist, or at least Western Marxist, debate as it has not been with bourgeois thought" and that "by spelling out the various meanings of totality and investigating their implications for other aspects of Marxist theory, we can fruitfully make sense of the tradition in new and revealing ways" (p. 14).

The study is impressive both in its meticulous scholarship and its philosophical subtlety and acumen. Together with his numerous other publications, *Marxism and Totality* establishes Jay as an intellectual historian and interpreter of the first rank. To be sure, intellectual historiography is always fraught with the tension between descriptive narrative and engaged philosophical argument. As Jay's work shows, however, the tension need not be debilitating: his new study succeeds remarkably well on both levels. In the case of his chosen topic—Marxism—the temptation to reduce philosophical ideas to their genesis or social-economic context has traditionally been very powerful, but Jay does not succumb to it. In terms of methodology, he shows himself an astute student of hermeneutics and post-hermeneutics from Gadamer and Ricoeur to Derrida—both with regard to the creative role of the interpreter and the "intertextual" relation of text and context. Without converting Marxist texts into abstract thought systems, he writes, "I nonetheless want to respect the arguments they contain rather than treat them as mere symptoms of external circumstances. Insofar as intellectual historians have come to recognize contexts as themselves texts requiring hermeneutic decipherment, we can no longer expect answers by referring to an unproblematic social reality fully external to the works we examine" (p. 17). Following Timothy Bahti, he calls his approach *Nachkonstruktion* rather than simple *Rekonstruktion*: "Instead of being a neutral recording of history 'as it actually happened' . . . it will be a rhetorical reenactment shaped by my own concerns and experiences."

The historical terrain traversed by the study is vast and complex. I can retrace here only the barest outline. Viewed from the perspective of the concept of totality, the story is basically one of rise and decline—followed by a more moderate resurgence in recent times. After a

brief sketch of the "topography" of Western Marxism, the study opens with a survey of the "discourse of totality" in Western philosophy before and after Marx—a survey stretching from Spinoza, Vico and the Enlightenment over Rousseau and German Idealism to Marx and the diverse trends of "bourgeois" thought in the last century (including Comte, Dilthey, and Weber). A tour de force, the chapter by itself warrants acquisition of the book. The story of Western Marxism begins with the formulation of a strong version of "totality" by three thinkers variously influenced by Hegel: Lukács, Korsch, and Gramsci. After a detour through Bloch's utopian naturalism, Jay portrays the progressive disintegration of Hegelian or "expressive holism" in three main settings or contexts: the German, French, and Italian. In the German case, the journey leads from Horkheimer's slow retreat from revolutionary élan over Marcuse's "anamnestic" holism to Adorno's post-Hegelian concern with non-identity and negative dialectics. In the French context, the study moves from the Hegelianism of Lefebvre and Goldmann over Sartre's existentialist Marxism and the ambiguous holism of Merleau-Ponty to the radical anti-Hegelian posture of Althusser's structuralism. The Italian study proceeds from Gramsci to the quasi-positivist scientism of Della Volpe and Colletti. As a counterpoise to the disintegration of the Hegelian legacy the study finally examines the linguistic or communicative "reconstruction" of Marxist holism by Habermas. An epilogue discusses the challenge hurled at any type of philosophical totality or totalization by recent post-structuralism and deconstructive analysis.

*Marxism and Totality* is too rich to permit a detailed acknowledgement of its strengths or virtues. Gramsci students may find intriguing the distinction between two holism's in his work: an idealist, Crocean and a non-idealist, Vichian type. Equally stimulating and insightful are the comments on Surrealism (as a backdrop to Lefebvre) and on Marxist aesthetics (in the Goldmann chapter). Its merits overshadow the study's quandaries and unresolved issues, of which I can mention only a few. Given the pivotal role of the Frankfurt School in the story of Western Marxism, close attention must be paid to accents set in the presentation of its key figures. On the whole, the older generation of critical theory tends to be

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assessed from a Habermasian perspective, for which Jay repeatedly voices sympathy. Despite a perceptive treatment of non-identity, the study finds that "Adorno's work inevitably led into a kind of cul-de-sac." This failure is traced basically to the neglect of communication, the "hostility to the dialogic, communicative function of language" (pp. 272, 274): "The possibility of a non-hypostatized intersubjectivity, a public realm in which non-coercive, rational discourse might take place among equals, was absent from his thinking, as it was in Critical Theory as a whole until Habermas." The chapter devoted to Habermas accordingly pays tribute to the "linguistic turn" in the latter's work and his stress on communicative rationality. In light of this evaluative slant, the reader is liable to be perplexed by the conclusion of the same chapter which raises doubts about Habermas's view of language (p. 508): "What if language is conceived as having more than one telos, other than the goal of achieving undistorted speech? What if the metaphoric, polysemic, playful capacity of language is highlighted, rather than its discursive function? . . . What if language's deep structure, which Habermas has so far neglected to study, can be understood to work against or even undermine the pragmatic utterances that strive for perfect communicability?"

As Jay realizes, questions of this kind have been forcefully articulated by spokesmen of contemporary post-structuralism, including Derrida, Foucault, Lacan, and Deleuze. The Epilogue shows that the "challenge of post-structuralism" extends not only to the telos of "communicability" but to all forms of holism, Hegelian or otherwise, and frequently to Marxism as such. Moreover, the intellectual challenge is reinforced in our time by real-life experiences, including the disclosures about the Gulag. While suggestive, Jay's concluding remarks on these issues are somewhat pale. If the threat of totalitarianism as well as the "negative totality of nuclear catastrophe" are to be avoided, he writes, "we may well need to find some positive alternative" (p. 537). Curiously, a phrase from Adorno serves to highlight this alternative: "What can oppose the decline of the West is not a resurrected culture, but the utopia that is silently contained in the image of its decline." As it seems to me, unearthing this utopia requires today a post-Hegelian, but by no means anti-Hegelian

reflection which resists positive totalization without lapsing into random particularism. Glimpses of this reflection can be found not only in post-structuralism but also in various post-metaphysical arguments from Levinas to Heidegger and Gadamer. In this light, much if not everything depends on the confrontation and reconciliation of modern rationalist holism and post-metaphysics—a point on which Jay seems to agree: "On that confrontation, I would contend in conclusion, rests the most likely possibility of a viable reconstitution of the Western Marxist tradition and, in particular, its much beleaguered concept of totality" (p. 509).

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**The Rhetoric of "Leviathan": Thomas Hobbes and the Politics of Cultural Transformation.** By David Johnston (Princeton: Princeton University Press, 1986. xx, 2334p. \$25.00).

**Hobbes and the Social Contract Tradition.** By Jean Hampton (Cambridge: Cambridge University Press, 1986. xii, 299p. \$42.50).

David Johnston's marvelously clear and lucid study examines the political teachings of *Leviathan* against the background of Hobbes's early writings, especially the translation of Thucydides (1628) and the *Elements of Law* (1640). Unlike conventional treatments that posit their own versions of an "epistemological break" between Hobbes's early humanistic and later scientific works, Johnston proposes a more complicated genealogy. From Thucydides and the ancients, Hobbes acquired a keen sense of the weakness of reason and logical demonstration as a means of political reform. While the axiomatic methods of philosophy (or science) had greater explanatory power, the more visual imagery of history and political oratory had greater practical efficacy. Political history would seem, then, to be the privileged mode of discourse for writers with a practical intent.

Johnston argues that Hobbes did not so much change his mind as shift his allegiance in the *Elements*, a sort of draft copy of *Leviathan*. Here Hobbes argued that rhetoric is the most dangerous enemy of reason and clarity. By

appealing to the passions, rhetoric deluded men into ignoring what had become for him the one axiomatic premise of human nature, namely that death is the greatest evil, and the policy of rational egoism that followed from this. But it was only in *Leviathan*, Hobbes's masterpiece of political philosophy, that he sought to synthesize the methods of the new science with the lessons of the older rhetorical and humanistic tradition. In *Leviathan* Hobbes developed a new kind of political rhetoric, one suited to turning the abstract deductive model of man sketched in the *Elements* into a practical program of political action. While Johnston remarks that "it would be wrong to suggest that *Leviathan* is not a work of science or philosophy," he goes on to suggest that Hobbes reversed his earlier priorities and allowed his political ambitions to dominate. "In writing this work," Johnston observes, "Hobbes was, above all else, performing a political act, not a scientific one" (pp. 67, 70, 91).

The largest portion of the book (chaps. 4-8) is devoted to an exposition of Hobbes's often neglected critique of theology in Parts 3 and 4 of *Leviathan*. Here is Johnston's most significant innovation. In the past, Hobbes's concern with religion has been treated as a residual legacy of certain seventeenth century theological controversies with no essential connection to the formal structure of his philosophy. Recently interpreters like Warrender and Hood have argued that Hobbes's politics are fully compatible with the Thomistic tradition of natural law. Johnston, however, sees Hobbes's treatment of religion more as a form of ideology critique necessary to rid the mind of its magical and superstitious elements and replace them with more "enlightened" views.

The dominance of the religious motif in Johnston's interpretation serves two functions. First, it allows us to see that reason and the politics of self-interest for which Hobbes is justly famous is not a "natural" propensity of man but an acquired skill. Man as he is is not a rational calculator but a being dominated by irrational beliefs in magic, prophecy, and religious superstitions. Thus men must be taught to be rational, to learn to fear death, if political society in the future is to be stable and peaceful. Second, because religious fears remain the single greatest obstacle to the realization of Hobbes's political project, Johnston

is enabled to present him more as the embattled *philosophe*, the propagandist of Enlightenment, than as a detached scientific observer. What he calls Hobbes's "politics of cultural transformation" is aimed not just at demonstrating the limits of sovereignty and the duties of subjects but at rooting out various kinds of metaphysical fallacies and false beliefs that make the new order impossible.

Jean Hampton offers a radically different Hobbes. Her goal is not the historical elucidation of Hobbes's context and development but a "rational reconstruction" of his social contract theory informed throughout by the use of "contemporary philosophical and mathematical tools" especially game theory (p. 3). Unlike Johnston who sees Hobbes's text as in large part an exercise in political rhetoric, Hampton regards it as a work of systematic theory where arguments for absolute sovereignty follow deductively from premises regarding human nature, psychology, and moral theory.

According to Hampton, the core of Hobbes's political theory is his "alienation" model of the social contract where people simply give up or alienate their power to a political ruler in the hope that doing so will satisfy certain important human needs. Because the alienation must be total, it follows that subjects retain no rights against the sovereign should he fail to satisfy them. Hampton believes that Hobbes's alienation model fails because it requires us to alienate the very rationality that we need to bring the state into being. A more satisfactory model of the social contract is attributed to Locke who regarded the granting of power less as a surrender than as a loan. According to the "agency" model, which became central to later liberal notions about constitutional government, the relationship between ruler and subject is not that of master to slave but of agent to principal where the ruler or agent is still ultimately dependent upon the will of the principal (pp. 122-23, 256-84).

Obviously much depends on how one understands the term alienation. In both the *Elements of Law* and *De Cive* Hobbes treated the creation of sovereign power as requiring the complete renunciation of rights by the subjects. However, in *Leviathan*, Hobbes used the term "authorization" to describe this transfer of power that implies actively appointing someone to be one's agent, not just passively surrendering one's rights. To say, then, that

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the sovereign's power is authorized by the subjects is, at least potentially, to limit that power by the consent of the governed.

Now here is a problem. For Hampton to save her thesis she has to argue that authorization in *Leviathan* is compatible with, even implied by, the full surrender argument developed in Hobbes's early works. Against revisionist scholars like David Gautier, she maintains that in the seventeenth century even proponents of parliamentary government, like Phillip Hunton, understood authorization as implying the total alienation of one's rights to the sovereign. What Hobbes, Hunton, and later Kant were all trying, unsuccessfully, to come to terms with was the "paradox of being governed"—how people can be both the creators of the state and its subjects at the same time—which Hampton seeks to "dissolve" in her final chapter (pp. 279–84).

The question that Hampton finds baffling is why Hobbes remained wedded to the social contract methodology when its "agency implications" clearly violated the absolutist politics he tried to defend. Johnston's answer here is, I think, the more plausible. He suggests that for Hobbes sovereign authorization was a way of meeting the nascent democratic demand that the rights of the sovereign be recognized as legitimate by the people (p. 83). Hampton, by contrast, is forced to present Hobbes as a deeply incoherent thinker who would have done better adopting the divine politics of Robert Filmer (or Plato in the *Crito*) to defend absolutist government (pp. 264–65, 267).

It is tempting to play these books off against one another. Because I find Hobbes more of a political thinker than a game theorist, I find Johnston's analysis more satisfying. I agree with Hampton that the "tools" of game theory and modern analytical techniques may be useful in helping to elucidate what a thinker had to say but more often they simply get in the way, preselecting the questions deemed to be important as well as the answers the thinker should have supplied. Thus, at the very outset of her book we read: "Of course, in one sense, most of us in the twentieth century are already confident that [Hobbes's] argument fails in some way, for we believe that there is no successful argument for a polity as distasteful to us as absolute sovereignty" (p. 3). But if it is already predetermined that his argument "fails" just because "most of us" find it "dis-

tasteful" then this is more of an obstacle to understanding than an aid. The result is a work that reconfirms contemporary prejudices about Hobbes but does not get to the core of his teaching.

The fact is that whatever the logical structure of Hobbes's argument, we live in a world that has been decisively shaped by his perspective. Unlike the ancients, Hobbes along with Bacon, Descartes, and Spinoza regarded his philosophy as part of a "project" that should be judged by its "fruits," mainly the conquest of nature and the relief of humankind's estate. The purpose of this project was to educate society regarding the potential benefits of the new science that would be put toward securing the enlightened ends of "peaceable, sociable, and comfortable living" (*Leviathan*, chap. 15). That so many today can even think like game theorists, can pursue strategies of rational egoism, and assume such behavior to be "natural" is a stunning testimony to Hobbes's success. One can only wonder whether the consequences of his success have been altogether benign.

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**Arguing for Socialism: Theoretical Considerations.** By Andrew Levine (Boston: Routledge & Kegan Paul, 1984. xiv, 241p. \$23.50).

Academic philosophers of the analytic persuasion have already transformed the modern liberal-democratic tradition from a lively, largely ideological, real world debate into a set of abstract postulates and propositions. Philosophy now seems bent on performing a similar alchemical transformation of socialist theory, judging from Andrew Levine's book. Levine has two distinct, although related, aims: first, to demonstrate the theoretical superiority of socialism over capitalism in terms of the "dominant tradition" of liberal-democratic values; and second, to affirm the "rational kernel" of a specifically Marxian version of historical change towards socialism. My objections to *Arguing for Socialism* do not stem from what may be regarded as Levine's ultimate conclusion: that political will is the essential ingredient in the translation of democratic socialist

theory into practice—and that victory over capitalism and state socialism is by no means assured. But Levine's book unintentionally reveals that analytic philosophy—although not philosophy in general, I think—is of negligible value in accomplishing this change.

Levine's difficulties begin with his introduction, where he produces excessively economistic "stipulative definitions" of capitalism and socialism (pp. 5-10). His rationale for the definitions he offers is typical of the ordinary language analyst; they are meant to cover all existing cases of capitalism and socialism, as well as feasible alternative models. Thus, capitalism is necessarily and sufficiently determined by the absence of market-related political unfreedom and the presence of private ownership of the means of production. Analogously, socialism is understood solely in terms of deprivatized, public ownership of productive means. Apparently neither economic system implies any characteristic constituent political structure whatsoever. In Levine's view, the bare economic similarities which seem to be shared by nations that claim to be either capitalist or socialist are sufficient to distinguish capitalism from socialism.

Why we should be compelled to accept these definitions on the basis of self-ascription is unfathomable. Levine refuses to apply a critical and discerning eye to the actual conditions of contemporary societies. Ideological distortion is not an eventuality with which Levine's philosophical method prepares him to cope. Consequently, his conceptualization of socialism and capitalism is logically rigorous but historically naive. More significantly, Levine champions a deceptive separation of the economic and the political. Neither capitalism nor socialism is compatible with just *any* political structures. Rather, the capitalist state, regardless of how constituted, must perform certain functions of a juridical-coercive nature inseparable from the "economic" arrangements of capitalism. Similarly, the realization of socialism arguably requires a political apparatus for worker self-administration and self-control; any system which does not allow for these activities cannot be reconciled with public ownership.

Another central problem of *Arguing for Socialism* is the inconclusiveness of its comparisons of capitalism and socialism. In part, this is because Levine invokes as his theoretical

yardstick "the dominant tradition" of *bourgeois* philosophy, which he treats as an independent and intractable system of values. But are capitalism and socialism even theoretically comparable? Socialism (or, at least, the democratic variety he desires) necessarily requires a future-directed theory in order to bridge the gulf to practice. But because capitalism is already well entrenched, its theoretical constructs perform the conservative function of abstract legitimization. Insofar as socialist and capitalist theories have such different orientations, any genuinely "objective" comparative evaluation must await the unknown outcome of historical development.

*Arguing for Socialism* is indeed an apt title. For Levine cannot see his way clear to do anything to imbue his arguments with practical significance. Formulating alternative strategies and groundwork for the transformation of society is the task demanded of socialist philosophy today. The ramifications of such historical change are far too important to be left to a Pascalian wager.

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**Marx, Nietzsche, and Modernity.** By Nancy S. Love (New York: Columbia University Press, 1986. xii, 264p. \$27.50).

Nancy Love's comparison of the visions of Marx and Nietzsche is a self-proclaimed attempt (1) to illuminate "the problem of modernity," (2) to establish the limitations of theoretical syntheses of these two intellectual titans, and (3) to contribute to the debate over the relationship between Nietzsche's philosophical and political thought currently being carried on in the work of Robert Eden, Mark Warren, Richard Schacht, Ofelia Schutte and others (pp. 8-11, 207). The second of these goals clearly dominates her study. Love's effort to achieve this goal, however, is characterized by a propensity for abstraction. She has, for instance, laced her study with an abundance of fairly long and intriguing quotes that indicate a more than passing familiarity with the writings of Marx and Nietzsche. Nonetheless, it is precisely this overreliance on quotations, many of which are left to stand by themselves, that is the most obvious sign of Love's afflic-

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tion with a major "problem" of modernity to which we are all susceptible—what Weber called intellectualization.

Evidence of this problem may be seen in Love's use of Weber's notion of ideal-type, which is, oddly, not explicitly introduced until the closing pages of her study, to explain why she avoids addressing specific syntheses of Marx and Nietzsche. "I have not criticized any specific attempt to combine Marx and Nietzsche," Love concludes, "but rather have tried to demonstrate the contradictions between their perspectives" (p. 201). What are these contradictions? According to Love, there are three fundamental differences that her "ideal-type synthesizer" (or "abstraction") ignores between Marxian science and Nietzschean skepticism (chap. 4), Marx's socialist and Nietzsche's individualistic critiques of liberal democracy (chap. 5), and opposing visions of liberation through production and play (chap. 6). And why does this "synthesizer" overlook such contradictions? According to Love, Weber attempts synthesis because both Marx and Nietzsche criticized German idealism and analyzed how humanity creates itself historically (chap. 2). Both also had similar theories of societal development that rejected strictly teleological and causal explanations in favor of functional analyses of the growth of human powers over nature and society (chap. 3). Nonetheless, attempts to combine Marx and Nietzsche, Love reminds us more than once, result in "schizophrenia, not synthesis" (pp. 17, 200).

Love's refusal to break free of the logic of her schizoid synthesizer prevents her from addressing this very important topic in a more sensible manner and strengthening her case against theoretical synthesis. Had she probed more deeply a few of her numerous citations from Marx and Nietzsche, she might have discovered that her three contradictions are not as neat as they at first appear. Barring her own unpacking, Love might have raised significant questions about the oppositions of science and skepticism by directly addressing the work of Hans Barth (p. 12); socialism and individualism through the writing of Louis Dumont; and production and play by reviewing the thought of Herbert Marcuse (p. 235). But the abstract reason of the schizoid synthesizer subsumes all particular theoretical critics.

Love's failure to counter criticisms of her

analysis leaves her defenseless before the similarities encouraging synthesis that she abumbrates in chapters 2-3; for Marx's and Nietzsche's revolutionary theories of "man's self-creation" (p. 31) have armed us with weapons for transforming the world, society, and the self that have striking affinities. According to both visions, a good theory confers power because it creates options. The radical opening of possibilities encouraged by such theories may well be the key to what Love calls "the problem of modernity."

ALAN WOOLFOLK

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**Civil Religion and Political Theology.** Edited by Leroy S. Rouner (South Bend, IN: University of Notre Dame Press, 1986. xv, 228p. \$24.95).

In *The City of God* Augustine struggles with a question which perplexed even the earliest Christians: What does the practice of religion have to do with the powers of the earth? *Civil Religion and Political Theology* treats this problem by focusing on the fragile relationship of political theology and public philosophy, and by using the United States as a case study for clarifying the pitfalls and possibilities of this relationship.

The names of many of the contributors to this volume (vol. 7 in the Boston University Studies in Philosophy and Religion series) will be familiar even to those whose knowledge of the subject is cursory—Jurgen Moltmann, Robert Bellah, Richard Neuhaus, John Wilson, Johann Metz, James Cone, John Cobb. Because the essays were originally lectures critiqued by a panel of scholars and revised for publication, they display an inner coherence uncharacteristic of collections of this sort.

The assumption shared by all the essays is that "religion must be relevant to political life without being co-opted by it," and the theme they all address is "the role Christianity does and should play in the life of the polis" (p. 2). This theme is interpreted from three angles that dictate the three major divisions of the book: the common intellectual foundations which make possible the practice of democratic values; the way civil religion based on these common values manifests itself, for bet-



ter or worse, in the American experiment; and several ways in which the values of political theology are practiced as modes of political liberation and economic justice.

Although these essays are excellent expositions, and indeed interesting for those not familiar with the terrain, they are for the most part all too familiar. There comes a time when repeating a conducted tour tends to weary the intellect rather than excite it. This is not to say that the authors are dull or incompetent. They are wholly competent as expositors. It is rather that these essays do not advance the discussion very far, especially those essays which focus on the nature and role of civil religion in America. One of several exceptions is John Cobb's contribution "Christianity, Political Theology, and the Economic Future." Cobb is bipartisan; on the basis of Christian beliefs and values, he assesses the strengths and weaknesses of capitalism and socialism, "calls for new theories" and suggests "some of the considerations important for their formation"

(p. 223).

Another matter detracts from the value of this volume. Political theology as liberation theology, the theme of the last section of the volume, not only excludes the Latin American perspective which has had an enormous impact on the formation of American liberation theology (e.g. the Roman Catholic Bishops), but also excludes Jewish liberation theology which in a fundamental theological and historical way (namely, the Exodus) is the paradigm for most political theology in the Western tradition. Except for James Cone who recognizes that black liberation is partly rooted in "exodus-liberation," this volume ignores (as is typical of volumes of this kind) the contribution of Jewish thought to the formation and practice of Christian political-liberation theology.

JAMES E. GILMAN

*Mary Baldwin College*

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## AMERICAN POLITICS

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**The False Promise of Administrative Reform:  
Implementing Quality Control in Welfare.**  
By Evelyn Brodtkin (Philadelphia: Temple  
University Press, 1986. x, 156p. \$29.95).

In the past two decades, welfare policy and administration has been at the center of much public controversy. Early demands that the "rights" of welfare claimants be respected were soon drowned out by criticisms of the "welfare mess"—claims of rampant fraud and abuse alongside rapidly escalating costs and administrative inefficiency. Despite widespread concern, Congress has been unable to enact welfare reforms, because it cannot resolve conflicts between social responsibility to the disadvantaged and concerns to limit fraud and to ensure work incentives for claimants. As Evelyn Brodtkin explains in her excellent book, however, congressional failure did not prevent the reform of welfare administration. Both the Nixon and Carter administrations adopted quality-control programs that, while osten-

sibly aimed at reducing error rates in welfare decisions, also caused substantial changes in the operation of welfare programs.

Brodtkin neatly combines very different theoretical perspectives in explaining welfare politics. She begins with the "governability crisis theory" of Samuel Huntington and others: neither the Congress nor the executive can resolve political conflicts over welfare, so coherent and responsive national policies cannot be formulated. She combines this pluralist analysis with the neo-Marxist analysis of Claus Offe that when political deadlocks occur, alternative forms of political resolution develop, such as administrative reform. Brodtkin describes the debate over welfare policy in the 1970s, showing how it was converted, particularly by the Nixon administration, into the "technical question" of quality control after direct efforts at reforms were frustrated. Welfare policy politics were made "more manageable by: (1) de-politicizing policy choices, (2) limiting participation in the policy-making

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process, (3) making policy without appearing to, and (4) legitimating administrative practices on the basis of widely held, nonpolitical values" (p. 22).

Brodkin then uses the ideas of street-level bureaucracy to explain how quality control affected the delivery of welfare. Concerns about welfare fraud and abuse motivated quality-control efforts, focusing it on procedural compliance and errors of liberality (overpayments and payments to ineligible claimants). This focus, backed by threats of financial sanctions by HEW, made the states concentrate on procedures and errors of liberality without recognizing the resulting increase in errors of stringency and new barriers that discouraged some eligible clients from applying. In a case study of Massachusetts, she explains how the threat of sanctions led the state to give district welfare offices strong incentives to reduce quality-control errors while maintaining or increasing productivity. Welfare offices in turn tied caseworkers' evaluations and promotions to measures of productivity and quality-control errors. The street-level bureaucracy perspective generally points to restrictions on managers' abilities to control case-level decisions, but Brodkin shows how the narrow focus of the quality-control effort altered decision making despite officials' considerable discretion. In another version of that familiar organizational story, the means—quality control—became the operative ends of administration.

I have only two quibbles with this book. First, Brodkin may have been too successful in presenting her arguments and data concisely. She writes lucidly and her analysis is rich, so that in some places I wish she had gone into more depth. Second, she is not wholly persuasive in her argument that officials adopted quality control *in order to* restrict access to welfare. She shows that the effect of quality control was to restrict access, but it is not sufficient to assume that policymakers intended those effects, particularly as she explains how the Carter administration adopted quality control to remove the taint of fraud and inefficiency that might provoke reductions in congressional and state support for welfare.

Brodkin has written a compelling study of the administrative policy process with important conclusions that extend beyond the confines of welfare policy. Studies of social serv-

ices and government administration generally would do well to incorporate her concern with the relations between policy-making and administration and her careful working-through of the politics and consequences of administrative reform.

JACK TWEEDIE

*State University of New York, Binghamton*

**Presidential Communication: Description and Analysis.** By Robert E. Denton, Jr. and Dan F. Hahn (New York: Praeger, 1986. xxiv, 332p. \$39.95 cloth; \$15.95 paper).

"The purpose of our study," Robert Denton and Dan Hahn note in the preface of this work, "is to investigate the institutional presidency from the traditional perspectives of communication scholarship" (p. xiv). One cannot but be impressed by the authors' thoroughness as they go about developing their thesis that "communication—its quantity and quality—defines the nature and essential characteristics of the U.S. presidency" (p. xii). In fact, one of the major strengths of this study is that Denton and Hahn have fashioned their work on a firm and solid foundation of scholarly research in the areas of communication theory, history, and political science. Without doubt, this work is one of the most thoroughly researched yet readable books on the presidency to be published in the last 25 years.

Denton and Hahn observe and investigate the presidency from interpersonal, intrapersonal, small group, and mass communication orientations. One chapter in the book is devoted to each of these perspectives. The overall quality of the study is enhanced in that the authors have included various readings or ministudies that illustrate and elaborate upon the points made and conclusions drawn in each chapter. These readings and ministudies, appropriately placed at the end of every chapter, are very instructive in that they link theory and political reality.

The chapters dealing with the intrapersonal and small group communication dimensions of the presidency are especially interesting and informative in that they address aspects that warrant more attention and study by today's scholars. Denton and Hahn do a superb job of dealing with these areas of presidential com-

munication and raise several intriguing points and questions for their readers to ponder. For example, they argue that "ironically, a strong president may be the least effective user of (small) group communication" (p. 239).

This work is unique in that "it is the *only* text that focuses on communication variables in relation to the presidency" (p. xv). There is no question that Denton and Hahn have made a significant contribution. Their work may soon join Richard Neustadt's classic *Presidential Power: The Politics of Leadership* as one of the most influential publications in the area of presidential studies.

C. DON LIVINGSTON

Western Carolina University

**Exploring Federalism.** By Daniel J. Elazar (Tuscaloosa: University of Alabama Press, 1987. xvi, 335p. \$28.95).

No contemporary scholar has observed as many federations at first hand or written so prolifically about federalism as has Daniel Elazar. Apart from his writings, his intellectual leadership is embodied in the work of the Center for the Study of Federalism, which is celebrating its twentieth anniversary at Temple University under his directorship and in his efforts as the founding editor of *Publius*, the quarterly journal of federalism. *Exploring Federalism* is the fruit of Elazar's mature experience and reflection and is a dauntingly comprehensive and erudite book.

Elazar makes bold claims for federalism in both its philosophical-theological and institutional dimensions. According to him, the roots of federalism are in the partnership between God and the Hebrew people as elaborated in the Pentateuch and the subsequent agreement among individuals and families to found polities based on consent. In its institutional manifestation, the characteristic federal relation is based on a "matrix," a fragmentation of power, rather than hierarchical power relations. Federalism not only embodies durable values of liberty and justice but accords closely with the findings of contemporary cybernetics, developmental psychology, and anthropology about human nature and human society. Thus federalism "embodies a world-view and an approach to the study of political phenomena

that come close to being comprehensive" (p. 29).

I do not find persuasive Elazar's attempt to rewrite the standard accounts of the history of political theory to elevate federalism to a first-order concept akin to natural law and natural right (p. 1). Apart from some attention to Althusius, he has little to say about major figures in the history of political thought. Proudhon, who is often regarded as a major federalist thinker, is relegated to a short paragraph. Significantly, Elazar finds "fragments" of federalism in Rousseau's *Social Contract*, although one might persuasively argue that of all the concepts of political thought the *general will* is the one most directly in contradiction to federalism. One contemporary thinker who *does* advance federalism as a first-order political idea is Pierre Trudeau in his association between federalism and reason and his sensitive analysis of the relation between federalism and rationalism (*Federalism and the French Canadians* [Toronto: Macmillan, 1968]). In general, G. F. Sawyer's analysis of the place of federalism in the conflict among political ideas is more accurate than Elazar's account: "People are not likely to go to the stake or the barricades to defend federalism as such. They may undertake heroic actions for the sake of some value which federalism happens at the moment to favour and may then even inscribe federalism on their banner—Liberty and federalism—Equality and federalism—but never just Federalism" (*Modern Federalism* [London: C. A. Watts, 1969], 186–87).

In his enthusiasm for federalism and his emphasis on its consensual aspects, Elazar refuses to face the extent to which federations have been maintained by physical violence and federal forms used to buttress illiberal institutions. After all, of the three most populous contemporary federations, the United States and Nigeria have been sustained by the bloody defeat of secession, and India has relied on the role of physical force in maintaining that nation's continuing existence. Federal jurisdictional guarantees have supported illiberal governments in, for example, Huey Long's Louisiana, Duplessis's Quebec and Bjelke-Petersen's Queensland. William Riker ended his 1964 book on federalism in eight nations with this statement: "If in the United States one disapproves of racism, one should disapprove of federalism" (*Federalism: Origin Operation,*

political happenings of the region have not been a major political science agenda item. Still, the politics of the area can be of interest to more than those political scientists interested in the collection of esoteric data for no other reason than that the federal system over-represents the Mountain West in the Senate by over 300%. One of every six senators is elected from the Mountain West.

*The Politics of Realignment* is clearly the best political science study of the area of which I am aware. The book, a collection of 10 essays, has as its focus the rise to dominance of the Republican party. For much of the twentieth century the Democratic party was highly successful in the Mountain West. As several of the book's authors point out, only the South was more favorable to the Democrats. Today, judging by a variety of measures, the Mountain West is by far the most Republican region in the nation.

The objective of this book is to describe the course and nature of the party realignment and to speculate about its causes. In addition, the main authors have done an excellent job of fitting the Mountain West case into the vast literature on party realignment. Basically, they conclude that the current realignment in the Mountain West is quite unlike previous party realignments. As they point out, the realignment has not been marked by a "political earthquake" such as the Great Depression. It has been a slow, fragmented movement in which the Republicans targeted Democratic Senate seats and concentrated on open House seats. The changing demographic composition and increased prosperity of the region; the increased policy polarization of the national parties; President Carter's attempt to reduce federal subsidies for western water projects; and the increased importance of television campaigning and changes in campaign finance made the drive to Republican ascendancy possible.

Although the various authors follow the book's theme very well, students of the region will find several of the chapters more useful and interesting than others. John Francis does an excellent job of providing a political overview. The chapters by Walter Stone, Arthur Miller, and Robert Benedict contain astute analysis and valuable data concerning changes in party identification and policy preferences. Gary Jacobson's examination of congressional

elections and Henry Kenski's discussion of political campaigns are both valuable pieces of political analysis.

*Political Realignment* is remarkably free of factual errors considering that many of the authors do not reside in, and have not done much previous work on, the Mountain West. One exception can be found in the chapter by James L. Gibson. Using data from an article by Wright, Erikson, and McIver (*Journal of Politics* 47 [1985]: 469-89), Gibson states that the Democratic party is the stronger party in Wyoming. This piece of information is radically at odds with repeated surveys of the Wyoming population, party registration and election results since statehood. Surely the editors should have informed Gibson of the obvious error.

One inherent shortcoming of a work of this sort is that state variations are deemphasized. For instance, the Mormon influence in Utah, Nevada, and Idaho is a major factor in the explanation of politics in these states. One cannot understand New Mexico politics without considerable reference to its Hispanic heritage, a heritage that is absent from a state such as Montana. It might even be argued that to aggregate the eight states is to obscure much significant political data.

In all, however, *Political Realignment* is a most useful and valuable study of western politics.

OLIVER WALTER

University of Wyoming

**Strangers or Friends: Principles for a New Alien Admission Policy.** By Mark Gibney (Westport, CT: Greenwood, 1986. xv, 169p. \$29.95).

This book attempts to develop the basis for a moral alien admissions policy. In part 1, Gibney briefly outlines the approaches of a number of political philosophers on issues ranging from the importance of community to human rights as they relate to the ethics of alien admissions. He then builds on these philosophical discussions to advance his own recommendations.

Gibney's argument is comprised of three fundamental ingredients. First, it takes as central the "Harm Principle" ("HP"); that is, that

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no individual (or, by extension, nation-state) should harm another individual and that if an individual (or nation) does harm another, it has a "special duty" to provide restitution to the victim. In addition, there is a less specific duty, according to Gibney's "Basic Rights Principle" ("BRP"), which mandates that even in the absence of culpability, we have a moral obligation "to honor the basic rights of subsistence and security that individuals possess" (p. 103). The third building block of Gibney's argument concerns the right of a community (generally synonymous here with *nation*) not to sacrifice "communal processes" in satisfying HP and BRP.

Taking these principles together and applying them to alien admissions, Gibney argues that the United States (or any country) should construct immigration-refugee policies that first favor those whom it has harmed via wars, foreign policy, and so on; that after this Harm Principle has been satisfied, it should offer admission to those whose basic rights are being violated, even if it has played no role in this violation; and, finally, that care should be taken in implementing HP and BRP (especially the less specific BRP), so that the receiving community is not disrupted. In order to minimize such disruption, Gibney suggests (1) that alien admission should only be considered after other types of aid have proven unsatisfactory; (2) that admissions should often be only temporary; and (3) that the Harm Principle and the Basic Rights Principle must not be interpreted too liberally, applying in most cases only when the victim is in danger of losing "life, limb, and vitality" (p. 142).

Perhaps the most consistent weakness of the book is the indiscriminate mixing together of philosophically based moral imperatives with descriptions of existing reality. For example, the primary defense of his recommendation that each developed nation give 1% of its GNP to alleviate suffering in other countries is that "many nations have already agreed to the 1 percent figure as a goal. . . . Perhaps 1 percent is not the 'right' figure, but it does not exceed what many nations think is the right answer." In light of Gibney's goal of building an ethics of aid based on moral principles, this defense of his ethics as no more than has already been done seems strikingly out of place.

Similar to such confusion between what "ought to be" and what "is" is a disconcerting

mix of naïveté and hard-nosed pragmatism throughout the book. This problem can best be explored through an analysis of Gibney's statement "this system is not meant as an 'ideal' theory" (p. 147). On the one hand, Gibney's approach is indeed far from "ideal" in that, having set the normative basis for a moral admissions policy, he retreats into communal self-interest and an almost callous practicality in setting the limits on these admissions. For example, in his discussion of Mexican migration (p. 113), Gibney argues that while many aspiring Mexican migrants may live in poverty, they do not live "on the edge of subsistence" (a questionable evaluation). Therefore, they should be restricted, particularly, Gibney argues, since the United States has done no harm to Mexico (again, this is questionable), and therefore only the less stringent principle of BRP applies.

On the other hand, Gibney's approach is decidedly "ideal" in glossing over the difficulties of getting governments to act with a moral conscience. Indeed, it is here that the intermingling of the ideal with the real is the most perplexing. Speaking of the need for governments to accept as refugees those whom it has harmed with its foreign policies, Gibney takes the immorality (or at least self-interest) of given countries' foreign policies as relatively immutable, while assuming that in their refugee policies they can be convinced to become moral actors.

Gibney's treatment is also "ideal" in that it includes no analysis of the role of the state in Western capitalist societies, accepting instead the myth that public interest dominates policy-making. In spite of some reference to U.S. society as "pluralistic" (p. 15) and "diverse" (p. 16), he usually implies that policy-making is a consequence of "national goals" (p. 146), or what he refers to as the *trustee* arrangement, in which governments pursue the self-interests of their own citizens. In fact, he takes national self-interest to be the unfortunate reality of state decision making, the counterpart to his moral duties and obligations approach. Not only does this consensus view of decision making lead to a misinterpretation of past immigration policies as it underplays the role of economics and the conflict of interests based on economic factors, but this "idealism" vis-à-vis the U.S. state is probably also responsible for Gibney's failure to address the very real diffi-

culties of implementing any of his recommendations.

KITTY CALAVITA

*University of California, San Diego*

**American Public Opinion toward Israel and the Arab-Israeli Conflict.** By Eytan Gilboa (Lexington, MA: D. C. Heath, 1987. 366p. \$34.00).

Gilboa attempts a comprehensive review of U.S. public attitudes, as reflected in the polls, concerning Israel and Israeli-related issues. His survey begins around the mid-1940s and ends in 1985. The discussion is first presented in a chronological manner; in a second part, the author deals with "issues and group attitudes."

The main value of the book is that it represents the largest body of data on the subject, as well as attempting to analyze the polls on "Israel and the Arab-Israeli conflict" on the basis of the respondents' socioeconomic, ethnic, and religious backgrounds. Also, a brief section deals with "opinion trends."

In the introduction the author states that "this book questions not only whether public opinion affects American policy on the Middle East but also whether it affects the policies of Israel and other Middle Eastern countries" (p. 3). Unfortunately, however, there is no systematic treatment of these issues other than quotes, assertions, or speculations. Gilboa himself appears to be uncertain as to whether opinion follows policy (pp. 199, 317) or whether, as he more frequently asserts, public opinion affects, if it does not determine, U.S. policy on the Middle East (pp. 60, 95, 96, 155, 198, 229, 308, 320, 321) *as well as* the foreign policies of Israel and the Arab countries, especially Egypt (pp. 59, 63, 67, 74, 115, 161, 164, 320). In this regard, the author fails to cite perhaps the only study that deals with this issue systematically and concludes that U.S. images of Israel and Egypt tend to follow U.S. official policy (S. Iyengar and M. Suleiman, "Trends in Public Support for Egypt and Israel, 1956-1978," *American Politics Quarterly* 8 [1980]: 34-60).

While Gilboa amasses a great deal of information and data on the subject of attitudes, the study is definitely marred by certain flaws. First, the author—a professor at Hebrew Uni-

versity and the National Defense College of the Israeli army, as well as a consultant to the Israeli ministries of Foreign Affairs and Defense—uses the lengthy sections devoted to "appropriate historical contexts and perspectives" (p. 2) in order to defend, justify, or otherwise sympathetically explain Israeli positions or actions. Furthermore, the terminology is precisely that used by the Israeli government and its supporters. Thus, the 1973 war is always mentioned as the *Yom Kippur War*, the PLO and PLO activities are repeatedly designated as *terrorists* and *terrorism*, the Israeli annexation of East Jerusalem is referred to as *unification* and its potential return to Arab control as *redivision*, and so on. Also, the 1967 Israeli attack on the *USS Liberty* is simply described as an accidental *mistake* (p. 45)—without any reference to the extensive published reports and allegations that it was deliberate. Finally, a more objective scholar might have difficulty making the unequivocal assertion, for instance, that "the War of Independence" and the 1967 Six-Day War were both "initiated by the Arabs" (p. 309).

A second flaw is Gilboa's treatment of the Palestinians merely as an "issue," similar to the treatment of "aid to Israel"; that is, they are not viewed as a people nor as a major party to the conflict. In fact almost invariably throughout the whole book, whenever the word *Palestinian* is mentioned it is as a "problem," an "issue," a "question," or in reference to "refugees" or "terrorists." While the Palestinians are not specifically and collectively called *terrorists*, the constant reference to "Palestinian terrorists" definitely contributes to that impression.

A third weakness is that while Gilboa's study appears to be comprehensive and while it includes a large number of polls commissioned by the American Jewish Committee, there is no mention of the 1982 poll commissioned by the Institute of Arab Studies and carried out by Decision/ Making/ Information, which provided questions with Arab, especially Palestinian, perspectives and concerns (*Arab Studies Quarterly* 4 [1982]: 358-74).

In the end, the author's concern to present Israel and Israeli activities in the best light possible gets in the way of an objective interpretation of the data. That there is strong support for Israel and sympathy for the Israelis among the U.S. public is clear and not in dis-

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pute. However, Gilboa goes beyond this and appears to be anxious to see to it that U.S. opinion of Israel be maintained at a high level, and he attempts to show that it is even stronger than it actually is. This is a major shortcoming and greatly detracts from the value of the book.

MICHAEL W. SULEIMAN

*Kansas State University*

**Policy Design and the Politics of Implementation: The Case of Health Care in the American States.** By Malcolm L. Goggin (Knoxville: University of Tennessee Press, 1987. xv, 296p. \$34.50).

Malcolm Goggin has contributed to the transformation of *policy implementation* from a dirty word into a maturing—if not yet mature—subfield of political science. His book fits comfortably among a number of “second generation” studies that utilize comparative and longitudinal methods. It provides a more varied account of implementation than earlier single-case accounts that often chronicled policy debacles.

Goggin contrasts the “fizzle” of California’s experience in the federal-state Early and Periodic Screening, Diagnosis, and Treatment (EPSDT) program with the “sparkle” of the state’s Child Health and Disability Prevention (CHDP) program. He also examines EPSDT implementation in five other Sunbelt states. Finding enormous variation in the political and administrative processes of each case, he considers these findings in the context of four alternative models that explain why some states are more effective than others in implementing child health care programs. These models are not new but synthesize a diverse set of literature into a useful framework for analysis. Goggin ultimately concludes that organizational and personnel variables are more important to implementation success than are economic and political ones. He emphasizes, for example, that provider contentment and administrative stability may greatly exceed state economic conditions and policy liberalism in determining program success.

In *Policy Design and the Politics of Implementation*, Goggin also considers two addi-

tional issues that often go ignored in implementation studies. He attempts to address the economic and political realities within which programs must operate by considering major theories of power and its distribution in the United States. In addition, the book combines a most welcome assessment of the current limits on implementation research with a brief exploration of what “third generation” studies in this area might undertake. Goggin outlines a number of research alternatives. He stresses the need to develop long-term approaches and to “combine large and small-N studies” as has been common in other subfields.

Aside from its more general use in policy implementation, this book will be of particular interest to analysts of health politics. It is a fine supplement to other major works in health policy implementation. Goggin provides less breadth in coverage of health care programs and issues than books such as Frank Thompson’s *Health Policy and the Bureaucracy* but offers a more detailed framework for further exploration. And the book is quite timely for current health policy debates, given its extensive analysis of major prevention-oriented health care programs focused on children. It provides sufficient evidence of program success to hearten advocates of new initiatives in these areas. At the same time, it contributes to the growing body of evidence that government efforts to promote prevention are no panacea in terms of health outcomes or health care cost containment.

Goggin also provides fodder for a debate that has become central to health policy. Federal and state efforts to expand health care services have historically relied on substantial deference to provider-group preferences to assure smooth implementation. However, that approach gave rise to extraordinary costs and resulted in more recent policies that give government greater control over providers. Goggin argues that a fundamental component of successful implementation in his cases is mainstream providers who are content with the program and consider themselves properly rewarded. In fact, he considers the more than doubling of physician fees in the California CHDP case to be perhaps the single most important factor in turning the program around. He suggests that expanded eligibility and open-ended financing commitments without spend-

ing caps would best promote successful implementation of child health care programs. As such governmental expansiveness seems ever less likely in the late twentieth century, health policy implementation may prove even more troublesome.

*Policy Design and the Politics of Implementation* does not provide any definitive framework for examining changing contexts of implementation in health care or other spheres of domestic policy. Nor does it crack the cosmic egg of policy implementation that has attracted—and largely confounded—teams of scholars in the last 15 years. But it does constitute an important contribution to the incremental expansion of our understanding of the implementation process. Much more than a general list of guidelines for putting policies into operation, Goggin enriches the context in which we examine implementation, offers approaches that warrant emulation and expansion, and begins to chart the course of where we must head next.

BARRY G. RABE

*University of Michigan*

**The Decline of Organized Labor in the United States.** By Michael Goldfield (Chicago: University of Chicago Press. xv, 293p. \$25.95).

This book is well written, understandable and thought-provoking. The author's opinion, which I find congenial, is that unions are good institutions and that we should be concerned about their decline. The approach is rather argumentative, in some places verging on polemic, but on the whole it is crisp and interesting. A large amount of factual information is provided in support of the author's claims.

The first contention, which is persuasively demonstrated, is that union membership is declining. The next chapters, which describe union weakness in politics and collective bargaining, are a distraction from the major topic of the book (which is the decline in membership). Some important questions are not carefully addressed. What conditions determine whether labor will win or lose in politics or bargaining? Why did labor succeed in politics when it was weak (during the New Deal) and

lose when it was numerically stronger (1947 and 1959)?

The fourth chapter describes labor-management relations in the Marxist vocabulary of the class struggle and gives some reasons for concern about the decline of unions. In this discussion, the author's ideological predispositions are most evident. He notes that "union organization seems to be coincident with higher productivity, lower turnover, more efficient corporate practices, as well as many other social gains" (p. 76; based on the research of Freeman and Medoff) and that "the decline of trade unions casts a pall on the quality of life for the country as a whole" (p. 77). The contention is not widely accepted; without a more complete economic argument, it should not be. Others have argued that the post-1935 record of unionized firms is one of eroded markets share and that loss of profitability upon unionization is generally anticipated by stockholders. The discussion of labor-management conflict would be substantially richer if this conflict over the economic effect of unionization were elucidated.

The search for the causes of the decline in union membership takes up the second half of the book, which is the most important and serious analysis. Having shown that the prevailing theories (based on economic or sociological correlates of union membership) cannot explain the decline in membership, Goldfield concludes with his "class struggle" explanation of union decline. The argument is that labor law in the United States is unfavorable to unions, allowing employer participation (and interference) in the organizing process. A growing number of employer organizations fight unions with sophisticated legal (and illegal) techniques. The evidence of increased employer resistance during the past 30 years is quite substantial, though there is no reason it should be thought of as a class effort rather than an aggregation of individual business actions taken to prevent loss of profitability. The weakness in the explanation of membership decline is that though employer resistance is correlated with a reduction in union success, no persuasive explanation of the effect of employer activity on workers is offered.

Goldfield has not completely avoided the tendency of union membership studies to search for correlates rather than explanations.



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Before the circumstantial evidence linking employer opposition to changes in voting patterns can be accepted, basic theoretical questions should be addressed. How does a worker decide whether or not to vote for a union? How strongly are workers persuaded to vote for the union by campaign publicity? Why are some workers willing to risk their jobs by organizing a union, while others are afraid even to vote for a union in a secret ballot? The strong empirical relationships that Goldfield finds, for example, linking employer hostility to union losses (p. 203), would be much more persuasive if an explanation of the change in voting behavior were supplied.

At some points the explanation of the impact of employer resistance is not logically consistent. For example, it is noted that employers cause delays between the filing date and the representation election and that these delays are associated with union losses. Why do delays reduce union success rates? The only detailed study of representation elections, the Getman study, found that even unlawful employer campaign practices do not reduce union success rates. In fact, the campaign itself is usually rather unimportant. Goldfield's counterargument is that the Getman study overlooked the fact that the employer activities take their toll *before* the election filing has taken place (p. 199). If this is correct (i.e., if the worker's voting tendencies are set prior to the filing date), it would seem that a delay between the filing date and the election could not affect voting behavior.

The book raises a number of interesting questions that will inspire research in the future. One goal should be an integration of the basic economic logic of the situation with the political factors that Goldfield describes. I am troubled with the absence of an explicit discussion of the effect of unionization on wages, profits, and employment levels. The basic questions, which are relevant to the motivations and incentives of workers and employers, include How much do unions help workers (and hurt employers)? Is the assistance that unions provide worth more than the dues that unions charge? and Does the effect of union organization on business profitability depend on the type of industry and skill level of the work force? Confronting these issues would lead to a more realistic set of explanations linking employer resistance to the reduc-

tion in workers' willingness to vote for unions. For example, Goldfield mentions a study by Farber and Saks that found that within a workplace the higher-paid workers are more likely to oppose union representation. Unions equalize pay scales within workplaces, imposing costs on the higher-paid workers. Depending on the structure of the pay scale and the demographics of the particular work force, it can be that unions impose net costs on a majority of workers via the reallocation of salary. This type of economic result should be integrated into the explanation of union decline.

PAUL JOHNSON

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**Consumers, Commissions, and Congress: Law, Theory, and the Federal Trade Commission, 1968-1985.** Bernice Rothman Hasin (New Brunswick, NJ: Transaction Books, 1987. 236p. \$39.95).

The Federal Trade Commission (FTC) provides a fascinating case study in bureaucratic politics. Responding to the consumer movement of the 1960s and 1970s, the "Little Old Lady of Pennsylvania Avenue" became a zealous regulator unafraid to take on the most powerful economic interests. In turn, however, the agency's aggressiveness precipitated an intense backlash in the late 1970s and early 1980s, and many of its policy initiatives were blunted or terminated as the result of legislative sanctions and personnel changes.

The FTC's policies and its political fortunes have received much attention from journalists, and part of its saga, such as its shift from "mailbag" to "structural" antitrust cases and its emphasis on rule making as a means of policing deception, have been dealt with at length by academicians. Hasin's work may be the first comprehensive treatment of the agency's volatile history over the past two decades, however, and as such it consolidates a good deal of information regarding key policy changes. Although her presentation sometimes lacks clarity and detail, the author generally does a good job in describing the grants of statutory power, the innovative legal theories in the areas of antitrust and unfairness and deception, and the new precedents in civil pro-

cedure that provided the foundation for the FTC's activism.

Hasin also provides an explanation for the commission's behavior. She argues that the agency was captured by consumer advocates such as Ralph Nader and his minions, together with their allies on Capitol Hill. *Capture* may seem too strong a metaphor to many (as it does to me), and indeed the author fails to take on the argument made by Barry Weingast and others that the FTC has simply adapted to the changing balance of forces in its environment. Yet Hasin's assertion in the introduction that "consumerists" successfully established "network relationships within and external to government based on shared beliefs and common objectives" and that these networks enabled them "to influence and shape the direction and passage of law" (p. 2) is amply supported throughout the book. Her documentation of the FTC's persistence in the face of mounting conservative opposition in the mid-to-late 1970s further supports the capture thesis. Turning to ultimate causes, Hasin also examines the rise of consumerism itself. In so doing, she presents an interesting if limited discussion of the antiestablishment mood generated by Vietnam and the rise of a liberal and public-spirited "new class" to compete for political influence with the traditional bourgeoisie.

The book does have some limitations, however. The writing is awkward, both in terms of syntax and organization. In addition, Hasin's use of concepts from political science and economics sometimes reflects a lack of understanding or sophistication. For instance, her observation that "in one variant or another the capture theory of economic regulation is now predominant" (p. 1) may have been acceptable 10 or 15 years ago, but certainly not today. As another example, the employment of "issue networks" in support of her capture thesis seems to miss Heclo's main point.

Perhaps the most important flaw has to do with the author's treatment of consumerists, whom she views as arrogant, self-righteous, inflexible power seekers, ignorant of the needs and desires of the people they claim to represent. These charges may well be true to some extent, and they may help to explain the FTC's intransigence as political support for its activism dwindled. But by describing consumerists with such personal contempt the author

detracts from what purports to be an objective analysis. Hasin's apparent admiration for regulatory reformers such as James Miller and her unsubstantiated endorsement of economic efficiency as a measure of sanity in policymaking have a similar effect.

All in all, however, *Consumers, Commissions, and Congress* is a worthwhile contribution that will serve as a valuable resource for students of the FTC and regulatory policy in general.

WILLIAM F. WEST

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**Leadership at the Fed.** By Donald F. Kettl  
(New Haven: Yale University Press, 1986.  
xiii, 218p. \$22.50).

In this volume, Donald F. Kettl provides an historical account of the Federal Reserve from its founding to the present, emphasizing the relationship between the president and the Federal Reserve chairman. According to Kettl, the Fed's "enormous" and "remarkable" power is a function of the degree of political support it receives from its constituencies (p. 9). Getting this support is the chairman's job. "It has been," Kettl writes, "the principal responsibility of the chairman to recognize the politically acceptable boundaries on [the Federal Reserve's] decisions; to keep those boundaries as broad as possible; and to attempt to keep the Fed's policies within those boundaries" (p. 13). The president is the most important constituent for the Federal Reserve, so the primary test of leadership for a Fed chairman is the maintenance of the relationship between himself and the president.

With the exception of a chapter devoted to Congress-Federal Reserve relations in the 1970s, the book focuses on the twists and turns in the relationship between the president and the Federal Reserve. Kettl identifies three kinds of patterns of relations (he also calls them *strategies of leadership*) between the Fed and president: accommodation, confrontation, and transformation. In periods of accommodation, presidents get the monetary policy they want. In periods of confrontation, there is conflict between the president and the Fed about whether to pursue economic growth or to fight inflation. Resolving these conflicts has re-

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quired fundamental redefinition of the Fed's mission and the chairman's relationship to the president. Such fundamental change involves transformation, the third kind of relationship, or strategy. Transformation is the distinctive result of "special leadership from unusual Fed chairmen" (p. 16)—Marriner Eccles, William McChesney Martin, and Paul Volcker. Kettl insists that "the driving power of these transformations came from the chairmen themselves" (p. 202), not the president.

The strength of the book is its historical scope and Kettl's skillful historical reporting. This is a lively chronological account of Fed-presidential relationships as revealed in interviews and presidential documents. The research involved is ambitious and impressive, drawing on documents from seven presidential libraries and at least three other specialized collections. As far as I know, no one writing on the Federal Reserve has taken such excellent advantage of the Burns papers in the Ford Library and the Erlichman papers in the Fenn Gallery in Santa Fe. These new sources will make chapter 5 a special treat for students of economic policy in the 1970s. Students of monetary policy and bureaucratic politics are certain to find the book useful as a rich historical compendium of documentation about a very significant aspect of monetary politics. No other single source provides such a complete survey of the important transactions between presidents and the Federal Reserve.

Kettl does not argue that his account reshapes our view of Federal Reserve history, nor does it appear to do so in any fundamental respects. There are, however, points on which Kettl offers new interpretations (e.g., on the reasons why Truman decided not to reappoint Eccles) and instances in which his account differs from those offered by his predecessors. As one example, consider the account of Marriner Eccles's conflict with Treasury Secretary Morgenthau in the late 1930s (pp. 55–59). Kettl, focusing on the leading role of Eccles, does not mention the hypothesis of Friedman and Schwartz (Milton Friedman and Anna J. Schwartz, *Monetary History of the United States, 1867–1960*, pp. 520, 522) that the policy conflict originated in a New York Federal Reserve Bank scheme to limit the power of the board. Had Kettl considered the Friedman and Schwartz view, it would have added a very interesting dimension of complexity to our

understanding of the strategic situation. Clifford writes of these events that Morgenthau doubted that Eccles was the "master of his house" (A. Jerome Clifford, *The Independence of the Federal Reserve System*, p. 156), a position consistent with the Friedman and Schwartz account. Kettl concludes from his analysis that Eccles learned that "in any serious dispute with the Treasury, . . . he would have to fight the president, and that was one battle he could not win" (p. 56). However, Clifford, after examining many of the same primary sources, concluded that "the episode showed that the President would not always give quick support to the Treasury" (Clifford, p. 158). To have explored these links to prior scholarship would undoubtedly have made the work longer and less lively. It would also have added a valuable scholarly dimension.

Kettl's central argument that the decisive political relationship in this case is between the president and the chairman has been advanced by a number of other scholars. This is also true of Kettl's conclusion that the relationship is not primarily one of protracted, overt conflict. The novel information in Kettl's book mostly serves to add detail—often fascinating—to a larger outline already in place.

Readers concerned with developing concepts about leadership, exploring theories of bureaucratic power, and testing of hypotheses will find the book disappointing. There are many instances in the work when greater conceptual clarity, more skepticism about informant interview reports, and more analytic curiosity about the data would have produced improvements without loss of integrity in the historical mode of analysis.

In discussing the Fed's power, Kettl appears to conflate the Fed's bureaucratic power and the power of monetary policy to influence the economy. Monetary policy would be powerful in influencing the economy no matter who controlled it. But we would not call the Fed powerful without evidence of its bureaucratic power—its ability to achieve its goals despite political opposition. However, Kettl seems to regard a developing consensus among economists about the power of monetary policy to influence economic behavior as evidence of growing bureaucratic power at the Fed. More clarity about this might have led Kettl to place more weight on his evidence that presidents Wilson and Hoover both had considerable

respect for the independence of the Federal Reserve Board long before the first "transformative" events of the 1930s (pp. 24, 38). The Federal Reserve Board's ability to pursue an apparently disastrous course of action during the Hoover years despite opposition from many quarters (pp. 29-44) testifies to its considerable bureaucratic power at an earlier date than Kettl seems to credit.

One would also welcome more systematic development of the sequential leadership patterns Kettl has discovered in this history. Does Kettl think his concept of *transformational leadership*, which is rather different from James McGregor Burns's similar-sounding idea of *transforming leadership*, can be applied to other settings? Can Kettl offer further guidance as to the hallmarks of transformational leadership and its dynamics? Is there a way of systematically characterizing a relationship so we can know when transformation is taking place?

The historical account of Fed-president relations does not always fit very well with the sequential leadership patterns Kettl describes. At least one of the three instances of confrontation Kettl identifies (1965-66) did not result in transformation. One of the three instances of transformation (1979-82) did not follow confrontation. Why does not Kettl consider the period leading up to the 1951 accord to be one of transformation? How was Chairman McCabe, a weak and mistrusted leader, able to pull off the accord? For that matter, why did Truman appoint a chairman he didn't trust? If the chairman was not, in fact, the central actor in this important—if not transforming—event, then perhaps the basic argument of the work needs modification. Perhaps that modification would be useful in understanding more fully why Arthur Burns, whose relations with Nixon were often strained, enjoyed much better relations with Ford.

On a minor level, I would note flaws such as these: Contrary to the opinion of one of Kettl's anonymous informants, the change from Martin to Burns did *not* result in more Federal Open Market Committee conflict, as measured by the frequency of split decisions with one or more dissenters (p. 120). The 1974-75 recession, which actually lasted from November 1973 to March 1975, was *not* one of the shortest in U.S. history (cf. p. 136). It is therefore not compelling evidence of the virtu-

ous consequences flowing from close policy coordination between Arthur Burns and the White House. The data in Table 2 (p. 137) do *not* conclusively show that the CEA had a "steady bias for easy money." This is clear on Kettl's own evidence that the Fed itself has a "steady bias for tight money" and that the CEA has told presidents that Fed policy is "just right" nearly half the time.

Despite my complaints, I enjoyed reading this book and found myself engaged throughout. My desire that Kettl do more should not take away the fact that he has written a book both readable and useful. Students of macroeconomic policy will find it to be a valuable reference source.

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**Appointment of Judges: The Johnson Presidency.** By Neil D. McFeeley (Austin: University of Texas Press, 1987. xi, 199p. \$22.50).

Neal McFeeley guides us through the appointment process of federal judges from the perspective of the Oval Office during the Johnson administration. His is the sixth publication in a series of works based upon the presidential papers of Lyndon B. Johnson. As such, *Appointment of Judges: The Johnson Presidency* is a unique and authoritative description of the political pressures, strategies and proclivities that may enter the exercise of the presidential power to appoint federal judges. There are already some excellent studies of the politics of judicial recruitment and selection. That McFeeley further elucidates our understanding of the appointment process is to his credit and speaks to the importance of his study.

McFeeley begins with an overview of the appointment process that covers much familiar ground, yet integrates the major works in this area (notably those of Henry Abraham, Joseph Harris, and Joel Grossman). McFeeley paints for us a picture of a frustrated, almost aloof Lyndon Johnson prior to the 1964 election, supporting Kennedy appointees whom he does not enthusiastically endorse. Unable to fully develop his own appointment process, the president often delayed making nominations

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for long periods, and McFeeley blames most of the unqualified ratings of nominees attached to the Johnson administration on this period. After the resignation of Attorney General Kennedy and the 1964 election, the Johnson selection process began to emerge. From this point on, McFeeley hits stride in giving us the evidence to support his theme; namely, that Johnson's success in appointing "Great Society liberals" to the lower federal courts and the Supreme Court was the result of the selection process he designed to "balance the politics of judicial selection with his own policy objectives" (p. 138).

The success of the process depended upon several key elements, not the least of which was the president himself. Johnson's "values affected . . . the selection of judges to all levels of the federal judiciary" (pp. 51-52). Johnson's control over the process was substantially enhanced by another central ingredient, the almost one-man show in the Justice Department, Ramsey Clark. Clark and his liaisons in the White House were to ensure that after due respect was paid to senatorial courtesy no appointee slipped through the process who was not personally loyal to Johnson and politically committed to his programs.

Chapter 5, "The Criteria of Choice" (perhaps the best chapter), unveils "how" the Johnson machinery ensured the purity of the judicial candidates. The "loyalty program" was at its height in 1967, "rejecting those [judicial] candidates who were not 'all-out-J-men' and did not support Johnson's Vietnam actions" (p. 86). McFeeley attributes Carp and Rowland's findings that "appointees of 1967 were markedly less liberal than other Johnson appointees" to Johnson's insistence on Vietnam loyalty. This underscores not only Johnson's impact on the appointment process but also the relationship between appointing president and judicial behavior. McFeeley leads us further into this connection by using some of his more powerful documentation to illustrate how "rigorous" the screening was for judicial nominees with regard to their ideological loyalty to the Great Society, in particular, to civil rights. No appointment was made, particularly in the South, unless Ramsey Clark acknowledged that the nominee supported civil rights; "neutrality on the race issue [was] not enough" (p. 86). Many times President Johnson conducted personal interviews with

nominees individually and in groups to ensure that they understood what he expected from them on this and economic issues.

McFeeley convincingly offers his analysis of the politics as an underlying explanation for the solidly liberal propensity of the Johnson appointees as evidenced in the major behavioral analyses. Johnson's concern for "correct" views on the civil rights issue does not come as a surprise. The intensity and duration of his commitment to the ideological purity of the nominees on the issue is, however, a revelation.

Finally, some of what McFeeley discusses is vintage Johnson humor. For example, Johnson would give a "good talking" on civil rights to the prospective nominees (sometimes appointees) and would also ask them to avoid getting "arrogant," to "act as though they had to stand for election every once in a while," and to "get off the bench when you reach retirement age. . . . There is something about the nature of the job with everybody saying 'Your Honor' which seems to cause this [arrogance]" (pp. 54-55, 169). Johnson's intense concern for appointment of younger judges, comparable in average age to Reagan's judicial appointees, was another successful selection strategy. "Seventeen years after leaving office, one of the Supreme Court justices, thirty-five of the judges of the courts of appeals, and eighty-seven of the district judges appointed by Johnson remain on the bench in active or senior status" (p. 4).

That "politics permeated the process of judicial selection in the Johnson administration" and that he "understood the political aspects of judicial selection" (p. 138) are understatements. Judgeships were poker chips in Lyndon Johnson's high-stakes political game. Only one caution to all those who shall read *Appointment of Judges*, don't slight the footnotes. Some intriguing information is there. The recruitment and selection of federal judges is an integral and significant aspect of the governmental process and judicial behavior—so much so that we can never have enough studies in the vein of McFeeley's work.

DEBORAH J. BARROW

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**Parties in Transition: A Longitudinal Study of Party Elites and Party Supporters.** By Warren E. Miller and M. Kent Jennings (New York: Russell Sage Foundation, 1987. xxv, 284p. \$22.95).

*Parties in Transition* is a survey study of delegates to past conventions—specifically those of 1972, 1976, and 1980, in both parties—and it provides political scientists and journalists who are focusing on the 1988 campaign ahead with useful background on one key dimension of campaigns past. The book is about change and continuity in opinions, attitudes, and behavior of delegates as an important group of national campaign activists.

Warren Miller and Kent Jennings have drawn upon two large surveys of convention delegates. The first was done in 1972, using both personal interviews and mailed questionnaires; it has been reported on by Jeanne Kirkpatrick (*The New Presidential Elite*, 1976). The second survey, entirely mail questionnaires, was administered in 1981 to delegates to the three preceding conventions. A group of delegates interviewed in 1972 were thus reinterviewed, and new delegates to the 1976 and 1980 conventions were surveyed for the first time. The 1972 survey was first viewed as a stand-alone study of women in politics. In its aftermath, the principal investigators concluded that a panel design was needed, with the 1972 delegates to be reinterviewed after the 1976 and 1980 conventions. No further interviewing was done until 1981, however, and then the “new delegate” interviews (i.e., of those who were not delegates to the 1972 conventions) became of prime importance. All these shifts give an impression of the project’s being disjointed and ad hoc. Nonetheless, interesting and important findings have been reached, and a wealth of data has been made available for secondary analysis through the Inter-University Consortium for Political and Social Research.

In one of the most valuable sections of their work, Miller and Jennings explore changes between 1972 and 1980 in the incentives for political involvement. Basically, they find that the new 1980 Republican and Democratic delegates differ not at all in their weighting of incentives for involvement—among those classified as *policy-oriented*, *partisanship*, *social*, and *personal career*. In 1972, however,

the two delegate groups had differed greatly. The Democrats then responded more than the Republicans to policy incentives and much less to old-fashioned partisanship. The new 1980 GOP delegates were less moved by considerations of partisanship than their 1972 counterparts had been, while the new 1980 Democrats responded more to partisanship than had their 1972 brethren.

Miller and Jennings conclude that the several “classes” of Democratic delegates differ little in ideological orientation and policy preferences. The new 1980 delegates have much the same policy outlooks as do the 1972 class: “The delegates who nominated McGovern *and* who remained active in the 1980 campaign were, at least in 1981, not substantially to the left of their 1980 counterparts” (p. 135). The data bear this out. I disagree with the authors’ conclusions about the Republicans, however. They argue that “those who were chosen as delegates in the year of Reagan’s nomination [1980] were visibly more conservative than those first chosen in 1972 and 1976” (p. 133). In fact, the interclass differences Miller and Jennings present are far too modest to justify a “swing-to-the-Right” conclusion. The mean issue position score of the 1972 delegates was 71.1, that of the new 1980 delegates 75.2; the liberal-conservative self-placement of the former was 68.9, of the latter 74.4, on a scale where 0 is most liberal and 100 most conservative. In fact, neither partisan set of classes changed much in policy outlook; interparty difference, not intraparty change, is the big story.

Examining the differences between delegates and party rank and file, Miller and Jennings reach a conclusion that I find entirely persuasive: “Regardless of level of intensity, no rank-and-file stratum matches the least committed stratum of party delegates *in reflecting modal party positions*. . . . Since delegates are *committed activists*, it would be surprising if they did not exceed their ‘constituents’ in terms of ideological zeal” (p. 198, emphasis added). The authors go on, however, to argue that between 1972 and 1980, GOP elites swung to the Right, leaving them “seriously out of tune with their followers” (p. 219). In the earlier report on the 1972 survey we were told that Democratic delegates were then far to the Left and unrepresentative of their party’s rank and file. Miller and Jennings are clearly

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uncomfortable with this conclusion and inferences drawn from it; elites always "exceed their 'constituents' in . . . ideological zeal." Still, they are inclined to fit their 1981 findings into the current popular theme that Reagan-era Republican elites have swung far right, compared to the public at large or Republican identifiers.

In fact, the data presented in *Parties in Transition* point to another conclusion. The "distance" between 1980 Reagan delegates (the most conservative group) and the Republican rank-and-file (from the 1980 Michigan election surveys) was 11 points on the liberal-conservative self-placement scale, and 20 points on the issues index (mean scores); while the distance between Kennedy delegates (the most liberal) and the Democratic rank-and-file was 20 points and 19 points respectively. The Carter delegates scored 49 in liberal-conservative self-placement compared to 55 for the party rank and file and 40 on the issues index compared to 49 for Democratic identifiers. Considering all measures, Democratic delegates in 1980 stood in about the same relationship to Democrat identifiers as Republican delegates did to GOP supporters in the general public—with the delegates in each case more ideologically coherent and distinct.

I should stress in conclusion that my differences with Miller and Jennings over the interpretations of their data pale in comparison to my appreciation of the overall strength of their study. They have given the profession a valuable collection of longitudinal data.

EVERETT CARLL LADD

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**The Limits of Victory: Ratification of the Panama Canal Treaties.** By George D. Moffett III (Ithaca, NY: Cornell University Press, 1985. 261p. \$24.95).

What are the prospects for presidential leadership if the biggest treaty ratification campaign in recent memory failed to budge public opinion? George Moffett analyzes ratification of the 1978 Panama Canal treaties as an "object lesson in the limits of public education" (p. 136). The degree that Jimmy Carter staked his presidency on these treaties is still remembered, as is the cost he paid during the longest

hearings and debate in the Senate's history, to win with only one vote to spare ratification of a new Panama Canal Treaty and a Treaty of Permanent Neutrality. Moffett concludes that "it proved easier to arouse opinion than to direct it" (p. 131). This finding would be unremarkable had the Carter administration not devoted so very much time, effort, and scarce resources to the task.

Perhaps unusual in passionately dividing supporters and opponents, these treaties nonetheless illustrate "the difficulties involved in trying to force a major policy initiative on an unwilling public and a reluctant Congress" (p. 117). Moffett's book is a case study on this theme. It also has broader relevance to a recurring problem for U.S. foreign policy: the need to build a majority coalition in the absence of a majority. Thus the ratification campaign is a dramatic but not unusual case of how hard presidents and Congress must work to legitimate foreign policy.

The canal issue has passed, but deep divisions in the foreign policy consensus remain. Policy conflict continues, with where one stands depending on what one believes, as well as Allison's "where you sit." This insight goes back to early foreign policy theorists who accounted for decisions in terms of shared beliefs as well as organizational and vested interests. Complicating factors operating since the mid-1970s are fragmented and divisive—rather than shared—images and competing belief sets that cause conflict over foreign policy. Moffett, a historian, could benefit from the insights of foreign policy scholars who use a perceptual approach. Nonetheless, he does a good job. In the chapters "The End of Containment" and "The Divergence of Policy and Opinion," of setting canal policy in its historical and cognitive contexts.

Moffett poses three questions about the ratification campaign: Why did the treaties almost fail? How did the Carter administration finally win? and Why did the effort exact so high a price? Moffett is well equipped to suggest some answers. First as the research director for a protreaty citizens committee and then as assistant to the chief of staff in the Carter White House from 1978 to 1981, Moffett experienced the effort to mobilize support for the treaties. His participation enriches, and only occasionally taints, his analysis. Moffett interviewed over 50 participants in and outside of

government and used additional oral history interviews now at the Carter Presidential Library.

*The Limits of Victory* has three sections. The first describes ratification strategy as planned and implemented. This narrative substantiates many of the points in previously published works (William L. Furlong and Margaret E. Scranton, *The Dynamics of Foreign Policy Making*, 1984; and William J. Jordan, *Panama Odyssey*, 1984). The "Organizing for Victory" chapter also adds new details and insights to the historical record that will delight those who follow canal policy. The episode that most analysts want clarified concerns why the White House accepted the DeConcini reservation. Moffett sheds some new light, letting participants give their own interpretations. But the sum of the parts is not a convincing whole. Moffett fails to assess responsibility, and leaves the reader unsatisfied in trying to comprehend a major blunder, if not a fatal flaw, in Carter's dealing with the Senate.

The most original part of the book is his analysis of public opinion. Despite the impression at the time of protreaty movement in national polls, Moffett found that public opinion "remained almost completely static, . . . public attitudes never budged" (p. 116). Moffett used long-term trend analysis to examine comparable questions in different polls and to compare questions grouped by information content. For those interested in the data, Appendix A contains all 36 questions asked by 10 major organizations between 1976 and 1978. Moffett concludes that apparent movement "was nothing more than a trend toward wording in the polling questions themselves," with the wording itself becoming "more likely to draw favorable responses" (p. 122). However, changes in wording reflected exactly what the Senate was doing at the time: clarifying and strengthening treaty language on U.S. canal defense rights. Moffett's findings do not diverge from those reported by the State Department's opinion analyst, Bernard Roshco ("The Polls: Polling on Panama—Si; Don't Know; Hell, No!" *Public Opinion Quarterly* 42 [1978]: 551–62). Moffett's conclusions are confirmed in large part, despite some methodological criticism, by Ted J. Smith III and J. Michael Hogan ("Public Opinion and the Panama Canal Treaties of 1977," *Public Opinion Quarterly* 51 [1987]: 5–30).

Of broader importance is the problem that Moffett demonstrates. To what extent is such an opinion pattern an artifact or a measure of reality? Moffett sees an artifact and rightly chides the press for overgeneralizing a pattern based on an artificially high baseline for anti-treaty opinion. That is a fair point. But shouldn't poll questions change as policy content changes? If we want to know public reactions as policy evolves in the making, don't pollsters have to include new details as they emerge?

In the third part of the book, on interest groups and ratification, Moffett almost lapses into a plaintive mode chastising affected interests for not doing more to help the administration. In "A Policy without a Constituency," he describes the activities of groups ranging from the United Nations Association, the UAW, and a variety of church organizations to banks and corporations. This is the weakest part of the book, largely because groups were, in fact, so peripheral to the ratification effort. In "The Economics of Ratification," Moffett analyzes lobbying by those most directly affected by the treaties, U.S. flagship owners, public port authorities, and midwestern farm exporters. As Moffett rightly notes, the adverse impact on administration plans by these maritime interests was exacted after the treaties passed, in the implementing legislation that had to pass both houses.

All in all, Moffett's book fills a gap in the literature on the 1978 canal treaties. He analyzes all of the domestic political influences on the ratification process and very ably tells the story of the administration's ratification plans and its "Pyrrhic victory" (p. 11).

MARGARET E. SCRANTON

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**The Political Theory of the Compound Republic: Designing the American Experiment.** By Vincent Ostrom. 2d ed. (Lincoln: University of Nebraska Press, 1987. xxviii, 240p. \$22.50 cloth; \$8.95 paper).

In *The Political Theory of the Compound Republic*, Vincent Ostrom seeks to reconstruct the philosophical assumptions that underlie our original form of government and to



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demonstrate their continuing relevance and practicability. At the core of Ostrom's analysis is the notion of "constitutional choice." Beginning with the Declaration of Independence, Ostrom emphasizes the extraordinary degree to which the ex-colonists were free to design institutions which would secure their "safety and happiness." Although choice is always circumscribed by political necessity, necessity for Ostrom turns out to be rather benign. There is no mention of slavery here; necessity, in the form of the existence of independent political communities, points happily to some form of compound republic. By a *compound republic* Ostrom means not only a federal system of government but also "compound of decision structures within each unit of government" (p. 152). Still, because federalism is the most novel contribution to the U.S. experiment, Ostrom rightly devotes most of his attention to this principle.

In elaborating the role of federalism and its place in the compound republic, Ostrom relies principally upon the arguments of James Madison and Alexander Hamilton in *The Federalist*. In so doing, he explicitly rejects the interpretation of Martin Diamond and others that Publius, far from embracing federalism as a political good, remained skeptical of this innovation. He seeks to explain the discrepancy between Madison's and Hamilton's known antagonism to the states at the Federal Convention and their newly acquired "zeal for the federal principle" (p. 107) by suggesting that they learned something from their deliberations in Philadelphia. Although this is certainly possible, Ostrom offers no evidence in support of his claim.

To separate himself further from earlier theoretical studies, Ostrom disclaims any interest in the *motives* (which he mistakenly believes to be the same as the *original intentions*) of these two framers. This is partly because motives (unlike intentions) can never be known with certainty, but it is also because he wants to examine the idea of federalism strictly on its merits. Yet so congenial does Ostrom find the idea of federalism that he cannot bring himself to believe that Madison and Hamilton did not share his view. Despite his announced intention, Ostrom repeatedly brings his "sympathetic" reading of *The Federalist's* view of federalism to bear on the merits of the principle.

Chapters 2-7 seek to "reconstruct" the theory of the compound republic as it emerges in *The Federalist* and to relate this theory to the larger issue of constitutional choice. Here Ostrom argues that the political theory of *The Federalist* provides a genuine alternative to the Hobbesian view of the modern state. Yet once we get past the obvious differences, Ostrom's reading of *The Federalist* suggests a far closer affinity with Hobbes than is in fact the case. According to Ostrom, *The Federalist's* view of human nature is one of selfish individualism, while the purpose of government is the efficient allocation of goods and services. In a work that seeks to recover the political theory of the compound republic, it is surprising to find not one trace of republican thought. There is not the slightest suggestion that *The Federalist* prized republican self-assertiveness or saw the continuing necessity of civic virtue, even if it no longer relied exclusively upon it. Ostrom's "basis assumptions" are rather the building blocks of liberal political philosophy, as first articulated by Hobbes and later refined by Locke and Hume.

As a political theorist, I found Ostrom's discussion of the practical question, How well has the compound republic worked? to be the most engaging. Here Ostrom tackles two big questions, slavery and the rise of machine politics and boss rule. I admire his forthrightness in raising the slavery issue in connection with federalism but was disappointed in his treatment. Ostrom fails to consider the role of the states as political units in perpetuating slavery and racial discrimination. Instead, he focuses on the "variety of remedies" presently available to blacks in the compound republic.

Ostrom then discusses how the federal system was corrupted after the Civil War and transformed by Progressives into a centralized national government. But in contrast to those who continue to press for greater centralization and "responsible" government, Ostrom insists that a return to the original compound republic can promote both justice and efficiency. These are big claims, and the reader is naturally led to wonder how a federal system will produce all these political goods. At this point, Ostrom moves to a discussion of a "federal theory of public administration," but his argument needs further elaboration to be made comprehensible to the generalist. If, in the last chapter, Ostrom had developed his

federal theory of public administration instead of recounting his intellectual odyssey through, among other things, "Laswellese" and "Economese," the practical advantages of the compound republic, so badly in need of restating, might have emerged more concretely.

JEAN YARBROUGH

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**When Federalism Works.** By Paul E. Peterson, Barry G. Rabe, and Kenneth K. Wong (Washington: Brookings, 1986. xvi, 245p. \$28.95 cloth; \$10.95 paper).

In the years since the original publication of Morton Grodzins' classic, *The American System*, a growing chorus of critics has charged that the "mild chaos" that Grodzins praised has reached the point of total chaos, in which confusion and complexity have rendered the federal system totally unmanageable. In a valuable reassessment, Peterson, Rabe, and Wong present an in-depth analysis of a number of federal domestic programs which, in their early years, contributed to the image of total chaos. The authors find, however, that persistence, growing professionalism of local administrators, and some additional flexibility from national officials often succeed in overcoming the implementation problems that commonly face new national-local programs.

The authors contend that the image of cooperative federalism emerged from the federal system's early emphasis on *developmental* programs, which seek to boost individual local economies. The general agreement on goals led to relatively harmonious relationships among national, state, and local governments. In the 1960s and early 1970s, more emphasis was placed on *redistributive* programs, which seek to reallocate wealth (or other things of value) from one group to another. The controversial nature of redistributive programs provoked considerable difficulty in their early years, a circumstance that led to charges that the federal system had grown unmanageable. In time, however, the difficulties were overcome to a large degree.

In spite of the relatively cooperative nature of developmental programs, the authors conclude that they generally involve activities that

state and local officials would support anyway (assuming that they have sufficient resources, a matter of some uncertainty). As a result, a national role in developmental programs may not be necessary in most cases. By contrast, although a national role in redistributive programs is often controversial, interjurisdictional competition for wealth and jobs makes sub-national governments biased against redistributive efforts. A national role is therefore necessary for redistributive programs. The frequently hostile local political climate often means that redistributive programs are more effectively administered by professionals who are somewhat insulated from the local political environment. The local appeal of developmental programs, however, causes politicized administration of those programs to be relatively successful. A mayor who believes his or her political career will be boosted by a downtown redevelopment project can be strikingly effective in overcoming obstacles to that project. Administrative arrangements that are suitable for one type of program, then, may be highly unsuitable for another.

The most problematic part of the analysis is the recommendations for reform—always a perilous enterprise in U.S. federalism. The recommendations stem from the following premise: "In general terms the proper distribution of power (in a federal system) can be readily stated" (p. 11). Decisions that have very broad consequences should be made nationally; decisions with localized consequences are best made locally. The history of U.S. federalism indicates that this formulation is extremely difficult to apply in practice. Were northern whites affected by discrimination against southern blacks? Does vocational education have national consequences? The authors' answer to the latter question is negative; however, in a society with a mobile labor force and significant foreign economic competition, the cumulative effects of a number of local vocational education programs might have significant national consequences. As Schattschneider argued in *The Semisovereign People*, the size of a political conflict often has major policy implications; making decisions regarding the distribution of responsibilities (national, state or local or both) is, consequently, rarely straightforward.

The authors contend that developmental policies normally involve activities that state

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and local governments are inclined to support with or without national help and that national grant programs that support developmental activities are not very effective in targeting resources to jurisdictions with the greatest needs. As a result, the national government should avoid involvement in developmental programs unless significant externalities are present or a redistributive program needs some developmental features to enhance its acceptability. The national government would then be free to devote more attention to redistributive programs, a shift that might lead to improvements in individual redistributive programs and to greater coordination across redistributive programs.

This formulation presents several problems. First, it asks the president and Congress to refrain from involvement in programs that are often popular and build political goodwill and to focus their attention on programs that are typically controversial and create political antagonisms. That advice can expect a cool reception in Washington. Moreover, to the degree that developmental programs help to create political obligations that can help build support for decisions on other issues, an end to national involvement in developmental programs could lead to even greater difficulty in dealing with redistributive issues. Finally, national developmental programs that manage to provide *some* resources to localities in need (at the expense of providing resources to areas not in great need) may still have merit if the alternative is nothing for areas in need. When zoning, building codes, and other devices are used to exclude poor people from many localities, aid to people and aid to places often overlap, particularly if policymakers want poor people to have opportunities in the private labor market.

Overall, *When Federalism Works* is a valuable corrective to the view that the federal system is an incomprehensible, unmanageable morass. Students of intergovernmental relations, public policy, and public administration would do well to explore its insights.

DAVID C. NICE

*University of Georgia*

**Controlling the Federal Bureaucracy.** By Dennis D. Riley (Philadelphia: Temple University Press, 1987. xiv, 200p. \$24.95).

Most of the scholarly work on accountability and control in public administration derives from the well-known Friedrich-Finer debates on internal versus external sources of control. This book addresses the latter type of control. It describes the main institutional sources of external control over the federal bureaucracy, namely, the president, Congress, the courts and various components of the broader public. The book's main thesis is that the well-known combination of special interest groups, congressional committees, and career public employees (whether iron triangles, subgovernments, or issue networks) results in a policy-making process that makes it difficult to control the federal bureaucracy. The author moves from this widely accepted characterization of U.S. public administration to a much more controversial one about the federal bureaucracy, namely, that the federal bureaucracy is fundamentally irresponsible.

The book is well written and presents very thorough explanations of the subtleties of various external control mechanisms and details the nuances of controllers' interactions with public administrators. In the process of detailing control strategies, the book illustrates some fundamental dynamics of public administration, such as how an accumulation of several reasonable, independent administrative decisions can have unintended consequences. The book's section on law as a control mechanism presents insights that are often missing in discussions of control of public administrators.

The book has two serious shortcomings: its narrow focus leaves us with an incomplete picture of the prospects for accountability of public administrators, and it puts forward a conclusion that is beyond the reach of the data presented. Of course, choosing to limit the scope of his book to external mechanisms of control is the prerogative of the author. However, the book is not exhaustive in its discussion of external sources of control. For example, there is no discussion of the media as watchdogs with potential to contribute to external control efforts. Of more serious concern, the book is incomplete because it does not address adequately the issue of cumulative

or interaction effects of the various control mechanisms.

Having once made the choice to limit the scope of his analysis, the author must also accept limitations on the breadth of his generalizations. The conclusion of this book, namely, that the federal bureaucracy is fundamentally irresponsible, cannot be reached from the data and discussion presented in the book. The author only makes passing reference to internal mechanisms of accountability. Hence we know only part of the accountability story. More importantly, the book does not present analyses or systematic arguments to show that external mechanisms fail to result in responsible federal administrators. Furthermore, failures at external control do not necessarily yield irresponsible administrative behavior. To support this conclusion the book would have to examine the cumulative effects of the various mechanisms of control, rather than just the potential for their separate failures, and link those effects to irresponsible administration. It may be possible to make such a case, but the author has not done so in this book.

It is time to move beyond the Friedrich-Finer debate and the external-internal dichotomy of accountability. Accountability is a complex, dynamic *relationship* between administrators and a multitude of diverse, legitimate claimants. Agencies and administrators play an active part in the accountability relationship. They often take the initiative in meeting claimants' expectations and frequently can choose from a variety of public sector accountability mechanisms. Such administrative initiatives can yield both responsible and irresponsible behaviors. Consequently, complete models of accountability must incorporate the perspective of public administrators. We need to explore both the opportunities and constraints public administrators see in the various accountability mechanisms and the conditions under which administrators are likely to rely on one mechanism over another.

The book presents very good descriptions of external control techniques. We know that public administrators often face different types of accountability mechanisms, sometimes simultaneously, at other times in sequence. Some of these types of accountability systems are discussed here, especially the political and legal mechanisms. Other accountability

mechanisms, such as bureaucratic and professional systems and the effects of these systems on administrative behaviors, are missing from the discussion. Given these omissions, the book cannot support the conclusion it offers to its readers.

BARBARA S. ROMZEK

*University of Kansas*

**Braking the Special Interests: Trucking Deregulation and the Politics of Policy Reform.** By Dorothy Robyn (Chicago: University of Chicago Press, 1987. ix, 295p. \$24.95).

Of all the deregulatory successes of the last two decades, the reform of the motor carrier industry is perhaps the most exceptional. It represents a rare victory for diffuse political forces over concentrated interests. Dorothy Robyn furnishes an eyewitness account of the final months of the saga that led to the passage of the Motor Carrier Act of 1980.

Robyn maintains that four "ingredients" explain the changes in the regulatory system: (1) strategic use of economic evidence and analysis to demonstrate the merits of reform; (2) formation and maintenance of an ad hoc coalition to lobby actively; (3) use of transition strategies to soften the opposition to change; and (4) strategic bargaining by the president to gain sheer political leverage. After a brief survey of motor freight regulation and an overview of the political battle over trucking deregulation, the author analyzes each of these four factors in turn. She relies heavily on her interactions in Washington with those involved in the reform effort—particularly the members of the ad hoc interest-group coalition favoring change—in the eight-month period that culminated in the signing of the Motor Carrier Act. Robyn concludes by buttressing her argument that these four elements of political strategy were necessary and by indicting economic theories of utility maximization.

The strength of this book is that it provides fascinating details of the machinations that took place before legislative victory was achieved. However, the fact that the author only went to Washington in December 1979 may have had some negative consequences. In

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particular, Robyn pays relatively little attention to pre-1979 events at the Interstate Commerce Commission (ICC) that substantially altered the regulatory system. Generally, her analysis of the ICC is less thorough than her exploration of the agency's external political environment. This discrepancy probably reflects her involvement in the ad hoc pro-reform coalition that by late 1979 was pushing for statutory passage of deregulation. Despite these reservations, the author provides a level of detail that will undoubtedly be very satisfying to those fascinated by deregulation or how large-scale policy changes are possible in a politically charged context.

The book is weaker in its attempts at developing a theoretical framework. To an extent, such objections are endemic to any case study. Yet not all of the ingredients stipulated as necessary for reform were present even in the motor carrier case. Consider that evidence on the costs of motor freight regulation existed long before motor carrier reform occurred; that the ad hoc interest-group alliance was no match for the American Trucking Association and the Teamsters; and that transition strategies were not heavily employed. Nor is it apparent what tangible instruments Jimmy Carter had at his disposal, besides his own persuasive abilities, that made him more successful than his predecessors. These shortfalls suggest that Robyn has not delineated all those factors that were sufficient for change.

This leaves one with the feeling that perhaps the author was too quick to reject economic theories of politics. Clearly, Robyn is correct in her assertion that explaining deregulation is problematic for those adhering to the standard economic perspective on regulatory politics. Nevertheless, the analysis of reform requires a more intensive investigation of political actors' incentives and resources. If theories are going to be rejected, then they should be given a thorough hearing.

Robyn's work will be of interest to anyone concerned about deregulation or nonincremental policy change. Whatever its shortcomings, it chronicles an important segment in the history of economic regulation.

LAWRENCE S. ROTHENBERG

*California Institute of Technology*

**Organization in a Changing Environment: Unionization of Welfare Employees.** By Russell K. Schutt (Albany: State University of New York Press, 1986. xiv, 243p. \$34.50 cloth; \$14.95 paper).

Observers of welfare politics sometimes speak, almost yearningly, of the prospects for uniting welfare workers and their clients in behalf of social reform. That prospect seemed tantalizingly close during the midsixties, when demands for reform came both from clients on the receiving end of the welfare system and also from the bureaucrats within the system. In the politically charged atmosphere of that era, clients mobilized within welfare rights organizations and bureaucrats mobilized within labor unions briefly raised the possibility of a two-pronged attack on the welfare system.

Robert K. Schutt's account of workers' unions suggests that hopes for a worker-client alliance were, and may continue to be, nothing more than wishful thinking. This book indicates that bureaucratic activism was an epiphenomenon associated with the temporary infusion into the welfare bureaucracy of a small cohort of college-educated activists who did not share the long-term career interests or short-term bread-and-butter concerns of their colleagues in the permanent bureaucracy. Schutt's study of welfare-worker unionism provides a valuable counterpoint to the literature on the rise and fall of client-centered organizations. His detailed and well-documented case study of welfare-worker unionization in Cook County, Illinois, portrays a radical union movement born with the fervor of social reformism that quickly shifted from "social consciousness" to "bread-and-butter" concerns.

Schutt attempts to account for these and other changes that occurred in the welfare workers' union from its inception in 1965 to the early 1980s. He also seeks to elaborate an analytical model of organizational change that incorporates both internal and external developmental influences. The model Schutt proposes focuses attention on changes in the sociopolitical environment that influence an organization's goals, the involvement of its members, and its structure and tactics. Many of the elements of this model will be familiar to students of organizations. However, the

framework that Schutt specifies seems to offer few new insights into the political sociology of organizational development and, at times, fails fully to appreciate the richness of the existing literature in this field.

The real value of this book is to be found in the case study that traces the formation and evolution of the Illinois Union of Social Service Employees. In an enterprising use of multiple methodologies, Schutt, a sociologist, pieces together this story using data from interviews, quantitative survey research, and archival histories.

The union began as the Independent Union of Public Aid Employees, whose members "were just an informal group that got bigger," as one early activist recalls (p. 52). The so-called militants that formed the union were largely comprised of new employees, many of them recent college graduates infused with both the alienation and idealism of the sixties. They pursued conventional union demands for formal recognition of workers' rights but also less conventional demands for institutional responsiveness to the needs of poor families.

These dual concerns were linked in an early statement of goals and principles in which union organizers asserted that "a stable, experienced and trained staff, operating under conditions which permit them to exercise [*sic*] their competence, is prerequisite to the ability of the department to accomplish its stated purpose" (p. 54). They concluded, in a rhetorical flourish, that "should this be the end result of our efforts, we will have witnessed the creation of a new weapon in the increasing struggle for the commonweal" (p. 54).

But in the short space of a year, concern for the "commonweal" had taken a back seat to concern for the survival and growth of the union itself. Union leaders, needing the support of rank-and-file welfare bureaucrats, turned to the "bread-and-butter" issues that directly motivated their constituents. Generally, Schutt finds that the ebb and flow of union membership depended largely on whether workers believed that the union could deliver better wages and working conditions. Rank-and-file members were unimpressed, if not hostile, toward efforts of "militants" to join clients in protesting welfare policies. In this context, the concern for the "commonweal" that motivated some bureaucratic activists in the sixties necessarily took a back

seat.

Here Schutt touches on a theme of central interest to students of social movements and community organizations. As scholars (notably William Gamson, Michael Lipsky, Francis Fox Piven, and Richard Cloward) have documented in a variety of contexts, organizations advancing social causes are often torn between the conflicting demands of institution building and the pursuit of their fundamental political objectives. Apparently, the union of welfare workers in Illinois, like its organizational counterparts elsewhere, resolved that conflict in favor of institution building and conventional union politics.

While Schutt does not always use his data to their full advantage in addressing some of the themes noted here, his case study does provide a valuable glimpse into the activities and motivations of workers in a significant public bureaucracy. For those sufficiently interested to delve into the case data, this book offers a useful perspective on the complexities and tensions that produced, altered, and ultimately attenuated union activism within the welfare bureaucracy.

EVELYN Z. BRODKIN

*University of Chicago*

**The Burger Years: Rights and Wrongs in the Supreme Court, 1969-1986.** Edited by Herman Schwartz (New York: Viking, 1987. xxv, 292p. \$22.95).

If one enjoys well-written liberal critiques of conservative Supreme Court policy, this volume is recommended reading. It contains an introductory essay by the editor, one chapter on the court access decisions of the Burger Court, four chapters on First Amendment rights, three on equality, three on criminal justice, and four on economic regulation. Some of the pieces are written by leading authorities: Yale Kamisar on criminal procedure, Morton H. Halperin on national security and presidential power, and Michael Meltsner on capital punishment, to name a few. Virtually all chapters conclude that the Supreme Court wrongly decided many significant questions of civil liberties and rights between 1969 and 1986. The only contributor who has much to say commending the Burger

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Court is Wendy W. Williams (law professor at Georgetown University), who writes the chapter on sex discrimination.

The book's stated purpose is to examine the "legal soundness" and "real-world impact" of the Burger Court in the selected topics. Additionally, most chapters contrast to a limited extent the policy trends of the Warren and Burger courts and make some general predictions concerning the Rehnquist Court. As would be expected, they typically conclude that during the 17 years under Chief Justice Burger, major strands of Warren Court policy tended to survive—although at times severely battered or altered. The two areas of legal policy identified as experiencing the most dramatic shift to the Right are access to federal courts and national security. As for the Rehnquist Court, the volume's contributors almost always suggest that the nation can expect a continuation of Burger Court trends, with more conservative decisions on some substantive issues.

Generally speaking, the legal analysis of case law and decisional trends is of high quality. Sometimes much more could have been said about Burger Court decisions; the editor undoubtedly imposed fairly strict page limitations on the contributors' manuscripts. Yet one need not have a thorough knowledge of public law or Burger Court trends to understand the volume and to learn about the court's doctrinal tendencies during this period of its history. And unlike many edited collections, *The Burger Years* has coherence as a unit.

Beyond these considerations, the book contains other features that should be of interest to political scientists. For example, it contains a number of references to the policy preferences of the Reagan administration, with suggestions of the extent to which Burger Court decisions were consonant with Reagan's political and legal leanings. Thus, the chapter on separation of church and state by Norman Redlich (dean of the School of Law, New York University) and the chapter on racial discrimination by Haywood Burns (dean for urban and legal programs, City College of New York) will make informative reading for those concerned with the issues of school prayers or affirmative action.

Beware again, however, that this volume reflects somewhat limited scholarly objectivity. Even those with liberal views may question

the accuracy of some of its analysis and conclusions. Although some of the chapters are quite objective, such as the one by Redlich, others are not. To a large extent, the book's subjective tone is a function of those selected to contribute to it. Most contributors have held academic posts at one time or another but are principally known for their active involvement in reform groups, research organizations, public interest law firms, and the like. Such a line-up predictably has its pluses and minuses. The book makes a valuable contribution to the literature on the Burger Court but must be viewed chiefly as a liberal assessment of a conservative era in Supreme Court history.

CHARLES M. LAMB

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**The Right to Life Movement and Third Party Politics.** By Robert J. Spitzer (New York: Greenwood, 1987. xii, 154p. \$29.95).

This slim volume makes a substantial point. Like Rosenstone and colleagues' *Third Parties in America*, Spitzer demonstrates that third parties are not an oddity in U.S. politics but a fairly regular response to failures of the major parties to provide responsive and capable leadership. The book focuses on single-issue parties and, in particular, the New York-based Right to Life party (RTLTP).

Spitzer attempts to do four things: First, he examines four single-issue parties of the nineteenth century (Liberty, Free Soil, Greenback, and Prohibition), in order to generalize about the characteristics of single-issue electoral movements. Next he offers what he terms a "dynamic model of third parties" in general, not limited to single-issue parties. Third, he details the development of New York's RTLTP in the context of that state's electoral rules. And finally, Spitzer argues that a reform of the national two-party system to resemble New York's—a kind of multiparty system with two parties dominant—would stimulate and strengthen U.S. politics.

One of the most interesting parts of the book is that it provides a fine example of the way electoral rules shape the development of political institutions. The rule in question is New York's cross-endorsement provision. Minor parties able to win a position on the

state ballot are permitted to slate candidates who are already running on another party's line. Because a candidate's votes on the minor party ticket are added to his or her major party vote to produce a final count, casting a third-party ballot for a cross-endorsed candidate is not "wasting" a vote. The rule, Spitzer argues, removes a major constraint on third-party voting, helps sustain minor parties, increases their value to many candidates, and expands their influence on the major parties' choice of candidates and issues.

But the book comes closer to being four separate essays whose relationship to one another is not as clear as it should be, than to developing a coherent argument. The author's stated concern is with third-party politics, and that is the subject of his "dynamic model." His case studies, however, involve a subset of third parties: single-issue parties. His reasoning is that single-issue parties are a kind of limiting case: that they are more troublesome and disruptive than other types of minor parties—because they are led by zealots, take extreme positions, and view compromise the way Dracula reacts to the cross—and therefore that if even these troublesome single-issue parties can be shown to have positive effects for the party system, the case for the value of a multi-party system is enhanced. These very characteristics of single-issue parties, however, make them unique among minor parties and also may threaten both their effectiveness and their longevity. Their use as a limiting case, then, needs more justification. The ways in which they resemble, and differ from, other kinds of minor parties should be more fully specified.

The author's plea for the liberation of U.S. politics from "two-partyism," while most intriguing, would also be strengthened by closer connection with the rest of the book. Spitzer contends that the current major parties, while inadequate in many ways, have nevertheless developed a stranglehold on U.S. politics that has prevented the long-awaited (and, he writes, much-needed) realignment from taking place. A set of electoral rules that—like New York's—encouraged third parties could reinvigorate the party system. Reacting to the charge that third parties threaten political stability, Spitzer suggests that "a party system is principally a reflection of the nation's society" (p. 124) and that the U.S. two-party tradition will prevent the fragmentation and in-

stability of some multiparty systems. But this reassurance seems to gloss over the author's own deft illustration, earlier in the book, of the very real impact of electoral rules on political behavior.

The data analysis presented—a survey of RTLP leaders and activists—is quite limited. Further, readers expecting an extended discussion of the abortion issue and the national pro-life movement will find only a brief section; the book concentrates on New York and the RTLP. But these lacks shouldn't erase the book's contribution. As an argument it needs to be fleshed out much more fully. Still, it offers an interesting challenge to our thinking about the impact of a two-party system on the vitality of U.S. politics.

MARJORIE R. HERSHEY

*Indiana University*

**Religion and Politics in the United States.** By Kenneth D. Wald (New York: St. Martin's, 1987. xiv, 301p. \$29.95 cloth; \$10.00 paper).

In *Religion and Politics in the United States*, Kenneth D. Wald describes a tense conversation with a fellow political scientist about current religious issues that abruptly ended when his colleague declared, "I hate religion and politics!" (p. 270). Wald includes the anecdote as an illustration of how the moral and personal dimensions of religion in politics can undermine any semblance of scholarly discourse. The incident also underscores the need for the balanced, carefully crafted text that he has produced for the emerging subfield of religion and U.S. politics.

Wald begins with a documentation and analysis of the persistence of religion in U.S. life as evidence that religion deserves more attention than it has received in the social sciences. (See also J. David Fairbanks, "Religion and Morality in American Political Life," *PS* 19 [1986], 869–71.) This is followed by chapters on the incentives and opportunities available for religious participation in politics, Puritan contributions to U.S. political culture, Supreme Court resolutions to church-state conflicts, four chapters on different aspects of religion and U.S. public policy, and his conclusions. The broad scope of the book is



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supplemented by a lengthy list of references in each chapter attesting to Wald's thorough secondary research. Because it is well organized with useful summaries, the narrative avoids an encyclopedic style.

A primary objective of the book is to dispel the scholarly stereotype "that religion is inevitably a force for conservatism and preservation of the existing social order" (p. xii). Wald accomplishes this goal most effectively in the policy chapters where he first examines General Social Survey data on the political identification and policy positions of six religious groupings. One finding from this exercise is that gains in social status were associated with a shift toward Republicanism for white Protestants; however, gains in status had less effect on prompting conservatism among Catholics and practically no such effect on Jews, black Protestants, and persons without religious affiliation. The author is careful to note that religious affiliation is one independent variable among many; yet he also asserts that relationships with policy positions cannot be dismissed as spurious.

Wald continues his religious diversity theme in two contrasting chapters addressing the mobilization of politically conservative evangelicals and the shift of Catholics and mainline Protestant clergy toward political liberalism. In the latter chapter, one glaring omission is any mention of liberation theology, a theology that is challenging Protestants as well as Catholics to become politically active on behalf of the oppressed. If Wald wishes to demonstrate that religion in politics does not always support the status quo, then discussion of the radical teachings of liberation theology seems warranted. Such gaps are perhaps inevitable in a survey text such as this (for instance, Wald's treatment of the complexities of Puritan covenant theology is questionable in its representation of a God bound by contract).

The author is to be commended for tackling the controversial issue of whether religious activism is beneficial or detrimental for our political system. He critiques numerous attitudinal studies dealing with religion and decides that "the evidence supporting a link between religiosity and the propensity to intolerance is not strong enough to warrant alarm" (p. 272), a decision based on mixed findings in the research. The more sophisticated studies focus

on the *quality* of religious involvement, noting a distinction between those who rigidly follow religious authority and those who have internalized ethical principles of religion into their own attitudes and behavior. Wald reports these findings; but he has no ax to grind regarding the role of religion in politics, and he also cites historical and current examples of religious and political fanaticism. His neutral conclusion is that "an upsurge in religiously based political activity is neither to be welcomed uncritically nor condemned out of hand" (p. 285). Throughout this valuable text, Wald's sensitive, even-handed approach leaves one questioning why political scientists have so long avoided this important topic.

ANNE MOTLEY HALLUM

Stetson University

**Philosophy, "The Federalist," and the Constitution.** By Morton White (New York: Oxford University Press, 1987. xi, 273p. \$29.95).

Presented as the sequel to his influential 1978 *The Philosophy of the American Revolution*, Morton White offers a rigorous, philosophic examination of the main arguments contained in *The Federalist*. Although *The Federalist*, its language and internal logic, is the central focus of White's attention, he skillfully draws on other writings by John Jay, Alexander Hamilton, and James Madison to clarify his position when necessary. White argues that Publius can be understood more fully by reading *The Federalist* "with one strict philosophic eye on the text of Publius and the other on any philosophic text which influenced him when he was writing" (p. 8); the latter approach is particularly relevant since the authors "frequently used philosophic terms such as 'reason,' 'human nature,' 'science,' . . . 'duty,' 'good,' 'passion,' and 'interest' without saying much about *how* they were using them" (p. 4). Most prior scholarship, White claims, has appreciated the role of philosophy in *The Federalist* only in the excessively narrow terms of Publius's theory of human nature or his view of history, failing to recognize that "it contains or relies upon a theory of knowledge, a doctrine of normative ethics, a psychology of motivation, a theory of the causes of

faction . . . and even some metaphysics and theology" (p. 7). This book, then, is a careful, analytic exegesis of the complete philosophy contained in *The Federalist*.

White presents an exhaustive explanation of the relevance of both David Hume and John Locke, "the most important philosophers," to Publius: Hume shaped the authors' "methodology or epistemology of political science," Locke their "ethics and theory of ethical knowledge" (pp. 9, 20, 22). In the process, White engages the seminal work of Douglass Adair and challenges the interpretations of Charles Beard, Arthur O. Lovejoy, and Robert Dahl. The discussion of Adair's understanding of Hume's influence on Madison is particularly instructive; the lengthy analysis of Beard and Dahl, however, borders on being superfluous. White's objections to Beard and Dahl are not in error, but they add little to the contemporary scholarly debate on understanding Publius. One wishes that this talented thinker had devoted his attention to discussing the more recent interpretive efforts generated by Bernard Bailyn, J. G. A. Pocock, and others.

In his discussion of Madison's theory of human motivation, White articulates the significant differences between Hume's and Madison's views of factions. While Madison believed all economic interests constituted factions, Hume denied that "the landed and trading part" of England were factions; similarly, Hume argued that "a skillful legislator" could prevent the rise of factions while Madison "thought that factions based on interest are not preventable in civilized nations" (p. 69). White painstakingly demonstrates that even though Madison was deeply concerned with controlling the effects of factions, one should not jump to the false conclusion that Madison believed all individuals, behaving as individuals, could not be trusted to act justly: "Madison disparaged the motives of individuals acting in *political groups*. I emphasize the phrase 'political groups' because I do not think that Madison held that the hearts or inward parts of all individual men are depraved, corrupt, and filled with evil motives" (p. 95). Rather than holding a bleak or pessimistic picture of human nature, White characterizes Publius's humanity as a darker shade of Humean gray: individuals are neither angels nor beasts.

In this otherwise excellent book that offers a

sophisticated presentation of the philosophy contained in *The Federalist*, there are a few philosophic questions left unexamined. White fails to question the implications of treating the three authors of *The Federalist* as if they were one. Given the eventual rupture between Hamilton and Madison, a discussion of treating Publius as a single, philosophic persona seems warranted. It is also questionable to assume that *The Federalist* presents a complete, or even the best, understanding of the philosophy contained in the Constitution; an examination of the debates at the Constitutional convention and at the state ratifying conventions would be essential to accomplish that monumental task. White also fails adequately to justify his position that Madison did not believe that all political groups are inevitably factious. White's position is contrary to that held by David Epstein (whom White draws upon at other moments) through his careful reading of *The Federalist* (*The Political Theory of "The Federalist,"* 1984, pp. 64-65). Finally, with the notable exception of a powerful discussion of slavery, White does not critically assess the philosophy of *The Federalist*. Perhaps this should be the final step in this influential scholar's illuminating explication of the United States' founding; that is, a critical, philosophic evaluation of the values and dreams of the founders. Although historians may want more historical evidence to substantiate some of his claims and political scientists will want a fuller appreciation of the political realities behind much of Publius's thinking, all serious students of the early republic will find White's book another valuable, clearly written, illuminating contribution to understanding our past.

RICHARD K. MATTHEWS

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**Capital Punishment and the American Agenda.** By Franklin E. Zimring and Gordon Hawkins (New York: Cambridge University Press, 1987. xviii, 192p. \$22.95).

Although those who support the abolition of the death penalty in the United States will find little to cheer about in the recent *McCleskey* decision of the Supreme Court, they may find some comfort in this combination of careful

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scholarship and passionate argument produced by Zimring and Hawkins. The book focuses our attention on issues that social scientists raise and the legal profession has too often ignored. What factors were associated with abolition in European countries? What political changes might lead to its ultimate disappearance in this country? The role of public opinion in countries where abolition has been accomplished, the relationship of *de facto* and *de jure* abolition, and the off-again-on-again pattern in many European jurisdictions—these are all factors that might have enhanced the debate in the *McCleskey* case and that the authors use to build a case that the United States “will be catching up with itself” when it abolishes capital punishment (p. 165).

Some of their points are not new but are forcefully made: “It is no coincidence that the list of actively executing countries matches that of politically repressive countries” (p. 6), a fact that Amnesty International has noted in its current campaign for abolition. One of the most interesting sections examines the questions raised, especially by Justice Blackmun, in the *Furman* opinion and notes how the other justices failed to respond directly and forcefully in a way that might have shaped future Court decisions (pp. 50–73).

Their summary of the evidence against deterrence is well done but, as they note, probably beside the point as far as the debate itself is concerned. If, however, the death penalty does indeed serve primarily as symbol and its supporters are guided more by “faith” than “empirical evidence” or “ethical argument” (p. 19), then two conclusions follow. The first is that reasoned debate such as this book might

spark will be useless. The second is that a change in political leadership is a necessary precondition for a policy change.

In predicting the future they are cautious for the short run and seem to shy away from direct political predictions. Certainly, they note that the role of the Supreme Court is both complicated and critical (p. 102). Yet what many feel would be the strongest predictor of change is mentioned only once, on page 157, when they note, “Developments in abeyance for the duration of the Reagan Court might quickly have come to pass during a Mondale administration.” In sum, who is in the White House decides who is on the Court that decides the future of capital punishment. Especially if, as they suggest, elites will have to go ahead of public opinion on this issue, who wins national elections may well be the critical variable.

This book ought to be read by every thoughtful advocate of change in our system of punishment. It not only makes a strong (if not unique) case for abolition of the death penalty, but it also indicates many of the political factors that have been at work to continue death sentences. For example, understanding that states’ rights rather than law and order may well be the key to interpreting post-*Furman* response (p. 44) can sharpen the focus needed to bring about real change. As a resident of the state that executed Velma Barfield (pp. 103–4) while political leadership allowed savage public behavior to direct public policy, I hope this volume receives the attention it deserves.

MARY THORNBERRY

Davidson College

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## COMPARATIVE POLITICS

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### **Military Power and Politics in Black Africa.**

Edited by Simon Baynham (New York: St. Martin's, 1986. 333p. \$32.50).

In the words of the editor, this book “aims to provide a comprehensive overview of military power in black Africa, from the period since independence but with con-

siderable emphasis on quite recent and contemporary events.” The book accomplishes its aim. While some of the contributions are uneven, the totality of the scholarship is valuable in understanding the broad dimensions and substance of the role of the military in black Africa. Equally important, the contributors represent a wide range of experience

in African matters and most have a wide reputation as scholars of the first order.

The book is divided into three parts: an introduction, a part on the internal influence of the military, and a part on the international dimensions of military force in black Africa. The scope of the subject makes the book susceptible to criticism for its selection of topics and balance. Much of this criticism seems to be anticipated by Baynham's excellent and perceptive introduction. Not only does he highlight and summarize each contribution, but he provides his own valuable interpretation and assessment of the subject matter.

In the first part, Samuel Decalo, Claude E. Welch, David Goldsworthy, and Annette Seegers provide excellent chapters in examining the internal influence of the military. These range from Decalo's examination of the behavioral motivations of soldiers who become involved in political action and Claude Welch's perceptive study of military disengagement from politics to David Goldsworthy's examination of various views regarding the impact of military coups, military rule, and the idea of the "inevitability of instability" and Annette Seegers' insights into a much ignored subject, the evolution and character of armies of revolutionary states, specifically Mozambique and Zimbabwe. Aside from their own focus, the general thrust of the contributions conclude on a pessimistic note regarding the likelihood of continuing military rule, even when such rule is not a great deal different from civilian rule.

The second part of the book begins with another valuable contribution by Dennis Austin on the use of force to protect and perpetuate colonial boundaries in Africa. This is followed by contributions by Arnold Hughes and Roy May, Anthony Clayton, Abbott A. Brayton, and J. E. Spence. Hughes and May study the use of African armies as instruments of foreign policy. In general support of their views, Clayton shows the impact of the many foreign military interventions in African states. Brayton probes these matters in the context of arms control and concludes that among African states there is military restraint with effective arms control—a possibility only by the cooperation of major external powers. Finally, Spence analyzes the military in South Africa, arguing that its short-term interventionist policy in neighboring states has proven

successful, but that such a policy is not likely to succeed in the future.

Contrary to the standard menu for reviewers of edited volumes, it is the view here that contributions should be assessed on two levels: their relevance to the purpose of the book and whether each contribution can stand on its own. Although one may disagree with some of the conclusions and assessments of individual contributors, the fact is that each has provided a scholarly study in its own right. Yet at another level, the book would have been more balanced had more attention been given to the anti-Marxist revolutionary military that has emerged in Angola, Mozambique, and Ethiopia, for example. Additionally, the study of external power involvement seems somewhat superficial in its treatment of the United States. Further, while some attention was given to why there are no coups in certain states, a much more comprehensive treatment of that subject is needed in a volume on the military in Africa. Finally, although scholarly objectivity requires some degree of circumspection, it would have been challenging to readers if more of the contributors had speculated on future developments, for instance, in South Africa. These minor criticisms aside, this collection is a valuable contribution to the literature.

SAM C. SARKESIAN

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**Interpreting the Third World: Politics, Economics, and Social Issues.** By Jacqueline A. Braveboy-Wagner (New York: Praeger, 1986. ix, 357p. \$39.95).

To the question she poses, "Is there a Third World?" the author responds, "definitely yes" (p. 347), and in this text for undergraduates she examines in 10 chapters and a short conclusion the major characteristics of the Third World through three lenses: colonialism, modernization, and development. The result is a survey that covers much familiar ground but is somewhat unusual in the emphasis accorded different topics.

If there is a Third World, the question is how to get students from the First World, with all their readily identifiable cultural traits and biases, to engage in serious, sustained study of

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it. In both geographical and topical terms, as the subtitle of the work indicates, the Third World is a vast and varied subject. Within the literature on the Third World are examples of a number of approaches, each of which attempts to persuade students to shed their ethnocentric blinders and observe closely the other settlements in our global village. One approach, represented by Paul Harrison in his *Inside the Third World*, attempts to involve students emotionally and intellectually with frequent references to the conditions of ordinary people living in Third World countries, in circumstances very different from the students' own. A quite different approach is used by Braveboy-Wagner, who concentrates on the big picture, complete with many tables of national data of representative Third World states, with only the briefest references to the lives of ordinary people. These national data are loosely evaluated within the context of current major theories in an effort to introduce students to the theorists and their writings in the field.

According to Braveboy-Wagner, the Third World consists of those states that in general share the characteristics of having been colonies of Western powers, are technologically underdeveloped, are resistant to Western values, and are unwilling to align themselves with one of the superpowers. Having defined the Third World states, the author opens the first major section of her study with a discussion of colonialism and its aftermath. In contrast with most studies, which first examine the impact of colonial rule on the social, political, and economic structures of Third World societies, the author emphasizes the "international dimensions of nationalism" and the attempts by newly independent Third World states to create international cooperative movements such as the pan-Arab, pan-Islamic, and pan-African ones, on the grounds that "the Third World has institutionalized its cooperation in such a way that fundamental changes and agreements can be achieved only by dealing with the whole" (p. 348), a conclusion which many may find premature. Examining another aspect of the political aftermath of colonialism, that of nationalist conflict, the emphasis is again on its international dimensions, such as irredentist disputes and superpower interventions, rather than on the domestic economic, social, and cultural conse-

quences of colonialism.

In the second half of the book, the distinctive but often related issues of *modernization* and *development* are considered, the discussion being shaped by general references to theorists like Parsons and Weber and their critics. Again, the stress is on the international aspects of development, such as international aid, rather than on the domestic, underscoring the author's commitment to discuss the "difficulties facing these countries and . . . the responsibility the international community has in helping the Third World resolve them" (p. 349).

Students using this text will have much useful data to consider, but they will not encounter illustrative cases of the consequences of hunger or illiteracy on specific individuals, or of the throbbing, jostling life of the slums. Interpreting the Third World is accomplished in this text through aggregate data, and beginning students are unlikely to experience much of an emotional response to Third World conditions. They will, however, have some useful data to ponder and will have been introduced to some of the insights of theorists with which to begin their own interpretation of the Third World.

HARVEY G. KEBSCHULL

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**Israel: Building a New Society.** By Daniel J. Elazar (Bloomington: Indiana University Press, 1986. xi, 287p. \$29.95).

In the prolific output of Daniel J. Elazar certain themes have often been present: federalism and intergovernmental relations, local government, covenants as the basis of law and politics, the concept of political culture as a methodological tool, the mixture of traditional and new elements in emerging societies, and a devotion to constitutional principles. In his new book, some of which is familiar from earlier essays, Elazar uses these themes in his rigorous, sophisticated examination of the factors relevant to the creation and development of Israel. But though Israel can be usefully studied within the context of comparative politics as an example of a newly emergent political system, it is also, as Elazar suggests, unique because of its link to the Jewish people.

Early theories of modernization seemed to suggest that an increasing separation of church and state would accompany development. Empirical evidence from many countries has belied such a simple relationship. Israel provides a particular example of this by its increasing concern about its Jewish authenticity and the search by part of its population to link the state to appropriate forms of Jewish religious expression. In his own concern to link contemporary and ancient Israel, Elazar propounds a somewhat strained parallel between groups—the Saducees, Pharisees, and Essenes in Biblical times, and their contemporary approximations: secular Zionism and a civil religion, the Orthodox who value Jewish religious norms, and the communal kibbutz.

Though sometimes repetitive, Elazar, in this thoughtful and engaging study, paints a distinctively original portrait of the nature of Israeli society. He constantly presents his empirical analysis in the context of an elegant and often challenging theoretical structure. He also implicitly argues a case—based on a number of his favorite themes—that might be termed intellectual Sephardism at its best.

Throughout his book Elazar stresses the significance of federal principles in both Jewish affairs—the World Zionist Organization and early Zionist societies—and in Israeli society, from the covenants of the colonies in the first Aliyah at the end of the nineteenth century through the kibbutzim and the linkage of moshavim for cooperative purposes to the accommodation of the various groups to reach a working relationship. The three major Zionist camps—Labor, civil and religious—existing in a triangular relationship to each other, have led to what Elazar calls “a compound republic” rather than an organic state. Israel has been built by highly autonomous elements that surrendered a minimum of their power to a central authority. Paralleling this is the tension between *mamlachiut* (strengthening of state institutions), and *halutzit* (the pioneering spirit or voluntary cooperation).

Amplifying the familiar terminology of Almond and Verba, Elazar discusses the emergence of an Israeli political culture out of the three existing cultures; the subject culture of many Jews, with government seen as malevolent and limited concept of political participation; the statist culture of modernizers who see the state as an instrument for social change;

and the traditional Jewish culture, civic and republican, in which God is sovereign and rulers are bound both by the Torah and by popular consent.

Contemporary Israel is an amalgam of all three strands; the first now represented largely in Arab villages whose inhabitants are instructed how to vote by clan leaders, the second shown in the bureaucracy, and the third—seeing power as federal and the result of a series of covenants—is reflected in the Israel Defense Force. But it is not always clear from Elazar’s analysis how the emergent political culture will embody both the sense of national unity required by both security and Jewish isolation and a common vocation of building Israel, in which the federal or contractual element resulting from the amalgam of kinship and consent must be an important part. His view that the Israeli system has shifted from ideological democracy (based on doctrinal faithfulness) and sectoral groupings to territorial democracy is not wholly borne out by the complex mixture of traditionalism and civil society, and the unresolved tension between the concept of Israel as a Jewish state and the view that it is to be a light unto the nations.

Perhaps the most interesting—because least familiar—pages of the book are those on local government and politics. Local government has been important not only in providing and administering governmental services but also as a base for recruitment of political leadership, for fostering channels of communication between rulers and ruled, and for maintaining diversity in a small country. Elazar sees local affairs as the likely direction for future political life. The number of settlements enjoying municipal status has increased as have both the number of regional councils and the Arab and Druze villages with modern municipal governments. If the new state, between 1948 and the early 1960s, absorbed functions once in the hands of local, voluntary, or party bodies, it is now experiencing a trend to decentralization, partly because of the great growth in the number and size of cities, partly because of the pattern of more independent voting in local elections, and partly because of the emergence of a local leadership, often from groups otherwise largely excluded from politics.

It is on this last factor, the unduly neglected significance of the Sephardim, that Elazar is most distinctive. He tries to dispel the myth of

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two Jewish cultures and argues, at times in polemical fashion, that the Ashkenazim did not have a monopoly of Zionist pioneering and that the Sephardic cultural heritage is consonant with modernization. He denies that the Sephardim are more aggressive, more anti-democratic, and more hard-line on occupied Arab territory than the Ashkenazim. Differences in the levels of modernization of the two groups he sees as largely the consequence of inequality of opportunity in education, in occupation, and in economic matters like receipt of German reparations. By the late 1950s Sephardim obtained leading positions in local affairs, in the development of towns and rural settlements. On the national level, they moved to the Likud, which, unlike the Labor camp, welcomed them. Elazar extrapolates from this useful and novel analysis a controversial conclusion that the Likud is likely to become and remain the majority party in the country. Notwithstanding one's views on this particular issue, all students of Israeli politics will be grateful for and benefit from the acuteness of analysis, the penetrating shrewdness, and the commanding sweep of this book.

MICHAEL CURTIS

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**The Making of a Liberal Democracy: Senegal's Passive Revolution, 1975-1985.** By Robert Fatton, Jr. (Boulder: Lynne Rienner, 1987. ix, 189p. \$25.00).

Senegal is a country plagued by grave and deepening poverty, mired in economic stagnation, with a government in thrall to international creditors holding it to a policy of severe austerity. It is also a functioning liberal democracy with a dozen active opposition parties. The leader of the strongest one in recent years has rivaled the president in popularity and some of the others advocate revolutionary socialist policies and sharply criticize the neo-colonial subservience and capitalist direction of the current government. How can liberal democratic practices be established in such a setting? Could other poor and economically troubled countries achieve similar democratic openings? What difference does democracy make, given the failure of the rulers of independent Senegal to prevent the deepening

impoverishment of the majority of the people?

Robert Fatton, Jr. turns to Gramsci and Poulantzas for the concepts with which to address these important questions. Some of his answers will be surprising to those who accept the widespread view that poverty, traditionalism, peasant economies of affection, IMF adjustment measures, entrenched class cleavages, patron-clientism, and statist measures to develop industry and agriculture all militate against democratic practices. Fatton holds "not only that liberal democracy is possible in underdeveloped societies, but that it might present the best vehicle for the persistence of ruling class power" (p. 18). His analysis is stimulating and interesting for its theoretical imagination as well as for its perspective on Senegal.

In the late 1960s—with peasants blaming the government for diverting peanut earnings to feed the bureaucracy while the producers were going hungry, labor unions chaffing at the declining real value of the minimum wage, Senegalese business leaders pushing for a better deal in relation to French capital, and university students and other intellectuals outraged by the continuing power of the French presence—the country entered into a period of organic crisis. Fatton here makes excellent use of the existing studies of Senegalese society and politics. The ruling class, he shows, lost its capacity to impose leadership, while the lower classes were too divided and disorganized to mount an effective political challenge. But the rulers were strong enough and sufficiently skillful to elaborate and to carry through a passive revolution: *passive* because "although authoritarianism was replaced by the politics of hegemony, the structures of power remained fundamentally unchanged" (p. 53).

One key to the transition (and here Fatton brings a new variable into the equation) was the work done by a set of organic intellectuals, the "managers of legitimation" (p. 64) whose job it was to convince the lower classes that the politics of hierarchy was in the general interest. Another was the political skill of presidents Senghor and Diouf and the political elite of the country. A third was the largely complementary hegemonic power of the Muslim marabouts over the peasantry. The fourth element was the persistence of patron-client politics as a form of "coercive dependence" (p. 102) limiting class conflict in a class-divided

society. Fifth was the divided and inchoate nature of the challenge posed by the revolutionary Left. And finally, the passive revolution was underwritten by the French military presence ready to defend the existing class alignments. This combination may make Senegal an exceptional case rather than a model for other peripheral societies; but Fatton seems uncertain about this, perhaps because his method eschews measures of the strength of social forces and stresses the existence of dialectical alternatives.

There are other ambiguities. It is not entirely clear what groups belong to the "ruling class." Nor are the reach and impact of the writings of the "organic intellectuals" plumbed. Are they constructing a powerful hegemonic ideology, or might they simply be currying favor with those in power? Popular political discourse in Senegal at all social levels takes place in Wolof, not French. It is a spicy mix of personal and policy, economic, and symbolic matters. How this popular discourse might relate to hegemony receives no direct attention in this book.

The analysis permits contradictory conclusions and Fatton does not shrink from drawing them. The passive revolution establishing liberal democracy "embodies . . . the death of fundamental change" (p. 165), but it also "represents a still fragile foundation on which more profound democratic values and practices may be erected" (p. 170). It "has curbed the intensity of opposition . . . and blurred the horizon of options" (p. 166), but "it has also unfolded a dynamic of reforms that may generate among subaltern classes a momentum for more radical demands" (p. 169).

Despite some imprecisions, ambiguities, and gaps, both the form and the content of Fatton's analysis are helpful and stimulating both for the study of Senegal and for the study of democracy. It has broken a trail others are sure to follow.

JONATHAN BARKER

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**State and Countryside: Development Policy and Agrarian Politics in Latin America.** By Merilee S. Grindle (Baltimore: Johns Hopkins University Press, 1986. ix, 255p. \$25.00 cloth; \$11.50 paper).

**The Transformation of Mexican Agriculture: International Structure and the Politics of Rural Change.** By Steven E. Sanderson (Princeton: Princeton University Press, 1986. vii, 324p. \$42.00 cloth; \$10.95 paper).

These books are significant both for their substantive findings about trends in agriculture in Latin America and for the theoretical issues they address. The authors generally agree on substantive trends, but they diverge interestingly with respect to how to characterize the state and interpret its role in agrarian politics. Sanderson's approach ("new international division of labor") tends to depict the state as a homogeneous actor with a single motivating logic and to relegate the state to a marginal role, essentially in facilitating international market forces. Grindle assigns greater importance to the politics and complex rationales that influence state action in agrarian policy, but she reaches similar conclusions about the consequences of state behavior.

With respect to substance, the books demonstrate how agriculture has played a subordinate role in import-substituting industrialization in the Latin American region since the early 1950s. In brief, population growth, urbanization, rising incomes, and international trade patterns have reinforced market forces in the countryside such that cattle, sorghum, oilseeds, and other tradeable goods have benefited, to the relative neglect of food staples (e.g., black beans, rice, maize) traditionally consumed by lower-income and marginalized groups. This has implied greater competition for scarce, arable land and increasing misery of the rural poor. The thrust of government policy has facilitated resource flows to commercial agriculture. At the same time, governments have sponsored reform programs first to distribute land, then to develop selected regions, and finally to integrate the rural poor in more comprehensive programs to raise productivity and living standards. These reform efforts have delivered limited benefits to the poor, but have proven useful in complex state strategies of cooptation and repression.

Sanderson's book begins with a theoretical



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chapter that reviews current approaches to the study of development and adopts one that implies "a domination by trade relations and by the transnational integration of production itself, not in the context of empire, but through the medium of the less nation-bound internationalization of productive capital" (pp. 19-20). This is followed by chapters on the more significant traded commodities (fresh fruit and vegetables; the frontier beef industry; and basic grains) and a discussion of the shifting logic of state intervention in Mexican agriculture. The strengths of the discussion are clearly in the treatment of fruit and vegetables and beef. Here Sanderson's impressive mastery of agronomy, economics, and trade patterns and his familiarity with the U.S.-Mexican border economy are brought to bear to support his thesis that market forces tend to overcome whatever ameliorative projects the state might undertake.

Though still solid, Sanderson's treatment of basic-staples agriculture and the complex government apparatus involved in production and distribution seems less surefooted. And his discussion of the Mexican Food System (SAM) suffers from a neglect of the complexity of the politics of government policy-making and bureaucratic infighting. This is due less to lack of knowledge (given the recency of the program) than to the depiction of the state as a unitary actor and an "argument [that] follows a more parsimonious concept allowing the state to follow the economic logic of the age: rational self-interest" (p. 234). Nevertheless, Sanderson's conclusion constitutes an important hypothesis: "The separation of production increases from agrarian reform leads to a highly undesirable effect: the state's modes of intervention in the agricultural economy . . . have been shown to *enhance* the internationalization of the Mexican agricultural system, not to challenge it" (pp. 280-81).

Grindle's book casts a broader comparative net to include specific case studies of Mexico, Colombia, and Brazil, with a thorough grounding in the pertinent literature on other Latin American countries. She sets out to challenge the important contribution by Alain de Janvry, *The Agrarian Question and Reformism in Latin America* (Baltimore: Johns Hopkins University Press, 1981)—which stresses the logic of capitalist accumulation in the periphery—because of her belief that "state

elites have a variable capacity for autonomous decision making and often have specific interests in national development that cause the state to become active in shaping economic and social relationships with dominant-class interests in society" (pp. 3-4). Grindle also has a distinctive view of *reformist policies*, referring "only to efforts by the state to address problems of rural poverty, exploitation, and inequality. General policies for agricultural modernization are thus not considered reformist" (p. 195).

Grindle rejects both neo-Marxist and dependency approaches in favor of the Weberian tradition and a view of the state that "as an executive and administrative apparatus for decision making and control, has identifiable and concrete concerns about the definition and pursuit of 'national development.' These concerns are independent of, but not necessarily opposed to or different from, the immediate interests or welfare of any particular group, class, class fraction, coalition, or alliance in society" (p. 17). This perspective allows her to focus on political factors, such as ideology, leadership, bureaucratic complexity, and policy implementation. Further, the state is assumed to be neither autonomous nor homogeneous: "The relative autonomy of the state is assumed to be a desirable condition sought after by state elites insofar as it presents them with greater flexibility" (p. 18).

The discussion of theory is followed by chapters on agrarian history, the state and the ideology of modernization, patterns of commercialization of agriculture, survival strategies of the rural poor, agrarian-reform ideology and politics, and the "New Dualism" of integrated rural-development programs. Her conclusion pulls together her arguments that agrarian-reform projects are products of ideological commitments conceived in complex state bureaucracies and that the programs have been significantly reshaped by interest politics in the course of implementation. Her data and findings essentially reinforce those of Sanderson. But her approach to the state leads to different expectations. Grindle finds, for example, that "even state intervention often entrapped smallholders in market relations that caused a loss of security and control over productive decisions" (p. 125). Sanderson would have introduced the same remark with *of course*.

Both authors make significant contributions, which makes the theoretical dialogue even more impelling. Sanderson would guide us to the international division of labor, concluding that the state is an insignificant actor, one limited to working out the details of exploitation: "In a word, the globalization of the Mexican food system has left the national state without competent mechanisms to govern rural growth and development" (p. 275). Grindle would agree that state intervention has largely advantaged the wealthier, commercialized farm interests; but her approach holds out the possibility of greater state independence and the possibility of more complex and nuanced government behavior.

JOHN BAILEY

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**Elections in Independent Africa.** Edited by Fred M. Hayward (Boulder: Westview, 1987. xvii, 318p. \$23.85).

This is a volume in the Westview series, "Special Studies on Africa," composed of an introduction and conclusion by the editor, and eight country studies. The cases examined are Sierra Leone, Ghana, Nigeria, Botswana, Tanzania, Zaire, Kenya, and Senegal. Like the editor, many of the authors have been affiliated with the African studies program at the University of Wisconsin.

This is clearly one of the most important and innovative works on African politics in recent years. It is important because of its continent-wide perspective on a recently neglected aspect of African political processes. More significant, however, is the potential contribution it makes to our understanding of the function of elections generally. The case needs to be made more often that Africa is a rich laboratory in which to study the variety of human political experience; this book can contribute powerfully to reducing parochialism in comparative politics.

Area specialists often find it difficult to write about their region for a general audience. The specialist often unwittingly limits the impact of a case study by making assumptions about the audience's prior knowledge of local context. These authors have not entirely succeeded in overcoming this problem, and it is unfortunate

that the studies with the best summary descriptions of political history are those on Nigeria and Zaire, already the best known to a general audience. Still, the chapter introductions are generally sufficient to make the analyses comprehensible, and they carry the reader to a rich testing-field of major propositions on electoral politics advanced in Dahl and Huntington's classics.

These descriptions document the presence in most African traditions of the expectation that people will have some measure of control over political authority. Although the electoral process was itself new to those traditions, it was easily accepted in the context of decolonization. With independence, many leaders saw elections to be threats to their power and took steps to eliminate them as effective measures of participation; yet there remains "a powerful desire for mass participation in Africa" (p. 281).

Hayward notes that in only one of the cases examined (Sierra Leone) has Africa seen an election result in a change of government. The centrality of elections in African politics clearly diminished after independence. Furthermore, the sample of countries examined here is clearly biased toward the minority of countries in which the electoral tradition is strongest. Still, there is powerful evidence in this work of the need in many countries for electoral legitimation and of the degree to which leaders are pushed to make the electoral process credible. It is unwarranted to dismiss elections as irrelevant in the African context when we unthinkingly accept the validity of the process in American and British history. The pervasiveness of the patron-client relationship in these African countries would bear comparative examination with nineteenth century voting patterns in the advanced Anglo-Saxon models. The one-party dominant regimes in Botswana, Senegal, and Kenya could be closely compared not only to that of Mexico but to Japan and its Liberal-Democratic Party. Mass-elite linkages are invigorated by elections in strikingly similar fashion in all these cases.

Hayward imbeds in his conclusion the assertion that "African experience with elections over the past decades has much to suggest that enriches our understanding of the political process" (p. 280). This theme should be given more prominence, for *Elections in Independent*

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*Africa* has much to offer in developing our knowledge of the evolution of electoral systems and in fitting that knowledge to major theoretical writing on the subject.

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**The Kuna Gathering: Contemporary Village Politics in Panama.** By James Howe. Latin American Monographs, no. 67 (Austin: University of Texas Press, 1986. xvi, 326p. \$27.50).

The 30 to 35 thousand San Blas Kuna inhabit numerous small near-shore coral islands along Panama's northeastern Caribbean coast. To the anthropologist, the Kuna are a fascinating people for a variety of reasons: they are relatively recent migrants to their island environment (and thus yield important data for various studies of adaptation), they are one of the few twentieth century indigenous success stories, maintaining a remarkable degree of autonomy within the confines of a nation-state (aided by a successful revolution—with a little help—in 1925), and they are an intensely political people. The Kuna have been the focus of numerous ethnographic studies, particularly since the late 1960s.

James Howe has been conducting research among the Kuna since 1970. His interests in the Kuna (reflected in his publications) extend well beyond political topics to include kinship, ritual, mythology, cosmology, oral literature, subsistence, ethnosemantics, and ethnohistory. In his own view, one amply supported by the content of the book under review, "almost all [of these topics] have political implications and have contributed in important ways to my understanding of the social and cultural context of Kuna politics" (p. 264). In *The Kuna Gathering*, he provides us with the first full-length account of Kuna local village politics. His account is lucid, readable, and theoretically informed. Howe avoids rigid adherence to any particular theoretical viewpoint, but this is not to say that the study is atheoretical. Rather, it highlights the remarkable extent to which general models may require revision in the face of empirical data. Each standard theoretical lens through which Kuna politics is

viewed leaves it anywhere from slightly to considerably out of focus.

His methods of data gathering include many of those deemed standard in anthropological research: participant observation, structured and unstructured interviews, listening to and recording conversations, paying informants to keep diaries, taking censuses, copying documents, and the administration of a standard comprehensive questionnaire—in this case on political organization—to informants from as many villages as possible. (Details of his field research are presented in an appendix.)

Most of the book is set in the decade from 1970 to 1980, but enough historical information is presented (in an introductory chapter) to assist understanding of how things got to be the way they are, with considerable attention given to significant changes and to the adaptability of the system. Sufficient contextual description is provided throughout the book to permit discrimination between general features of Kuna political behavior at the village level and their contextually dependent variants.

The core event of Kuna politics is the *gathering*, an almost nightly affair in many villages. The gathering has two modes: the sacred, singing gathering and the secular, talking gathering. Both play important—and distinctive—roles in politics and public life. The traditional core hierarchy of elective offices in both forms of gathering are those of chief, *arkar* (spokesman or interpreter), and policeman. Today in many villages political offices have proliferated but all officeholders must report to the talking gathering and abide by the collective decisions made there. Howe focuses in this study on the talking gathering but does devote two chapters to the singing gathering, one a description of a typical singing gathering and the other an analysis of the significance of the sacred gathering complex.

The singing gathering provides at the village level "the social and symbolic foundation for more directly political action" (p. 78). At its core is Father's Way, the collection of all the Ways or Paths (e.g., stories of the spirit world, cosmology, history, morality tales, and contemporary events and narratives), which the chiefs chant and the *arkars* interpret, filled as they are with metaphor, while policemen exhort the audience to stay awake. Howe argues that Father's Way is, among other things, a political constitution in metaphors.

Eight chapters are devoted to various facets of the secular, talking gathering, the scene of direct political action. At this secular gathering, the political aspects of matters of concern to the village are discussed and decisions made.

After outlining the characteristics of Herbert Lewis's tripartite typology of leadership in prestate and early state societies—*free enterprise*, *kin-based*, and *republican*—Howe demonstrates in two chapters, one focused on chieftainship and the other more generally on public office, that the Kuna political system closely approximates Lewis's republican type. Attention is also given to F. G. Bailey's conceptualization of *support* as essentially a dyadic relationship between leader and follower. Howe shows that this does not fit the Kuna case, where "support consists more of a decision about a leader, and less of personal allegiance to him. . . , [where] each voter decides between candidates in terms of a personal calculus [rather than] a model of personal followership" (p. 102). Several other features of public office are also examined in detail.

One chapter is devoted to a presentation of a representative evening at a talking gathering. The focus is on speech events and the variety of functions that they serve, principal among them being *informing* (expected of a chief and regularly done), *admonishing* (regarded as a primary task of leaders, especially of chiefs), and *adjudicating* of trouble cases.

A chapter devoted to public works is concerned principally with communal labor. Howe is considerably more optimistic regarding its future in Kuna villages than is Holloman, another Kuna specialist. Both agree that there is considerable stress on the system. Disagreement over communal labor tasks has caused village fissioning in at least four recorded cases. Holloman assumes that the system functioned more smoothly in the past, an assumption contradicted by Howe's ethno-historical data. Howe proposes "that disagreement, resentment, heated discussion, and even occasional schismatic conflict be considered normal parts of village task organization. . . the inevitable miscommunication, mistakes, unfairness, and breakdowns in communal labor inspire considerable dissatisfaction among the Kuna. Neither this unhappiness nor the Kuna propensity to pessimistic hand wringing should blind us to their overall suc-

cess at getting things done" (p. 149). Howe's position is supported by the fact that close to 20 years have passed since Holloman's research and there has been no noticeable collapse of the system of communal labor. In fact, the system seems remarkable in its capacity to incorporate additional tasks.

Four chapters examine—among other topics—the distribution of influence (Howe prefers this term to *power*) and patterns of alignment; political action and process and the role of outsiders in village politics; the political aspects of dispute and trouble-case adjudication; and conflict and village fission. A final chapter discusses three problematic theoretical issues in the light of Kuna politics: influence and leadership, political culture and contradiction, and democracy. Throughout the book, Howe's analyses are careful and stick close to the facts of the matter, and his arguments are thoughtful and thought-provoking. Given space limitations, I shall comment on patterns of alignment and process only.

Howe takes the position that the standard concepts—interest group, following, faction—are of little utility in analyzing Kuna patterns of alignment. He notes that "a key principle of Kuna politics is that each man acts in the gathering house primarily as an individual" (p. 185) with the result that there is tremendous variation in alignments; that is, alignments are issue-specific. Nonetheless, general viewpoints or orientations held by individuals may result in more consistent alignments than would be predicted by a strictly issue-specific model. In the Kuna case, traditionalist and modernist orientations bear directly on issues involving innovative change, and individuals are more likely than not to be consistent in holding to these orientations. This may very well produce more regularity in patterns of individual alignment than would be apparent from an examination of issues one by one.

Howe portrays the dominant process models in political anthropology as characterized by conflict and competition, as a basically antagonistic game consisting of a series of intermittent episodes interspersed by periods during which the protagonists marshal their forces. He does not argue that such episodic conflict does not occur among the Kuna but rather that Kuna politics cannot be understood as a series of discrete episodes. Thus, while his overall orientation is one of process and there

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is attention to conflict and its consequences, Howe's Kuna model attends as much to harmony as to conflict, as much to continuity as to periodicity, and as much to outcomes as to the processes by which these are achieved. Howe states that standard process models neither comfortably fit, nor tell us very much about, Kuna politics. He contends that insufficient weight has been given in current process models to the "verbal nature of politics" (p. 196). He convincingly uses a single case to display the broad range of near universal kinds of argumentation employed by the Kuna in striving for consensus through persuasive argument in their gatherings. That they often fall short of this ideal and achieve only grudging acquiescence or majority rule goes almost without saying. But as Howe correctly notes, it is the collective nature of Kuna decision making that deserves emphasis, not the fact that the process may not always result in consensus.

Overall, in *The Kuna Gathering*, Howe provides us with a vivid, largely qualitative, microview of Kuna village-level politics while at the same time judiciously attending to a broad range of issues of current concern in political anthropology and political science. The book is a valuable contribution to the study of politics in small-scale societies—and a pleasure to read.

PHILIP D. YOUNG

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### **Reagan, Thatcher, and the Politics of Decline.**

By Joel Krieger (New York: Oxford University Press, 1986. viii, 247p. \$29.95).

This is an ambitious book about an important subject. The *decline* in Krieger's title really turns out to refer to several distinct, though related, developments: the declining competitiveness in world markets of both the British and U.S. economies, the decline of an international order marked by U.S. hegemony, the decline of a set of institutions and policies dubbed by Krieger the *Keynesian Welfare State*, and the decline of the political coalitions supporting that Keynesian Welfare State. These changes plainly affected all advanced capitalist nations. Krieger's concern is to show that they produced a distinctive political re-

sponse in the United States and the United Kingdom, to trace the common elements of that response and to suggest how and why Reaganism and Thatcherism nevertheless differ in key respects.

This ambitious task is accomplished in a quite straightforward way. Krieger begins with pictures of the "Calvary" on which the Callaghan and the Carter administrations were crucified at the end of the 1970s—economic stagnation, labor unrest, and military failures. He sketches (chap. 2) the rise and fall of the Labour coalition that was so closely associated with the development of the Keynesian Welfare State and performs a similar analysis in tracing the decline of established ideologies of Conservatism and their displacement by Thatcherism. This is succeeded by a sketch of the changing ideological and structural setting of British politics in the 1970s, and of Thatcherism's exploitation of the resulting divisions within electorates. The focus of the U.S. chapters is more internationalist—on U.S. imperial decline, on the challenge of foreign economic competition, and on how structural change means that a "Sunbelt consensus has emerged to replace the old Keynesian consensus" (p. 143).

The book's main thesis is manifestly true—"that when the generalized economic crisis of the 1970s beset Western Europe and the United States, in the United Kingdom and the U.S. in particular, neo-conservatives and new right forces would be better positioned to take advantage of the political-electoral openings" (p. 22). What is distinctive is the racy and provocative way in which the thesis is elaborated.

First, Krieger draws an excessively clear distinction between the kinds of coalitions supporting the Keynesian Welfare State and those supporting Reaganism and Thatcherism. On the one hand, the Keynesian coalition is pictured as resting on a "historic vision of a society motivated by social justice" (p. 110); on the other, new coalitions are pictured as the creation of social division and mere political arithmetic. But this neglects the key point about the Keynesian Welfare State—that it, too, rested on alliances that were instrumental in character and that it, too, defended some sectional interests at the expense of others. Both the Keynesian Welfare State and its successors are based on the arithmetic of sectionalism.

Second, Krieger seems to me grossly to overstate the magnitude of the change that has occurred, at least in the case of Britain. This is nowhere clearer than in his treatment of non-white immigrants and their descendants whom, strikingly, he persistently calls *black*. Thus Thatcherism is pictured as the culmination of the "gradual transformation of black citizens into trespassing aliens" (p. 102). To put it mildly, this glosses a complex history—including the highly varied experience of different kinds of New Commonwealth immigrants and the development of an established body of anti-race discrimination legislation.

This is a quarrel on a narrow point, true, but it is symptomatic of a more general difficulty with Krieger's account, which is that some of his key conceptual categories are less than rewarding. Thus much is made of "suburbanism" as a characterization of Thatcherism. But the Conservative party has been suburban for a century. That is part of the secret of its success. Nor are the distinctive elements of "suburbanism" very convincingly described. Thus, Krieger refers to "the xenophobia of the private housing estate" (p. 63) as one element; yet the burden of evidence is that xenophobia is, if anything, more common to proletarian life.

MICHAEL MORAN

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**Comparative Democracy: Policymaking and Governing Coalitions in Europe and Israel.**

By Gregory M. Luebbert (New York: Columbia University Press, 1986. xiv, 341p. \$32.00).

Despite its misleadingly general title, this is a thought-provoking book on a specialized topic: the formation of governing coalitions in multiparty democracies.

Luebbert begins by examining government coalition formation in a number of multiparty systems, develops a general theoretical formulation to account for the observed patterns, then returns to these countries to test, or flesh out, the theory. His general theory relates the patterns of coalition formation to properties of the democratic regime through an analysis of the goals and rational strategies of party leaders. He buttresses the theory with some

broad statistical comparisons of 67 cases of government formations in Norway, Sweden, Denmark, Finland, Italy, the Netherlands, and Israel, as well as detailed applications to Israel, Denmark, and two periods of Finnish coalition formation. The empirical analysis draws on an impressive array of primary and secondary sources.

The starting point for Luebbert's analysis of the coalition formation itself is the party selected as the *formateur*, which has the responsibility for building the coalition. Other parties' policy goals are analyzed as convergent, divergent, or tangential (focused on different issues) in relation to those of the *formateur*.

Most parties are assumed to have a limited set of decisive policy principles—their "policy profiles"—that are relevant to coalition participation. The policy profile is usually derived from the history of the party and defines its identity to its supporters. On issues outside the profile, party leaders can negotiate relatively freely. But on the decisive party profile issues, the leaders can seldom negotiate without jeopardizing their leadership position.

According to Luebbert, different types of democratic systems differ in the incentives they offer to parties to cooperate, inside and outside of the government arena. He develops a fourfold typology of regimes—consensual, competitive, conflictual, and unconsolidated—based on the legitimacy of the regime and the role of opposition parties in policy-making. The incorporation of electoral and policy-making factors outside the narrow legislative arena is an important feature of the analysis. Within the competitive type, he further distinguishes between dominated and undominated systems, depending on the presence or absence of a party whose presence is believed to be essential for all future coalition governments. He then predicts the limits of policy tolerance that affect patterns of party cooperation in each type of system.

This analysis not only leads to specific predictions about government coalitions in each case, based on the party profile relations and their implications for cooperation in each type of system but to explanation of patterns of coalitions. We can thus understand the appearance of minimum winning coalitions in undominated competitive systems; the frequent appearance of minority governments in

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the consensual systems and, for different reasons, in the conflictual ones; the appearance of oversize government coalitions in the dominated competitive systems; and so forth. It is, of course, the ability to explain these latter events that makes Luebbert's theory innovative and important.

The work is highly suggestive but seems to contain notable limitations. At the theoretical level, I am troubled by failure to incorporate some of the factors usually considered in coalition analysis; for example, the loss of patronage and of policy implementation control necessitated by bringing additional parties into the coalition. The emphasis on policy profile goals to the exclusion of other considerations seems overstated. On the empirical side, the analysis of coalition formation in seven nations using four or five system configurations inevitably makes the results highly dependent on nation-specific patterns. Luebbert does include several interesting within-nation changes in system configuration. But the combination of an inferential approach and an analysis in which there are nearly as many relevant configurations as countries make the empirical results more suggestive than confirmatory, despite the accuracy of the retrospective predictions.

I have no doubt, however, that some of the innovative elements proposed here will become part of any mature theory of coalition formation. Luebbert's work departs from the large theoretical and empirical literature on this topic in several very interesting ways. The results are sufficiently impressive to merit serious attention from scholars working in this area.

G. BINGHAM POWELL, JR.

*University of Rochester*

**Authoritarians and Democrats: Regime Transition in Latin America.** Edited by James M. Malloy and Mitchell A. Seligson (Pittsburgh: University of Pittsburgh Press, 1987. 268p. \$25.95 cloth; \$12.95 paper).

The recent breakdown of most of the bureaucratic-authoritarian (BA) and "reactionary despotic" regimes in Latin America and the earlier demise of the corporative-authoritarian systems in Spain and Portugal have given rise

to a growing "transition to democracy" literature, to which *Authoritarians and Democrats* makes an exceedingly useful contribution. Of particular value is the manner in which a number of the authors have interpreted the well-known 20-year cycle of dictatorship and democracy in the region.

Mitchell Seligson argues that although the permanence of the current democratic phase is by no means assured, just as the BA regimes of the 1960s and 1970s represented a "new breed" of authoritarianism, the current democratic regimes are also of a different order from those of the 1940s and 1950s. This is due to three factors flowing from the character of the bureaucratic-authoritarian regimes that preceded them: a much greater reluctance by civilian political leaders "to consider the 'military option' a convenient way out of mounting economic and/or political problems" (p. 9), the inability of the military establishments to manage their national economies effectively, and the fact that the contemporary democratic governments have come to power at a time in which the minimum levels of socioeconomic development necessary for democracy—defined as an income level of 250 per capita 1957 dollars and a literacy level of over 50%—had been reached in most of the countries.

Applying his thesis to Central America, Seligson found that although there was considerable lag time, as those nations approached the minimum threshold of socioeconomic development there was a tendency for them to create civilian democratic governments and to return to authoritarianism when they dropped below that threshold. The current paradox in the situation is that while the rapid economic growth from 1960 to 1975 pushed all of these nations, except Honduras, above the economic threshold for democracy, the resulting income inequality generated by that growth has raised the level of insurrectionary activity.

In his very interesting contribution, "The Peruvian Experience," Luis Abugattas argues that the traditional association of populism with democracy in Latin America and anti-populism with authoritarianism obscures the fact that they really constitute two distinct, although interrelated, cycles. Peru, he states, has experienced both democratic (1963–68) and authoritarian (1968–76) populism, classic authoritarianism (1976–80), and antipopulist

democracy (1980-85)—all brought about by the classic growth-distribution dilemma.

James Malloy enhances our understanding of the double cycle by pointing out that while organized labor in Latin America tended to be populist, labor leaders often had more access to social benefits under authoritarian governments than they did in democracies. "In the end," he contends, "access to power and control over distributable goods is the name of the game, not forms of regimes" (p. 252).

This judgment appears to be largely confirmed by the other contributors to the volume. Aldo C. Vaca points out that at the very time President Bignone was beginning the redemocratization process in 1983, most of the major economic sectors in Argentina were demanding "the formulation and implementation of the state-led programs of reactivation and distribution better suited to their interests and goals" (p. 30). Similarly Silvia T. Borzutsky argues that it was primarily the failure of the monetarist experiment in Chile that almost destroyed the Pinochet regime in 1983. Finally, James Malloy and Eduardo Gamarra contend that the inability of General Hugo Banzer to manage the clientelistic demands in Bolivia's "neopatrimonial" society led to the move toward civilian rule there in 1978. Only in Brazil, Silvio Duncan Baretta and John Markoff argue, was the *abertura* largely driven by political considerations.

The essays in this book, including additional chapters on Ecuador, Central America and U.S.-Latin American relations, are of a uniformly high quality and make a valuable contribution to our understanding of Latin American politics over the past two decades.

ROLAND H. EBEL

*Tulane University*

**The Politics of the West German Trade Unions: Strategies of Class and Interest Representation in Growth and Crisis.** By Andrei S. Markovits (New York: Cambridge University Press, 1986. xx, 599p. \$54.50).

The German Trade Union Federation (*Deutscher Gewerkschaftsbund* [DGB]) and its 17 constituent unions comprise the strongest and most effective union movement of any

large industrial democracy, and Markovits has written the first book in English to show extensive knowledge of this organization. The first three chapters cover union structure, labor law, and the history of the federation. The history emphasizes DGB programmatic demands, conflicts between activist and moderate union wings, and relations with parties and governments from the 1949 birth of the Federal Republic to 1981.

Four subsequent chapters deal respectively with the activist metalworkers union (with over a third of DGB members the largest union in the noncommunist world); the moderate construction union; the chemical union riven itself by activist and moderate wings; and the oldest German union, the printers. The four are all private sector unions, and each chapter is heavily involved with union wage determination, collective bargaining, and strikes. Since each of these chapters and the DGB history are written chronologically, much repetition ensues. The issue of the 35-hour week, for example, appears in chapters on the DGB (pp. 146-53), the metal union (pp. 252-63), the printers union (pp. 410-14), and in the conclusion (pp. 432-37). The conclusion summarizes, updates from 1981 to 1984, and covers challenges to the unions in the crisis years since 1974, particularly rapid technological change and high unemployment.

There is an enormous literature by and about the unions, and Markovits has digested a sizable part of this and produced accurate and readable chapters on the DGB. The readability of the union chapters suffers somewhat in the details of one after another wage struggle.

Facts are generally handled well, but a problem with judgments seems to derive from overexposure to the more radical activist union point of view and very little exposure to empirical studies of West German politics. Unionists tend to an apocalyptic style of writing. On the side of the angels, they fight a sinister and powerful capital; and when union demands are not fully met, a great defeat has occurred.

Thus, the 1952 codetermination law, which gave labor a third of the directors on the boards of large firms, was a "disastrous setback" (p. 83). It did not match the equal seating of labor and capital on boards in the coal and steel industries given by the British in



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1947 and secured under West German law in 1951—"the unions' only success during the period under consideration and for some time to come" (p. 77). This judgment simply ignores the difference between *no* seats and *one-third*, and in addition all kinds of highly beneficial labor and social laws that were passed in the 1950s with strong inputs from a Christian Democratic labor wing, Social Democrats, and the unions.

Among many other questionable judgments are "The political 'independence' of the unions has undercut their ability to influence the policies of both individual parties and the federal government" (p. 28), "Capitalism in West Germany could not destroy labor (as happened in the Weimar Republic)" (p. 65), The "restraining qualities [of laws governing industrial relations] have on the whole outweighed the benefits for West German trade unions" (p. 419), and "Labor will by definition remain unsatisfied with the operation of government given West Germany's capitalist framework" (p. 425).

Two serious factual mistakes appear in the chapter on the construction union as a part of a diatribe (uncharacteristic of other parts of the book) against a former president—a leading moderate, Georg Leber. Leber's winning a seat in the Bundestag in 1957 supposedly "made him the first major union official to hold a seat in the legislature since Weimar" (p. 346). But prior Bundestags included three national union presidents (Freitag, Imig, Jahn), two presidents of DGB state districts, and 11 members of union and DGB national executive boards. Also, "Actual earnings in construction remained lower than in all other major industries in West Germany" under Leber (p. 357). Federal statistical yearbooks, however, report that construction workers were sixth highest out of 24 industries in 1955 and tied for seventh with iron and steel workers out of 36 industries in 1966, the years spanning Leber's stewardship of the union. This chapter, as well as the one on the printers, was "written with Thomas C. Ertman" (p. 327). Still, overall responsibility must remain with the senior author.

RICHARD J. WILLEY

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**Government and Private Sector in Contemporary Mexico.** Edited by Sylvia Maxfield and Ricardo Anzaldua Montoya (San Diego: Center for U.S.-Mexican Relations, University of California, 1987. iv, 146p. \$3.00 paper).

For much of this century, private business in Mexico has survived in a postrevolution constitutional system in which it does not share equal political status with other groups within the ruling Institutional Revolutionary Party (PRI). By the mid-1980s, however, increasing numbers of business people and organizations have become more militant, advocating a new political party for business, supporting the major opposition party, the National Action Party (PAN), or increasing civic education about business. This is occurring in a period of considerable change within the international and the domestic economies. The search by business for a satisfactory place in the Mexican social pact is explored in rich detail in this collection, detailing conflicts between business and the state and within business itself, as both the state and business attempt to adjust to the internationalization of the political economy.

In the introduction, Sylvia Maxfield offers a clear overview of the search by private business for an appropriate political role and of its divergent approaches to gaining more influence within the system. She notes—but might have expanded upon—the growing cleavages between the professional economic technocrats in the government and those who shape policy within the PRI. These conflicts could be critical to future economic policy.

The analysis by Matilde Luna, Ricardo Tirado, and Francisco Valdes traces the growth of increased political militancy by business since the nationalization of banks in 1982. Attacks on the presidential system, development of truly competitive political parties, and creation of a free market economy are included among the demands of militant business voices.

The free market is precisely what won't work, contends Ricardo Carrillo Arronte, who believes it is "outdated" because private business is unable to raise production to necessary levels for Mexico. Sharply critical of the export of capital by Mexican entrepreneurs, he argues that business is not willing to

reestablish confidence in the present system.

Apart from conflict between business and the state, sharp differences among Mexican business people and organizations constrain the exercise of political influence by business, according to Saul Escobar Toledo. Tracing political differences among business organizations between 1976 and 1986, he, nonetheless, believes that present conditions may be right for the exercise of increased political influence by business.

A closer look at one of the major business organizations—COPARMEX—the Confederación Patronal de la República Mexicana is provided by Luis Felipe Bravo Mena. Examining five phases of the organization's relations with the government, he regards the last five years as especially conflictual. While COPARMEX, as an employers union, does not aspire to political power, it "plays the political game, but it does so outside the party arena."

Celso Garrido Noguera and Enrique Quintana Lopez point to the structural change underway in Mexico in the 1980s from what had been the old "hegemonic bloc" controlling the distribution of capital and resources to a new, still-fragile, reconstituted bloc. The new "bloc" includes international banks, former Mexican bankers, reorganized industrial interests, and new elements in the state economic bureaucracy. These may be joined by other new sources of foreign exchange—drug traffickers and *maquila*-manufacturing enterprises.

In any case, the way Mexican businesspeople perceive themselves and their social-political roles is likely to shape their future political behavior, according to the study by Roderic Aix Camp. The attitudes and images of Mexican businesspeople are in flux, but Camp believes that more are accepting the concept of social responsibility and are defining themselves politically in a more assured and self-confident manner and more are inclined to play a political role.

Although according to most authors structural transition may be occurring in Mexico, none is convinced that Mexican business will achieve greater political influence in its relations with the state and within the society. Indeed, increased militancy could further fragment society and lead to powerful reactions by the state and by other social actors.

This is a most useful and detailed account

and analysis of the political role of private business in Mexico in the 1980s, for the ultimate definition of this role will determine much about Mexico as it moves into the twenty-first century.

JAMES W. DULL

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**Party and Participation in British Elections.**

By Anthony Mughan (New York: St. Martin's, 1986. vi, 159p. \$29.95).

Anthony Mughan's new book is innovative and thought-provoking. In the past decade political scientists interested in the study of voting and elections have made increasing use of time-series aggregate data. Much of the resulting research has focused on the impact of macroeconomic conditions on support for governing parties and their leaders. In *Party and Participation in British Elections* Mughan expands the scope of this emergent research tradition by using aggregate data gathered over a 33-year period (1950–83) to investigate factors influencing electoral turnout.

Mughan's decision to focus on turnout rather than vote shares is a welcome departure in the face of the voluminous literature on the latter topic and the paucity of studies of the former one. The lack of attention to the "turnout dimension" is particularly apparent when one considers the corpus of work on non-U.S. political milieux. Mughan is acutely aware of this lacuna in the literature, one that he explains as follows: "It is an undeveloped branch of the study of electoral behaviour precisely because the contribution of short-term forces to the explanation of turnout . . . has been largely ignored" (p. 6).

Short-term forces are, of course, many and various. Mughan's particular focus is on the efforts parties make to bring voters to the polls. He argues that such political mobilization efforts, while not completely neglected in previous research, have not been adequately studied. In *Party and Participation*, Mughan considers the impact of mobilization on three (dependent) variables: (1) constituency-level turnout in the several general elections occurring between 1950 and 1983; (2) turnout in 240 by-elections taking place in this period; and (3) the difference in turnout in particular constitu-

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encies in successive general and by-elections. Based on a host of detailed analyses Mughan argues forcefully that party mobilization is an important determinant of turnout in each of the three cases.

The argument is intuitively appealing and accords well with other recent research on the significance of short-term forces in models of electoral behavior. However, the analyses upon which it rests are likely to be viewed as problematic by some readers because of the way Mughan operationalizes his key independent variable, that is, mobilization. His constituency-level aggregate data do not provide direct measures of parties' mobilization efforts. To circumvent this problem Mughan relies on interparty competition as a proxy variable. Competitiveness at the constituency level "can be taken as a surrogate measure of the organization mobilization of support undertaken by political parties especially, but also by organizations affiliated to them and other partisan and non-partisan groups and individuals" (p. 50). Although it seems likely that levels of party competition and parties' mobilization efforts are positively related, the proposition that the latter is adequately indexed by the former is an empirical question—one that the author does not answer.

Other aspects of the analyses are provocative as well. One example is the neglect of constituency-level socioeconomic and demographic characteristics as factors that might influence turnout (and interparty competition and party mobilization as well). Mughan justifies ignoring these "nonpolitical" variables by arguing that "the explicit focus of this analysis is the politics of turnout and to include in it non-political variables would only risk obfuscating this focus unnecessarily" (p. 12). Granted Mughan's additional point that many such variables change only slowly, their neglect may prove troubling for readers concerned with the possibility of model misspecification caused by the omission of theoretically relevant variables. A second, related, example is the failure to include a measure of the strength of voters' partisan predispositions. Several studies have documented the weakening of partisanship in the British electorate in the post-1970 period, and it is plausible that this trend has had consequences for various forms of political participation including turnout. More generally, the failure to consider

possible time-series dependencies in the data created by the dynamics of relevant long-term forces might be judged by some as a risky research strategy.

Overall, most of the contentious aspects of the analyses presented in *Party and Participation* arise not because Mughan is unaware of important theoretical and methodological points at issue but rather because he is the captive of a data set that is not fully adequate to the research task at hand. It is abundantly evident that he is well aware of the limitations of his data—indeed, he repeatedly draws them to the reader's attention. Moreover, cognizance of these limitations prompts speculation about a variety of interesting questions that might be pursued in subsequent inquiries. In the end, Mughan's failure to address some of these questions within the framework of his empirical analyses is less important than the contribution he has made by raising them. Those doing subsequent research on electoral participation in Britain or other Western democracies will benefit by carefully considering the research agenda established by this thoughtful study.

HAROLD D. CLARKE

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**Political Parties and Elections in West Germany: The Search for a New Stability.** By Stephen Padgett and Tony Burkett (New York: St. Martin's, 1986. xii, 308p. \$32.50 cloth; \$14.95 paper).

**West German Politics in the Mid-Eighties: Crisis and Continuity.** Edited by H. G. Peter Wallach and George K. Romoser (New York: Praeger, 1985. viii, 278p. \$34.95).

From 1969 through 1982, when two worldwide recessions punctured the myth of boundless prosperity, when Western nations floundered in their transition toward the "post-industrial" age and were pronounced "ungovernable," one country in the Atlantic Alliance managed to keep the same party(s) in power: West Germany. True, even there change finally caught up with the government, but not until everyone else had gone first. In fact, throughout the whole postwar period,

West Germany had fewer chancellors than the United States had presidents or Britain prime ministers.

Never mind. Accounts of German politics de rigueur carry apocalyptic overtones. As if "continuity" was suspect and "crisis" more natural; as if the country had not found its political bearings but was still on a "search for a new stability." Even Wallach and Romoser as well as Padgett and Burkett feel compelled to cast their contributions in a mold of uncertainty, though only in their subtitles and introductions. Once immersed in the stream of politics, neither of them, nor the various contributors to the Wallach and Romoser volume, find much that is unsettling in West German politics. In fact, they all reassure us of the pragmatic, adaptive nature of politics in West Germany. What is surprising is how unsurprising West German politics is, how skilled at "crisis management," how deft in negotiating a *Machtwechsel* (change in power).

*West Germany in the Mid-Eighties*, edited by Wallach and Romoser, the latter a co-founder of the Conference Group on German Politics, focuses on the "critical election" of 1983. Chancellor Schmidt's coalition government of the Social Democratic Party (SPD) and the Free Democratic Party (FDP) had collapsed in September of 1982 and had been swiftly replaced by a new coalition composed of the Christian Democratic Union/Christian Social Union (CDU/CSU) and FDP. The March 1983 election was called to give the new parties in power the popular license to operate the government. The actions of the political parties leading up to that election are reviewed, with considerable historical depth, by Patterson (on the CDU/CSU), Braunthal (SPD), Soe (FDP) and Buerklin (Greens). The economic context is provided by Wallach, and the international setting by Hanhardt. Dalton and Baker dissect the electoral verdict of 1983.

Soe's contribution on the FDP, the "pivot" party, is a gem, a riveting story of a party flirting with self-destruction but without whose political high-wire acts the Federal Republic would not be governable. Soe chronicles the metamorphoses of the FDP from "residual bourgeois party" in the early 1950s through its social-liberal phase in the late 1960s back to economic laissez-faire, with Count Lambsdorff rising as a reincarnated "Erhard of the 1980s." We learn in suspenseful detail how the FDP

reached its decision in 1982 to quit Schmidt's government and forge a new coalition with the CDU/CSU under Helmut Kohl, with the names of the FDP deputies in favor of the move and opposed.

The electoral analysis by Dalton and Baker raises some intriguing points. For all the claims of an ideological *Wende* (U-turn) and the sound and fury of the "missile" campaign, the ordinary German going to the polls apparently did not endorse a sweeping policy cleaning—either a mandate for the deployment of new intermediate missiles or a dismantling of the welfare state. In fact, on a battery of key issues, the "CDU/CSU is most proximate to the electorate on not a single issue. Even the Greens are seen as the most representative party on three issues" (p. 52). Yet, as the authors also demonstrate, the public rated the CDU-led government as better able than an SPD-led government to handle almost every issue that mattered. So much for (position) issue voting in West Germany!

*Political Parties and Elections in West Germany*, by Padgett and Burkett, who teach politics at Loughborough University, England, for the most part chronicles party history over the past hundred years. There is no pretense of this being the definitive work on the history of German parties. Nor is this an analysis to settle scholarly controversies about party development. A mildly leftist undercurrent streaks through the flow of events now and then.

Padgett and Burkett emphasize the split personality of the Social Democratic party, beginning with the rivalries of its patron saints, Ferdinand Lassalle and Karl Marx. But their own account also displays a split personality. The pre-1933 SPD gets a scolding for lack of revolutionary zeal: "The successful and savage destruction of the German Revolution [of 1918–19] was accomplished by the surviving forces of Imperial Germany, encouraged by the quivering comrades [SPD] who now ruled the state" (p. 38). On the other hand, the post-1945 SPD is upbraided for its "refusal to join a coalition with the 'bourgeois' parties" (p. 46).

The freshest chapter of their book is the one dealing with Germany's newest party, the Greens. The authors stress that this is "by no means a single issue party. It is rather, a party of protest against modern industrial society and its economic, political and social effects"

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(pp. 181-82). The Greens are the partisan vanguard of "post-materialism," the harness of the "new social movements" of the 1970s. How the Greens as a party will do in the future depends largely, the authors argue, on how this party will answer the *Gretchenfrage* of a coalition with the SPD. Their chapter on the electorate, on the other hand, reads like a stale recitation of demographic patterns. The *Forschungsgruppe Wahlen*, whose data are cited here, polls on many more fascinating items than social class and religion. We learn little about reasons for the ups and downs of party fortunes in elections.

On the whole, however, the Padgett and Burkett book offers undergraduate students a handy compendium on party development. The Wallach and Romoser volume, in turn, fills the role of a *Germany at the Polls* for 1983, with crisp accounts of each party's background. My advice to the two editors: reissue the volume with updates on the 1987 election and with a more affordable price tag.

HELMUT NORPOTH

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**The Impact of Illness on World Leaders.** By Bert Edward Park (Philadelphia: University of Pennsylvania Press, 1986. xxiii, 377p. \$24.95).

Political scientists have long been interested in the study of leaders, and such work has typically focused on, for example, childhood socialization, wealth, education, social class, and psychological characteristics. For the most part, political and other social scientists have ignored the fact that leadership, at least at the head-of-state level, is normally a phenomenon of the middle to advanced years. As such, the diseases often associated with aging are relevant to an understanding of the behavior of leaders. The burden of this excellent volume by Bert E. Park, a practicing neurosurgeon with graduate training in history, is that a precise knowledge of medical science coupled with standard individual-level historical and political analyses will yield powerful information about the real behavior of political leaders, information that goes far beyond what we have come to expect from the discipline of political science.

The political behavior and medical histories of eight leaders are carefully examined. Woodrow Wilson, Franklin D. Roosevelt, and Adolph Hitler have been selected for extended treatment, while briefer case studies are developed for Paul von Hindenburg, Ramsay MacDonald, Jozef Pilsudski, Winston Churchill, and Anthony Eden. "Each leader," Park tells us, "was considered among the greatest movers and shakers of his day, yet all were afflicted by neurologic illness that impacted negatively on their leadership skills and decision-making abilities" (p. xix).

Methodologically, the author undertakes a remote medical diagnosis of each leader-patient by using memoirs and other primary source materials, combined with the most recent medical knowledge. To those potential critics of ex post facto medical analysis, Park asserts that "as 80 percent of diagnosis in medical practice relies on the medical history, and only 20 percent on physical examination, the latter is relatively unimportant in such medico-historical analysis" (p. 79).

The results of this methodology are impressive, and two examples can be offered. The academic controversy regarding whether Franklin Roosevelt was or was not competent during his last years in office is carefully addressed by Park in his examination of the president's hypertensive encephalopathy, which is a transient disorder causing extremely high blood pressure during which the patient is confused and disoriented. After an attack passes, the patient returns to a more stable and unconfused posture. Thus, Roosevelt experienced alternating psychomedical states, and this explains why historians and other analysts have had difficulty in interpreting his observed political behavior.

Most examinations of Adolph Hitler have been anchored in psychoanalytic theory, but Park points out that a broad range of organic factors had important behavioral influences. Hitler, physically ill for an extended period of time, was afflicted by Parkinson's disease, chronic cholecystitis (inflammation of the gall bladder), depression, temporal lobe epilepsy, and polypharmacy. The latter is especially important: Hitler's physician was treating him with over 70 medications including overdoes of two known poisons, atropine and strychnine. Moreover, there is strong evidence that Hitler regularly used cocaine and synthetic

morphine derivatives. As Park carefully illustrates, these illnesses were important determinants of Hitler's political behavior.

One of the rewarding strengths of Park's analyses is his sensitivity to the behavioral correlates of disease and medications. This, combined with his skillful analysis of decision-making processes, nicely integrates the multiple dimensions of political behavior within a single individual. Moreover, so effective is this work, that it speaks strongly to reorienting much of what we call *political decision making* to the individual level of analysis.

I do have some problems with the book. For a volume packed with a considerable amount of scientific detail, it would have been helpful to the reader to have the text broken up into subsections for ease of reference and for pulling out comparative data. Beyond the chapter titles, no subsections exist.

I found the concluding chapter disappointing, not for what it said but for what it did not say. This chapter deals essentially with the provisions of the 25th Amendment to the U.S. Constitution. However, since only two of the eight cases in the volume were U.S. citizens, the applicability to other political systems is limited indeed. A cross-national focus on disability would have been useful.

Park is strongly critical of the physicians who treated the leaders under study here. A considerable amount of professional incompetence and deceptive behavior is documented. An extended reflection on this would have been helpful. Why did heads of state not recruit the best medical talent of the day as their private physicians? How should physicians for heads of state be chosen? How should they be removed when found incompetent? Why did the political entourage around each head of state tolerate medical incompetence and deception? The author provides the beginnings of responses to some of these questions throughout the book, but the answers never come together in a coherent way in the concluding chapter.

But these criticisms are minor. Park has produced a volume that is destined to become a classic interdisciplinary study of individual-level political decision making. It deserves serious attention.

THOMAS C. WIEGELE

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**Political Authority in a Sicilian Village.** By Filippo Sabetti (New Brunswick, NJ: Rutgers University Press, 1984. xi, 293p. \$35.00).

Over the past decade-and-a-half a number of books have taken on the problem of the Mafia in Sicily, confronting the apparent paradox of an outlaw regime coexisting with a modern western state. From Anton Blok's *The Mafia of a Sicilian Village* (1974) and Jane and Peter Schneider's *Culture and Political Economy in Western Sicily* (1976) to Pino Arlacchi's *La mafia imprenditrice* (1983), they have asked whether there is any special cultural basis for the Mafia and how the Mafia has been produced by national and international political economic forces.

Sabetti, reconstructing the political history of a western Sicilian village over the past two centuries, tackles these same problems and is at pains to show that the Mafia arose not due to any Sicilian defects of culture or personality nor to any aversion to taking collective political action but is instead attributable to an administrative structure and national policies that robbed the people of their local autonomy. In this view, the Mafia was created as a means through which peasants could defend themselves from the caprices of extralocal powers, especially as these developed in the wake of Italian Unification in the 1860s.

One of the book's merits is the meticulous reconstruction of the relationship between the development of a Mafia system of power in this village and the development of Church-inspired and socialist social movements around the turn of the century. According to Sabetti, the Mafia grew up as a system of local consensus, sanctified by the local clergy. Hence, Sabetti argues against his predecessors' view that the Mafia developed as the exploitative arm of estate managers to maintain rural order and put down peasant attempts to assert greater power.

Sabetti also discusses the transformation of the local government in the post-World War II years "into a sort of trading company" of the Mafia (p. 168). Government patronage was dispensed to supporters and hurdles were erected to prevent opponents from availing themselves of the public benefits to which they were legally entitled. Yet according to Sabetti the Mafia collapsed shortly after this, in the

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1950s, following its transformation from a system that represented the interests of the peasants to one that was wholly parasitical. It was this loss of moral authority together with the increased strength of the national party organizations at the local level that brought about its demise.

Here we have a strikingly positive view of the development and functioning of the Mafia at the local level in Sicily. The Mafia is seen as a continuation of a local self-help tradition that lasted as long as it benefitted the villagers and then disappeared when it had outlived its usefulness to them. Rather than reflecting amoral familism, the political history of the village shows, according to Sabetti, a value system prizing "universalistic values of liberal democracy" (p. 235).

This is a surprising book, taking a thesis dear to Sicilian apologists—that all Sicily's problems derive from the national bureaucracy—and arguing that until the nineteenth century institutional arrangements in baronial Sicily were more conducive to local self-government than in more recent times. While provocative, it is unlikely to convince many scholars to overturn their less rosy view of the development and role of the outlaw regime, both because of questions of evidence regarding the relationship between the landowners and the Mafia at certain crucial points, and because, assuming Sabetti's portrait is accurate, the evidence from other parts of western Sicily make his village appear unusual. Blaming "general social disintegration" (p. 234) in Sicily on "institutional arrangements" and especially on centralization of power and lack of local autonomy, can be convincing only if it is shown that these institutional arrangements are different in Sicily than in northern Italy, which has a very different political culture. This Sabetti does not do, at least for the postwar years. Finally, it would also be helpful if more consideration were given to the movement of the Mafia from the countryside to the big cities and the change in the nature of the Mafia from an agrarian phenomenon to one principally involved in state parasitism in the form of living off development and construction contracts and the like, as well as the new-found wealth available through the drug trade. The decline of the Mafia in a small rural village of Sicily signals not the end of the Mafia but merely its

moving on to greater opportunities elsewhere, a statement with which Sabetti would likely agree.

DAVID I. KERTZER

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**Crisis in the French Labour Movement: A Grassroots Perspective.** By W. Rand Smith (New York: St. Martin's, 1987. xiii, 272p. \$29.95).

W. Rand Smith asserts that most literature on French trade unions focuses on the pronouncements of the peak confederations rather than on the actions of union locals at the shop-floor and plant levels. He addresses the problem by studying union locals of two major trade union confederations, the Confederation Générale du Travail (CGT) and the Confederation Française Démocratique du Travail (CFDT), in four metalworking plants in Grenoble.

The introductory section describes the theoretical and historical setting of the study. Smith seeks to determine whether the crisis of French trade unions derives from economic factors, such as shifts in the occupational structure from manual to technical positions; from political factors, particularly splits within the French Left; or from institutional changes, especially unions' greater legal rights, which paradoxically may weaken them by separating leaders from rank-and-file workers. (Smith later emphasizes the importance of management and local union strategy.)

Part 2 describes the four plants that served as research sites, with particular attention to the CGT and CFDT locals, and reports findings from Smith's interviews with union militants. The background and motivation of rival union militants are remarkably similar. In particular, and contrary to expectation, CFDT militants are only slightly more religiously observant than their CGT counterparts. The most interesting finding is that militants report that their greatest problem—mentioned more than twice as often as their relations with management—is the distrust in which militants are held by rank-and-file workers! Unfortunately, Smith barely explores the possible basis for this distrust, which would seem central in explaining union difficulties.

Part 3, which analyzes unions' activity during strikes, relations between CGT and CFDT locals, and union-management relations, is the richest section of the book. Smith finds that unions neither fully direct strikes nor merely capitalize on worker discontent. The unions' importance derives from their activities during periods of quiescence, when they raise workers' consciousness by offering alternatives to management's views. Smith asserts—coarsely but without evidence—that this lays the groundwork for subsequent mobilization at a point when arbitrary management actions precipitate worker protest.

Smith's observations of CGT-CFDT relations at the plant level supplement accounts of the two unions' stormy relations at the national level. He suggests that CGT and CFDT locals tend to conflict during periods of quiescence but are pressured by workers to cooperate during strikes. However, when a settlement is reached, conflict recurs as each union seeks credit for gains that have been achieved.

Smith describes an important recent shift in management's strategy from confrontation to consultation, common elsewhere but quite surprising in France. He suggests, as Michael Piore (whom Smith does not cite) did several years ago that French management's initiatives represent a response to union advances in the post-1968 period. As unions became more institutionalized following the May 1968 uprising, management devised consultative techniques that bypass and thereby weaken unions. Smith's assertion is based on speculation. Interviews with management of the four plants would have been useful here. The book concludes with an assessment of the Auroux industrial relations reforms enacted in 1982, and concurs with other recent research that minimizes the importance of the legislation.

Smith occasionally overreaches, as when he claims that because previous research has studied unions at higher levels, it has focused "inordinately on union ideologies rather than union practices, on what unions say rather than what they do" (p. 7). Given the fact that industrywide and peak-level collective bargaining in France has regulated key aspects of work life, this assertion is quite misleading. Even more questionable, in light of the CFDT's strategic reorientation since 1978, is to consider the union anticapitalist (p. 179). And he

remains faithful to the common practice of neglecting Force Ouvrière, the third major French trade union confederation, whose substantial gains are in marked contrast with CGT and CFDT decline.

*Crisis in the French Labour Movement* provides a useful addition to the burgeoning literature on French trade unions. Smith reminds us that national shifts reflect complex and often contradictory local developments. He underlines the importance of management and union strategy in shaping working class structure and activity, rather than accepting economic or technological determinism. (For example, he notes—without analyzing sufficiently—that technicians, engineers, and even lower-level management are unionized in the plants he studied.) Both because of what it accomplishes and fails to accomplish, *Crisis in the French Labour Movement* suggests the value of local studies of unions.

MARK KESSELMAN

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**The Primordial Challenge—Ethnicity in the Contemporary World.** Edited by John F. Stack, Jr. (Westport, CT: Greenwood, 1986. x, 231p. \$35.00).

*The Primordial Challenge—Ethnicity in the Contemporary World*, edited by John F. Stack, Jr., is a collection of 10 essays. The first 3 essays are primarily conceptual, outlining several approaches to the study of ethnicity. The remaining 7 essays are descriptive case studies of ethnic relations in particular countries.

The use of the word *primordial* in the volume's title and in the title of the editor's introductory chapter, "Ethnic Mobilization in World Politics: The Primordial Perspective," is misleading in both instances. None of the chapters argues for a primordialist conception of ethnicity; namely, that ethnicity is a thing sui generis, arising out of antiquity, prior in its origins, primal in its influence. Both Stack's introduction and his volume reflect a diversity of views and approaches to ethnic phenomena. This diversity opens the door to some interesting essays. Stack fails to provide an overarching framework, however, and the result is a very loosely constructed book.



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An example of an essay somewhat adrift is chapter 2, "Theories of Ethnic Relations," by Michael Hechter. Reflecting on the state of ethnic relations theory construction, Hechter identifies three necessary dimensions of any complete theory of ethnic relations. Such a theory must account for variations in (1) ethnic group cohesion, (2) rates of ethnic mobilization, and (3) relative strength of ethnic group formation (compared to, say, class). Hechter provides a cursory discussion of the literature and finds little evidence of successful theory construction. The fact that the essay was written in 1978 (a fact noted twice by Hechter) renders its conclusions essentially useless and raises questions about the author's and the editor's decision to include it in the volume.

Cynthia H. Enloe's chapter, "Ethnicity, the State, and the New International Order," though also somewhat dated, provides the reader with a much more current and lively exposition. Enloe argues that the significance and operation of ethnicity in modern states lie in the relationship among the state, ethnic groups, and the international system. The international system affects ethnic-state relations in two ways: (1) through the internationalization of the division of labor and (2) through the internationalization of state security. In this chapter Enloe discusses in detail only the first (though she has written extensively on the second). She catalogs several ways in which states coopt, coerce, exploit, and generally manage ethnic labor and discusses the ways in which the international economy influences state-ethnic relations. Enloe draws examples from a wide variety of states and her illustrations are lucid and convincing.

In chapter 4, "Visions of the Melting Pot in the American City: European and Native Expectations in the United States and Argentina in the Period of Mass Immigration," Mark D. Szuchman sets out to compare the immigration experiences of the United States and Argentina to "illustrate the results of . . . [elite] xenophilism" (p. 43) in Argentina. Szuchman begins by outlining the evolution of sociological and historical thinking about urban immigration among U.S. and Argentine scholars. He then presents two "overview[s] of immigrants" in U.S. and Argentine cities. These overviews reflect the disappointing weakness of this chapter. A promising agenda results in

no real information on immigration patterns (When? Where? How many? Who?). One is left quite ignorant of the timing, origins, destinations, and experiences of U.S. and, particularly, Argentine immigrants. The chapter's main contribution is a brief but interesting discussion of the Argentine state's efforts to employ immigrants for rural development.

The next two chapters focus on interethnic relations in Miami. Chapter 5, "Immigration and the Politics of Ethnicity and Class in Metropolitan Miami," by Christopher L. Warren and John F. Stack, Jr., examines the ethnic political economy of greater Miami. Warren and Stack briefly sketch the patterns of immigration leading to the current triethnic (black, Latin, non-Hispanic white) composition of the Miami area, as well as the economic and social characteristics of each of the three groups. They describe the structure of the urban government that oversees an uneasy triethnic constituency. The authors point to two factors contributing to Miami's interethnic tensions: (1) economic competition among the three groups for jobs and control of the economic sector, as well as electoral competition for control of the city government and (2) the city government's inability to mediate interethnic disputes and ameliorate interethnic competition.

As its title suggests, chapter 6, "Ethnic Bargaining and the Noncitizen: Cubans and Haitians in Miami," by Anthony P. Maingot, focuses the social science lens more closely on two components of multiethnic Miami, Cubans and Haitians. Maingot presents the first systematic quantitative evidence in the volume when he traces the historical demographic changes in the numbers and percentages of blacks, whites, and Latins in Dade County from 1950 to 1985 (p. 88). The chapter goes on to describe the timing and reception of the major waves of Cuban and Haitian immigrants. Maingot depicts the early Cuban experience as one of political and economic incorporation, whereas the Haitian quest for citizenship rights has been one of struggle, organization, and mobilization. Maingot attributes the different experiences of the two groups as mainly the result of the economic and political context of the period and the class composition of the different immigrant groups, noting the difficulties faced by the later "Mariel" Cuban immigrants.

Chapters 7-10 focus on ethnicity outside the United States. Steven L. Lamy examines the consociationalism in Belgium (chap. 7), Ralph S. Clem discusses patterns of regional economic development and ethnicity in the Soviet Union (chap. 8), Cheryl A. Rubenberg describes ethnic elitism in Israel (chap. 9), and Rene Lemarchand surveys interethnic conflict in sub-Saharan Africa (chap. 10). Unfortunately there is little in these chapters that is new or illuminating.

Lamy's chapter, "Policy Responses to Ethnonationalism: Consociational Engineering in Belgium," outlines the mainly European consociational recipe for multiethnic governance and describes the modification of Belgian consociationalism in the face of an insurgent Flemish constituency. The response of the Belgian state to Flemish nationalism was the adoption of ethnically proportional rules for public resource and job allocation, a reorganization of voting and governance, and an expansion of ethnoregional autonomy. Lamy concludes that these consociational changes equalized opportunity between Flemish and Walloon communities.

Clem's chapter, "Regional Economic Development and the Territorial Basis of Ethnicity: The Case of the Soviet Union," attempts to analyze differences in Soviet economic development among ethnoterritories in light of official Soviet policy of equality among the nationalities. He compares rates of urbanization among Russian and non-Russian territories at three points in time: 1926, 1959, and 1970. Clem finds that the period from 1926 to 1959 was characterized by a higher rate of urbanization among Russian territories than among non-Russian territories, but that the trend was reversed during the 1959-70 period. The result was reduced, but inequalities between Russian and non-Russian territories persisted. While Clem makes an effort to present systematic evidence, this chapter is flawed in two ways. First, it shares a problem with several other chapters: its results are dated. Second, and more important, Clem's use of urbanization as an indicator of economic development is highly questionable, especially in light of the vast literature on urbanization, particularly in the Third World, where urbanization is depicted as an index of poverty and agricultural decline, rather than a measure of progress.

Rubenberg's chapter, "Ethnicity, Elitism,

and the State of Israel," argues that Israelis of Ashkenazi descent represent an ethnic elite. This is hardly news to anyone familiar with the literature on ethnicity or Israel. The chapter's section dealing with the relationship between ethnicity and Israeli international security on the one hand and Israeli political policies on the other is of some interest, as is Rubenberg's passing comment on the role of labor markets in mitigating ethnic tensions. Unfortunately neither discussion is elaborated.

In the last chapter, Lemarchand's "Ethnic Violence in Tropical Africa," the author, like Hechter, admits in a footnote that the chapter is dated. Unlike Hechter, however, Lemarchand does not disown his earlier views. Although the chapter is interesting, it is simply a reprise of now familiar themes; that African ethnic conflict is rooted in colonialism and in the construction of new multiethnic states and that African postindependence politics are the politics of ethnicity. Some themes are worth repeating, however, in particular that there is much to African ethnicity that is political rather than primordial. And some themes are usefully illustrated; for example, that "new" ethnic groups can result from the fragmentation, as well as from the fusion, of cultures. Lemarchand also usefully notes the role of "external variables" (p. 194), citing Angola as a prime example of East-West interventionism.

*The Primordial Challenge* addresses an interesting and important topic in the social sciences, but the volume lacks cohesion. Stack provides no useful conceptual or thematic unifying structure. Each chapter seems almost randomly placed. The chapters do not build on or complement one another. Stack's introduction wanders through a small portion of the ethnic relations theoretical literature and never refers to the subsequent chapters nor makes any effort to integrate them. He writes no chapter introductions nor does he provide a conclusion. Finally, since the majority of the articles were written in the late 1970s, the book was published several years too late. *The Primordial Challenge* fails to live up to the promise of its content and its title.

JOANE NAGEL

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## Book Reviews: Comparative Politics

**Marxist Local Governments in Western Europe and Japan.** Edited by Bogdan Szajkowski (Boulder: Lynne Rienner, 1986. xvii, 216p. \$25.00 cloth; \$10.00 paper).

This anthology has as its purpose to elucidate the strategy and performance of Communist parties that participate in local government in Western Europe and Japan. Since all of the parties considered are nonruling Communist parties (CPs) operating within a democratic parliamentary framework, local government is seen as "the principal area in which they have been able to build up a substantial and consistent administrative record" (p. 1), and thereby establish their credentials as legitimate contenders for national power. As the authors of the introduction, David S. Bell and Bogdan Szajkowski, point out, while there have been many case studies on Western Communist parties, few have focused on the role of these parties in local government, and fewer still have provided a comparative perspective. This volume sets as its purpose the filling of this gap.

The book is only partially successful in fulfilling this purpose, however. The quality of the chapters is very uneven. This is perhaps due less to the capability of the various authors than to the great discrepancies in the intrinsic interest and relevance of the cases studied. Because of the electoral weakness of the CPs and their consequently marginal role in local government, some of the chapters—Britain, Greece, and Scandinavia (except Finland)—are very brief and consist almost entirely of an overview of the parties' electoral fortunes. Other chapters, although they deal with larger parties with important power bases in local government (Spain, Portugal, Japan), focus excessively on national strategies, electoral results, and electoral geography at the expense of a detailed analysis of the *performance* of those parties when they do participate in local government. The paucity of data on what these parties actually do at the local level can be explained by several factors: the centralized nature of the political systems in question, which allows little freedom of maneuver for local governments; the recent democratization of Spain, which has led all the political parties to focus their efforts on national-level politics; the closed nature of the Portuguese Party, which impedes empirical research into its ac-

tivities; the fact that where these parties do participate in local government, they most often do so in coalition with other parties, especially the Socialists, thereby complicating the discovery of a distinctively *Communist* style of government or policies. Despite these qualifications, in a volume purporting to provide new insights into the behavior of Communist parties by directing attention to the local level, the lack of detailed consideration of the actual behavior of these parties in local government is a serious weakness.

The reader gets an effective feel for the performance of Communist parties in local government only in the chapters on Italy, France, and Finland. On the basis of these more systematic presentations together with scattered evidence from the other chapters, some generalizations about the role of Communists in local government and the role of local government in Communist parties can be drawn. In all the countries considered in this volume (with the possible exception of Portugal), the CPs have abandoned the Leninist strategy of violent revolution in favor of a parliamentary road to socialism. As a result, local government provides an important terrain where they can test concrete policies and, more importantly, acquire governing experience and an image of efficient and innovative administration that can be used to enhance their credibility as potential parties of government at the national level. This raises the fascinating question of the degree to which Communist behavior in local government is shaped by ideology or by a pragmatic adaptation to electoral needs and local situations. In those chapters in which the authors provide evidence as to the policies and priorities of Communist parties in local government, a pattern does emerge, despite the very real constraints imposed by limited powers and funds and the demands of coalition politics. Communists appear to promote grass-roots democracy and increased "openness" in local government, in terms of increased popular participation and greater accessibility of local government to the citizenry. This emphasis on citizen participation can take various forms—support for neighborhood associations (Spain) and even formal delegation of certain city government functions to elected neighborhood councils (Italy), as well as mass mobilization of the population on issues ranging from protests

against central-government cutbacks in funds for social services to international issues like peace and nuclear weapons. Communists also tend to use their limited fiscal autonomy to promote redistributive policies in social-service provision, one of their top priorities being construction of public housing. Although such policies are far from revolutionary, and in many cases have been subsequently copied by non-Communist administrations, they do betray in their origins a distinctively leftist bias, demonstrating that ideology does make a difference, even though the local context places severe limits on the extent of that difference.

At the same time, Communist administrations in countries like Italy and France, where the fullest documentation is available and where Communists have administered a substantial portion of the country's urban population, have quite deliberately presented themselves not just as the voice of the working class, but as the guarantors of "clean" and efficient administration in the interests of the "whole population," thereby attempting to construct the broad cross-class alliances which would be necessary to carry them to positions of national power. This is especially important given the decline in their traditional constituency, the blue-collar working class. As a result, their policies and their governing style have been geared to compromise and consensus rather than to either class confrontation or direct confrontation with the central government. This is most clear in the so-called Emilian model in Italy and in the close working relationships between French mayors (including Communists) and prefects.

In virtually all the cases studied, Communist influence in local government appears to be on the decline. In the Conclusion, the editor attributes this decline to the persistence of democratic centralism as the organizing principle of CPs and the consequent subordination of local government to national policies and priorities. This is somewhat misleading, since in many of these countries *all* parties perceive local elections and local government as reflections of or testing grounds for national policies and priorities. Much more important has been the inability of left-wing administrators to come to terms with the increasingly serious problems of urban government (unemployment, social-service provision, violence) in a

period of austerity, the limited powers of local governments to deal with these problems, and the lack of control by Communists over the policies and funding decisions of the central government. In addition, the left-wing alliances that gave Communists a role in local government even where they did not have a clear majority (France, Italy, Japan, Spain) have broken down under the pressures of political change at the national level. Finally, fundamental unanswered questions about their identity, strategy, and goals undercut the ability of Communist parties, despite their deep involvement in democratic politics, to construct essential social and political alliances and to present a convincing left-wing alternative.

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**From Insurrection to Revolution in Mexico. Social Bases of Agrarian Violence, 1750-1940.** By John Tutino (Princeton: Princeton University Press, 1986. xvii, 425p. \$42.50).

For students of Mexican history, of agrarian history and peasant mobilization, and of social revolution, this is an important book. Original in its conception and masterfully executed, this comparative social history of Mexican regions over the course of almost two centuries addresses the fundamental question of why and when peasants rebel (and not so much for what). Offered as an "interpretive synthesis," the book does incorporate primary research by the author on north-central Mexican rural history. The bibliography attests to the author's thoroughness in covering the literature on the subject.

Intrigued by the contrast between 1810, when rural violence on the eve of Mexican independence from Spain did not lead to revolution, and 1910, when a revolutionary transformation of Mexico accompanied the outbreak of agrarian insurrection, Tutino examines not only developments surrounding these two specific periods but also the century in between, paying close attention to regional similarities and differences. He also considers the theories of Barrington Moore, Eric Wolf, and Theda Skocpol in searching for answers.

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Tutino contends that perpetual poverty, deprivation, and exploitation can only lead to the kind of outrage that expresses itself in violent reaction when certain social changes in the countryside cause these longstanding peasant grievances to become "acute," and when peasants perceive political opportunities to act without immediate repression. Such a moment occurs when a deterioration in the peasant's relative *autonomy* (ability to produce subsistence independently, for example, through ownership or access to land) declines without a compensatory rise in *security* (ability to attain subsistence consistently); concurrently, a divided elite weakens the state. In 1810, acute agrarian grievances were limited geographically, and the elite remained united in the face of peasant insurrection. In 1910, much more widespread social deterioration in the countryside was followed by political breakdown. A sustained and highly conscious peasant mobilization forced the postrevolutionary elite to incorporate anticapitalist agrarian reforms, thus preventing a full-blown capitalist development. (Whether the incorporation of agrarian demands weakens, or in fact, facilitates, capitalist development in Mexico can be debated but not within the scope of this brief review.)

Tutino's model results from his critique,

modification, and subsequent synthesis of Moore and Wolf on the one hand, and Skocpol on the other. He faults Moore and Wolf for placing too much emphasis on capitalist incursions, thereby incorrectly identifying class conflict as the sole or primary force in social change. Skocpol's corrective goes too far in another direction, attributing too much importance to international causes for the breakdown of the state, which in turn allows for revolutionary insurrection. For Tutino, it is the *dependent* nature of Mexico's capitalist development—capitalism "forced" by an elite intent on closing the gap—that produces the conjuncture of acute grievances and state breakdown that makes revolution likely. In the end, however, Tutino affirms more than disputes Moore, Wolf, and Skocpol. His revision is tinkering and borrowing, not a major overhaul or comprehensive theoretical reconsideration. This book will be valued for some time as a significant contribution to our understanding of modern Mexico, and less for its impact on the general theoretical debate on peasant mobilization and social revolution. Perhaps that is the way it should be, for Tutino is first and foremost a very good historian.

EVELYN HU-DEHART

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## INTERNATIONAL RELATIONS

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**A Changing International Division of Labor.** International Political Economy Yearbook Series, vol. 2. Edited by James A. Caporaso (Boulder: Lynne Rienner, 1987. xiii, 249p. \$26.50).

The editor of this volume sets out three extremely broad goals for this handbook: (1) to identify the changing role of labor in the global production process; (2) to see what part labor is playing in organizing and shaping the new international division of labor; and (3) to analyze the major causes of structural evolution of the work forces in the advanced industrial countries and to see what relationship

these factors have to comparable changes in less-developed countries.

This volume is of limited value as a handbook. It does not contain any systematic empirical presentation of the basic data that one would want before attempting generalizations in this field. Nowhere does it contain even rudimentary survey data on the size, composition, and income of national work forces. Nor is there sufficient comparative data to permit making judgments about how labor has fared under different types of economic-policy management. Although literally dozens of theoretical concepts are discussed or referred to, there is no common theme or methodology

that integrates the chapters. It is thus reasonable to assess this volume as a collection of essays.

As individual essays, the chapters vary considerably in quality. The Higgott and Mytelka chapters are excellent and the Skak chapter is very insightful though a bit thin on quantitative justification of its principal claim. The other three chapter writers (Caporaso, Junne, and Jacobsen) are so mired in jargon and prone to sweeping generalizations that they are not very helpful for explaining their respective subjects. Although all of the authors either accept or show a fascination with assorted Neo-Marxian terminology in their analysis, the strong chapters are the ones that formally link their terms to quantitative evidence.

The Higgott essay, "The Dilemmas of Interdependence: Australia and the International Division of Labor in the Asia-Pacific Region," is a superb country study. It carefully documents the composition and patterns of growth in Australia's exports and imports and concisely states the principal problem facing Canberra's economic-policy managers: whether to continue with the country's current comparative advantage in raw-material exports or whether the long-run terms of trade make it more advantageous to shift focus and concentrate on high valued-added manufactures? As a high-income, well-educated, labor-scarce nation, Australia raises some atypical policy issues and Higgott deftly analyzes the trade-offs available, given the current factor proportions. He also notes the uncertainty that policymakers face when launching programs in sectors where local entrepreneurs have limited experience. Higgott concludes by commenting sensibly that analysts can disaggregate the economic choices into discrete areas but that political necessity (historical commitments or domestic and international constraints) often overrides and has an autonomy of its own.

The Mytelka chapter, "Knowledge-Intensive Production and the Changing Internationalization Strategies of Multinational Firms," is very ambitious and addresses a critical theoretical and policy problem. Mytelka attempts to identify how changing patterns in the generation and use of knowledge are affecting the global production process. Thus, in economic terms, she is trying to put human-capital theory on a par with other factors, such as changes in the composition of demand (i.e., the shift toward

the service sector) and the structure of international capital markets.

The strength of the Mytelka chapter is that it directly links the theoretical parts with empirical evidence. Mytelka starts by showing the percentage increase of engineers and scientists in developed-country labor forces during the past two decades and by noting that research and development (R&D) costs are inversely correlated with an industry's product life cycle. She then shows how the decentralization and internationalization of knowledge generation has posed particular problems for multinational firms. Mytelka further documents various techniques firms have used to deal with the new circumstances: development of world products, flexible use of joint ventures, and links to university R&D capabilities. She concludes with two broad theoretical claims: multinational firms are coping with the rising costs and uncertainties of knowledge production by (1) attempts at cost minimization, and (2) efforts to control the markets in which the products are sold. The first claim is persuasively argued in the chapter; the second needs further elaboration. Overall, the significance of the arguments and the clarity of the exposition make this an impressive essay.

The Skak chapter, "The New International Division of Labor and East-South Relations," has a provocative and interesting theme. The central contention is that the European members of the Marxist countries' Council for Mutual Economic Assistance (CMEA) are in an increasingly competitive position with the newly industrializing countries (NICs). Both sets of countries are at "middle" levels of industrial development, are competing to import technology, and are anxious to get access for their exports in the industrial democracies. Although Skak's discussion occasionally over-emphasizes the importance of East-South trade per se (only 1.5% of world trade in 1982), the basic issues raised here are quite significant. If the NICs continue to grow at a rapid rate while the CMEA countries proceed at a desultory pace, both the economic and political implications will be portentous. In a skillful manner, Skak notes that Soviet research analysts are very conscious of the same issues and have recognized that differing levels of economic performance among the developing countries and among the CMEA countries will shape future political options. This particular chapter

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would have benefited from more exploration of the reasons behind the NICs' success, but the central theme is compelling and these issues should be integrated into future general discussions of international political economy topics.

Each of the other chapters in this volume has fundamental problems. The theoretical overview provided by Caporaso touches on so many subjects that few of them are dealt with adequately. Although the tour through medieval and renaissance concepts is erudite, the discussion of how neoclassical economists deal with labor neglects production functions, investment theory, human capital, and relative price shifts. These omissions are combined with frequent vague phrases like "the cultural economy" and assertions that the Marxian approach to the global political economy is "open (even if coercively), integrated (even if hierarchically), and cosmopolitan." The concluding Caporaso chapter is, unfortunately, another literature survey rather than an attempt to unify the themes of the volume or to summarize empirical material.

The Junne chapter, "Automation in the North: Consequences for Developing Countries' Exports," addresses a very important topic, but its methodology is flawed. Although there is one citation claiming that robots in developed countries account for "about 40% of labor cost in manufacturing," one wonders to which sectors this applies and how the computation was done. Distressingly, Junne moves from claims that automation is sweeping developed country industries to other broad claims about policy, with little quantitative support for his argument. The Jacobsen chapter on Ireland is in a similar vein. A bewildering variety of comparisons are made, but the central contention that "peripheral postindustrializing" nations face a set of distinctive problems is not well demonstrated.

In sum, this volume is not a handbook and the chapters are quite varied, but it has several impressive essays that deal with key international political economy problems.

DAVID B. H. DENOON

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**Strategic War Termination.** Edited by Stephen J. Cimbala (New York: Praeger, 1986. xvii, 227p. \$37.95).

The idea of ending a nuclear war at the lowest possible level of death and destruction is clearly a very important concern. The professional and scholarly literature on preventing nuclear war is mountainous if often uneven and ideological, but public sector studies on terminating a nuclear war once begun are virtually nonexistent. This paucity is somewhat strange given that it has been administration policy for several years to end such a war at the lowest possible level of violence and on terms favorable to the United States.

This imbalance certainly reflects to a large extent the liberal intellectual bias of U.S. academic analysts of nuclear deterrence. In the standard view, deterrence simply cannot be allowed to fail; thus all energies must be devoted to maintaining the deterrent condition. Moreover, thinking about how nuclear war might be fought and ended in a manner short of an assured destruction cataclysm is often held to make nuclear war somehow more likely.

The problem with conventional thinking about nuclear dynamics that focus exclusively on deterrence or the first "campaign" of a nuclear war is that they omit serious consideration about how to end such a war. The authors of this compendium of original essays commissioned by volume editor and contributor Stephen J. Cimbala individually and collectively agree that this is a serious deficiency. As a number of them argue, the actual circumstances of nuclear hostilities, likely to be filled with the most incredible Clausewitzian "fog and friction" in the history of warfare, are hardly going to be conducive to rational, dispassionate analysis about how to end the war. The absence of serious consideration of war termination during the calm of peace could, indeed, make the escalation of nuclear war to an all-out exchange more likely, even a "self-fulfilling prophecy" as Barry Schneider puts it (p. 124). Beginning to fill this vacuum is the avowed purpose of this volume.

Cimbala has assembled an impressive roster of analysts to examine the problem. They include, in addition to the editor himself, William C. Martel, David W. Tarr, George H. Quester, Colin S. Gray, Leon Sloss, Paolo

Stoppa-Leibel, Barry Schneider, Carl H. Builder, Robert Mandel, and Clark C. Abt. Cimbala gave each author considerable leeway both methodologically and substantively, and the result is a diverse set of perspectives on the problem. These range from examinations of the mechanics of nuclear war and its termination, the effects of different weapons systems and innovations on war and its ending, and notions about the costs and dynamics of nuclear war prevention and termination.

The various authors also present very different levels of optimism about the prospects of terminating a nuclear war. In one of the book's most revealing chapters, Colin S. Gray, long an advocate of the nuclear "war-fighting"—and even war-winning—school of deterrence, writes a very pessimistic chapter on the likelihood of stopping short of all-out exchange that should be read both by Gray enthusiasts and Gray detractors.

I found two problems with the book, one unavoidable, one not. First, the book is highly speculative, as indeed it must be in a world where there is absolutely no empirical evidence about the nature of nuclear war. A number of the papers attempt to make the study appear somehow scientific and substantive, but it is a hoax in the end. Thus, Martel draws elaborate alternative damage scenarios based on computer modeling in the opening chapter, and Mandel presents a large number of hypotheses about the war and its ending in his chapter. In the end, however, one is forced to admit that there are simply no facts on which to rely and build.

Secondly, in my view, the most important question about nuclear war—and thus about its termination—is why it might begin. In other words, if the purpose of all warfare is to achieve some political ends, what political purposes are adequate to initiate and conduct nuclear war? This question is central to deterrence because it defines the problems that require deterring. It is equally central to terminating nuclear war, since doing so presupposes either that certain goals are achieved or can be compromised. Unfortunately, it is a question that is nowhere seriously examined in this volume.

As Cimbala quickly admits in the preface, this is a first cut at, and not the last word on, the subject of terminating nuclear war. As a first effort in an area that has suffered from in-

tellectual "benign neglect," it is a good and valuable effort. Its value is not so much in the answers it provides, however, as in the debate that it stimulates.

DONALD M. SNOW

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**East-West Rivalry in the Third World: Security Issues and Regional Perspectives.** Edited by Robert W. Clawson (Wilmington: Scholarly Resources, 1986. xxv, 348p. \$39.00).

**The Soviet Union in the Third World: Threat to World Peace?** By Joseph G. Whelan and Michael J. Dixon (Washington: Pergamon-Brassey's, 1986. xlvii, 486p. \$27.50).

We all know about animals created by committees. Both these volumes suffer a bit from the "committee" problem. The contributed chapters in the Clawson-edited volume are of somewhat uneven quality, and the book ends with a bump. A broad summing up at the end would have helped. The "committee" that influenced the character of the Whelan and Dixon book is the House Foreign Affairs Committee of the U.S. Congress. The book is essentially a collection of Congressional Research Service reports that were first pulled together and published as a congressional study.

So far as the Clawson-edited book is concerned, there are some excellent chapters. The collection resulted, by the way, from an April 1984 conference at the Lyman L. Lemnitzer Center at Kent State University. The best chapter of all is probably Boleslaw Adam Boczek's sensible and balanced consideration of the possibility of a "resource war" between the superpowers, in southern Africa, the Persian Gulf and in other Third World areas. Boczek concludes that such an all-out war—and Soviet victory in one if it should come about—are unlikely. The article by Louis J. Andolino and Louis R. Eltscher on Soviet power-projection capabilities is sober, competent, and well informed. It is not supportive, by the way, of the more extreme scare stories about Soviet naval superiority. Timothy W. Viezer's article on the Soviet invasion and occupation of Afghanistan is lively and interesting. It contains a few nuggets that may intrigue even those students who thought they



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had already read all there was to read about the 1979 invasion in Henry S. Bradsher's series of authoritative and masterful works. The introductory historical overviews that start the Clawson volume, written by Lawrence S. Kaplan and the editor, are well done if not substantively very startling.

The last third of the Clawson volume consists of "regional perspectives," or continent-by-continent analytical summaries of fairly recent developments. How one gets around the publication lag problem, incidentally, is hard to know. It is now over three years since these chapters were written. In any case, Robert I. Rotberg writes in his usual breezy and fact-packed style about the superpowers in Africa. He concludes that the Soviets are reeling from setback to failure in their efforts throughout the continent. He may be right, but his judgments in this regard could be a trifle premature. The other regional articles are less provocative and also somewhat less interesting.

The regional articles in the Clawson book cover some of the same ground that Whelan and Dixon traverse in their volume, *The Soviet Union in the Third World*. The Whelan-Dixon book's publisher, Pergamon-Brassey's, fudges in claiming that for speed of publication and economy of cost, "the author's typescripts have been reproduced in their original forms." What the publisher should have said is that the House Foreign Affairs Committee study, published in 1985, has been reproduced between Pergamon-Brassey's cardboard covers—typos and all. For example, one of the chapter subtitles is "Soviet Strategic Interests in America" (for Africa; p. 206).

In fairness to the publisher, the congressional study was never put on general sale, although copies were available for some time without charge from the House Foreign Affairs Committee and the Congressional Research Service. But for Gramm-Rudman, no doubt they still would be. The congressional study was in large part itself a republication of Congressional Research Service reports of 1983 and 1984, although Joseph G. Whelan updated his Latin American section for the 1985 congressional study.

So much for the odd provenance of the book. So far as substance is concerned, both Whelan and Dixon are clearly able, careful scholars and the study contains a wealth of

useful material. Like many other government documents, it tends to have a lot of "on the other hand," and makes its points by selective quotation of academic scholars rather than by unvarnished assertion. This is not to say that the authors' views fail to come through. Whelan describes the USSR as having an "internal" empire (the minority communities in the USSR), a "contiguous" empire (the Eastern European satellites), and an "empire abroad" (India, Syria, etc.). The Indians would not be pleased. After characterizing the Soviet Union as a militarized society, Whelan quotes Arnold Toynbee: "Militarism breaks a civilization down . . . in destructive fratricidal conflicts. In this suicidal process the entire social fabric becomes fuel to feed the devouring flame in the brazen bosom of Moloch" (see pp. 444, 450).

Nevertheless, for all of their hard-line views, Whelan and Dixon are perceptive, extremely well informed and excellent students of Soviet policies and actions in the Third World. It might be worth closing with a contrast between two judgments, one expressed in the Clawson-edited book and the other presented in the Whelan-Dixon volume. In the Clawson-edited book, Gary R. Hess is discussing Soviet aid to Vietnam: "Considering the strategic advantages offered by the alliance with Vietnam, the economic and diplomatic costs are certainly reasonable" (p. 304). On the same subject, Whelan writes: "Vietnam is an expensive ally. . . . The Soviets pay a heavy price" (pp. 113-19). Interestingly, both books indicate that the Soviets are spending several billion dollars a year to support Vietnam; so "reasonable" and "expensive" are in the eyes of the beholder. As between the two judgments, Whelan's analysis appears closer to the mark.

NATHANIEL DAVIS

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**Foreign Policy Decision Making: The Influence of Cognition.** By Martha L. Cottam (Boulder: Westview Press, 1986. xiii, 262p. \$26.50).

The author of this book addresses three tasks. One is to provide a critical appraisal of previous research efforts to establish a link between the cognitions of decision makers and their foreign policy decisions. A second is to

focus explicitly upon a model of cognitive processes that establishes this link. A third is to test the model of cognitive processes by examining the cognitive processes of U.S. decision makers in both an artificial and a natural setting. The results of these efforts are generally informative and provocative.

The generally informative aspect of the book is mainly in the discussion of the findings by cognitive psychologists regarding the processes of forming categories; making judgments about which objects fit into these categories and to what degree; and integrating into the process of judgment the context provided by the categories and the circumstances surrounding the object. For example, the author notes that this literature distinguishes among three types of categories: the "most abstract level," which provides little differentiation among objects; the "basic level," which "divides the environment into 'natural discontinuities' and therefore is the most cognitively efficient level of abstraction"; and the level of "'subordinate' categories whose members share attributes [that] overlap with those of other categories" (p. 37).

The author attempts to make the case that the "basic category" foreign policy decision makers use is "type of state." She asserts that seven types of states are identified by decision makers: enemy, ally, neutral, hegemonist, dependent ally of the enemy, dependent of the perceiver's state, and puppet of the perceiver's state (p. 50). The contents of each of these categories contains scripts that identify the expected behavior from states in each category and the appropriate responses by the decision maker; there is also a prototypical example of a state that fits into the category.

Cottam hypothesizes that these categories influence the foreign policy decisions directed toward a state that the decision maker judges as fitting into a given category—the closer the fit between a given state and the prototypical state in a category, the greater the influence of the scripts associated with the category, *ceteribus paribus*; that is, unless there is a disruption of this relationship by contextual influences, the expectation scripts will influence the decision maker's perception of the other state's behavior; the response scripts will influence the decision maker's policies toward the state.

The author offers evidence to suggest that decision makers do use "type of state" as a basic category. The data come from a questionnaire survey of U.S. foreign policy elites who were asked to group selected states by type of foreign policy and then label the groups. The author's analysis of the results indicates that these respondents grouped the states into all but one of her seven basic categories. In addition to this artificially generated evidence, Cottam also offers an interpretation in a natural historical setting of U.S. foreign policy toward Mexico. She argues that contextual influences caused many U.S. decision makers to categorize Mexico as a dependent state during the 1970s when it really had the properties of a neutral state. Consequently, the wrong scripts were activated in the minds of key decision makers, which led to policies that caused negotiations over natural gas imports to fail in 1977. They succeeded in 1979, on the other hand, because U.S. decision makers recategorized Mexico as a neutral state.

The provocativeness is a product of the author's willingness to take a position on several issues facing foreign policy analysts. For example, she attempts to locate the contribution of her cognitive process model within the cognitive tradition by contrasting it with shortcomings of the operational code and cognitive map models of the link between cognitions and foreign policy decisions. Cottam also asserts that her interpretation is a better explanation of U.S. foreign policy decisions toward Mexico during negotiations over the import of Mexican natural gas into the United States than explanations offered from a power politics, domestic politics, or a bureaucratic politics perspective.

STEPHEN G. WALKER

*Arizona State University*

**Technology, Strategy, and Arms Control.**

Edited by Wolfram F. Hanrieder (Boulder: Westview Press, 1986. ix, 162p. \$28.00 cloth; \$13.95 paper).

This slim volume contains adapted scripts of lectures presented during 1984-85 to the Program of Global Peace and Security of the University of California, Santa Barbara. The areas of expertise of the nine contributors

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vary, but the political stance is uniformly moderate, favoring some form of United States-Soviet Union arms limitation. A further shared premise is that negotiated mutual reduction is the preferred means for arms control. There are neither Weinbergers nor Sagans, here.

The most critical stance is that of Richard Ned Lebow in "Assured Strategic Stupidity: The Quest for Ballistic Missile Defense," which cites Weinberger and relies on an earlier Union of Concerned Scientists publication jointly authored by Sagan, Lebow, and others. Lebow gives an overview of how a ballistic missile defense operates in theory, and criticizes the proposed Strategic Defense Initiative for undermining deterrence, increasing risks of use of nuclear weapons in crises, increasing stockpiles of offensive weapons to compensate for the percentage reliability of mutually deployed BMDs, abrogating the Anti-Ballistic Missile Treaty, and extending the arms race to new technologies and into space. This is the longest (24 pp.) and most extensively annotated entry and is more informative than several of the others in this uneven collection.

Herbert York and Paul Warnke each contribute a short historical overview of U.S.-Soviet negotiations and nuclear competition. The volume concludes with a brief moral argument for arms limitation, quoting extensively from others, by Catholic Bishop Thomas Gumbleton. These three papers provide context for more technical discussions in the volume but are clearly aimed at their young undergraduate audiences.

Chapters by Jonathan Dean and editor Wolfram Hanrieder discuss the European context of U.S.-USSR arms negotiations. Dean focuses on discussions between and within NATO and the Warsaw Treaty Organization in both Conference on Disarmament in Europe, and Mutual and Balanced Force Reductions talks and proposes an integrated regime for comprehensive negotiations on arms control in Europe. Hanrieder concentrates on the role of West Germany in U.S. management of NATO and criticizes the first Reagan administration for lessening the credibility of U.S. intentions to control arms and undermining West German faith in U.S. commitment to deterrence. He suggests a move away from the militarization of diplomacy but avoids specific policy recommendations.

Three essays discussing varied conceptual approaches round out the volume. Robert Hoover, in a well-organized and informative piece, contrasts presidential and bureaucratic politics paradigms for analyzing U.S. national security decision making, and suggests that actual policy-making, as in case studies of SALT and START, incorporates broader coalition politics. Kurt Gottfried raises probing questions on the impact of changing technologies, especially in deployment of Antisatellite Systems on deterrence and global security. He urges the scientific community to be active in the creation of an independent, "sound scientific advisory system" (p. 126). Ernst-Otto Czempiel argues for transforming the current East-West deterrence regime into one in which the conflict operates on a political rather than a military level. Defining the East-West competition in terms of antagonistic "isms," Czempiel musters historical evidence to advocate reliance on intergovernmental organizations for international conflict resolution.

The strengths of these essays are accessibility and breadth, ranging from relatively detailed discussions of ballistic missile defenses and negotiating stances to ethical appeals for global responsibility. The contributors do not differ significantly with each other on analyses of causes for the arms race or prescriptions for policies to pursue global security. A more comprehensive discussion of U.S. weapons deployment and the policy-making that underlies it is Paul B. Stares's *The Militarization of Space, U.S. Policy, 1945-84* (1985). A compendium with both more technically detailed and more politically diverse essays is *Weapons in Space*, edited by Franklin A. Long, and colleagues (1986). Each of these testifies to the inherent problems of the Hanrieder volume in gathering lectures designed for an undergraduate audience, regardless of the expertise of the lecturers.

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**Small Is Dangerous: Micro States in a Macro World.** Edited by Sheila Harden (New York: St. Martin's, 1985. ix, 212p. \$25.00).

This slim volume embodies the report of a study group sponsored by the David Davies

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former United States ambassador, a business professor, and an insurance expert. As would be expected from this diverse group of authors, perceptions of political risk vary greatly.

After a good general overview by Raddock on the types of domestic factors that might affect political risk in a given host country (e.g., regime legitimacy, leadership succession, ethnic conflict, civil disorders) and a very brief chapter, also by the editor, on financial and economic risk (e.g., debt service ratio, current account, currency competitiveness) there are five chapters by country specialists on political risk in the Soviet Union, China, Venezuela, Saudi Arabia, and Australia. The next two chapters present two different methodologies for forecasting political risk, each of which has been in existence for some time. One involves the "bootstrapping" approach, which attempts to "model the hidden cognitive process of the individual decision-maker(s)" (p. 157). The other depicts the "prince" system of political forecasting, which relies heavily upon expert opinions for identifying the key actors influencing the political climate in a given host country. The final two chapters deal with managing financial risk and political risk insurance.

With such a plethora of topics, Raddock is quite correct in stating that some of the chapters "can stand on their own and serve as examples for economists and political scientists to demonstrate how their own formal disciplines can be bent and interwoven to have a direct impact on bolstering profits in the business world" (p. 199). However, it is questionable whether he has succeeded in establishing "a theoretical interdisciplinary foundation for country risk analysis" (p. 199).

For such a foundation to be laid, there would need to be explicit theories, or even pre-theories, of political risk proposed. This would entail identifying a series of testable propositions linking the many different variables that can directly or indirectly lead to political risk. A conceptualization of the varying effects that host government policies and actions, host societal developments, and events in the external environment can all have upon the formation of political risk would be necessary. While Raddock does include a descriptive chapter on host domestic factors, these variables are not explicitly linked to a theory of political risk.

For political risk to develop into a full-fledged discipline, there will also have to be an

effort to build cumulative knowledge. That, however, goes against the understandable needs of the corporate planner who has to know, sometimes under a deadline of one or two weeks, what the political future of a given country is likely to be. This tension between the immediate applied side of political risk analysis, and the more long-range need for scholarly work remains a central problem for the profession. In that respect, Raddock's introductory statement that he has "drawn little from the field of political risk analysis and strategic planning because none of the books [or] articles . . . has more than skirted the subject or told part of the story" (p. xvii) does not help in the effort to build a discipline, and it much too quickly dismisses all previous work done in political risk analysis.

Nevertheless, this book will be of interest to those who want to become generally acquainted with political risk, and for those corporate decision makers who may want an overview of some of the types of political factors that may affect foreign investment.

JEFFREY D. SIMCN

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**Revolutionary Iran: Challenge and Response in the Middle East.** By R. K. Ramazani. (Baltimore: Johns Hopkins University Press, 1986. 311p. \$27.50).

Since the taking of U.S. embassy personnel as hostages in Teheran in November 1979, Iran has been identified as one of the main planners, financiers and supporters of international terrorism against the United States. The U.S. foreign policy establishment has conducted a concerted campaign, bordering on paranoia, to depict Iran's rulers as lunatic, fanatic, irrational, and dangerous. Paranoia in the conduct of policy, Richard Hofstadter argued, creates a paradox as it moves the policymakers toward "the imitation of the enemy," real or perceived. Thus, if Ayatollah Khomeini calls the United States the world's Great Satan, the Reagan administration considers Iran the Middle East's Satan. Consequently, U.S. policy, at least publicly, has been based on a rigid containment of Iran. A comprehensive and refreshing study of the foreign policy of the Islamic republic in the Middle East between

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to weaken and defeat the moderates accused of pro-Americanism. In managing Iran's military and economic systems, however, they have recognized the depth of Iran's dependency on the United States, a dependency created because of Iran's close ties with Washington in the post-World War II period. Thus, the fundamentalists have been torn between the requirements of a dependent system that they inherited from the Pahlavis and the rewards of verbal attacks on "U.S. imperialism" to mobilize support for keeping the revolutionary spirit alive. The net result has been secret negotiation with the United States to satisfy Iran's dependency and public condemnation of the Great Satan for internal consumption.

Enlightening, provocative, and well argued, this book is a must for students of the Islamic Revolution and a valuable text for courses on the Middle East.

MOHSEN M. MILANI

*University of Southern Florida*

**Indian Security Policy.** By Raju G. C. Thomas  
(Princeton: Princeton University Press,  
1986. xiv, 312p. \$37.50).

Since the late 1970s, India has undergone its most impressive military buildup since independence. Defense spending has increased almost 200%. A series of new arms deals have modernized the armed forces with about \$20 billion in new equipment. In his new book, Raju Thomas shows that Indian governments, like the Reagan administration, find it easier to increase military spending than to explain what all the new capability is supposed to do.

Thomas, professor of political science at Marquette University, previously wrote about Indian security politics in his 1978 book, *The Defence of India*. It remains the best introduction to the subject. His new volume recapitulates and builds upon this proven foundation. Many of the new arguments were rehearsed in a series of journal articles, so specialists will not find a lot of fresh meat here. But for newcomers, Thomas sets just the right tone. Indeed, the most significant aspect of Indian security policy-making may be how little it has changed over the years.

India faces several serious security prob-

lems, including civil strife, continuing worries about China and Pakistan, as well as the regional presence of the superpowers. To cope with this situation, New Delhi has assembled the world's fourth largest military establishment. Yet 40 years after independence, the government has never issued a defense white paper or a posture statement laying out its strategic objectives or long-range goals.

India's security policy-making is a curious mixture of the gyroscopic and the chaotic. Thomas's portrait of the armed services and military industries reveals a model of bureaucratic stability. As elsewhere in the world, big defense programs take on a life of their own. Helped by big budgetary increases, they advance toward popular goals. The government-owned military industries pursue self-sufficiency in modern technology. The army maintains its 30 divisions, while the navy grows towards three carrier groups and the air force modernizes its 45 combat squadrons.

Although there is a healthy public debate on defense issues, it has not fostered serious official investigation of these fundamental defense programs. Are they the best way to assure Indian security? Doubts are expressed, but not in the Congress party-controlled parliament. Nor is there serious questioning of the 70-20-10 rule for apportioning funds between the army, air force, and navy. The budgetary formula is the closest thing to a national security doctrine that India has.

At the cabinet level, however, decision making can be unpredictable, even panicky. Thomas's natural caution precludes direct criticism, but the picture he presents is unambiguous. India has become a major military power, but it still relies on the decision-making apparatus of a small and isolated country. Institutional arrangements tend to be unrefined. They often go unheeded. Civilian leaders long ago grew accustomed to disregarding military opinion. Major decisions usually are made by the prime minister personally with a handful of close advisors.

Policy is guided by the shibboleths of non-alignment, development, and self-reliance. But these provide little practical guidance. In lieu of long-range goals or strategic planning, decisions often reflect crisis reactions. For example, despite achieving regional supremacy by the time of the 1971 war, Indian officials were acutely sensitive to any Pakistani arms acqui-

sition. When Washington offered Islamabad \$1.6 billion of new arms in 1981, including 40 F-16 fighters, the Indian press pictured skies black with the planes. New Delhi forgot about its own \$4 billion of recent warplane purchases and sought new aircraft from several suppliers. Reflecting this climate, Thomas joins Indian leaders who refuse to believe that their new Mirage 2000s and MiG-29s are comparable to Pakistan's older U.S. aircraft. For insurance, New Delhi wants superior quality and greatly superior numbers. Should Pakistan acquire AWACS or test a nuclear device, India would no doubt follow a similar course.

The obscurity of Indian security policy-making leaves one hungering for more insight. Why have defense expenditures increased so much? In reaction to Pakistan? In pursuit of regional hegemony? What of the consequences of Soviet involvement in Afghanistan? What of the future direction for the nuclear weapons program? Thomas sticks close to the official debate. He has surprisingly little to say on these questions.

One strength of the book is its discussion of internal security problems. Civil strife and sectarian violence have grown more serious, with secessionist movements in several parts of the country. Thomas shows how India has built a panoply of paramilitary forces to cope. Like the professional military, he hopes the armed services can be kept out of these affairs.

AARON KARP

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**Multilateral Development Diplomacy in UNCTAD.** By Thomas G. Weiss (New York: St. Martin's, 1986. xviii, 187p. \$30.00).

In prior works, Thomas G. Weiss has argued that international organizations such as UNCTAD (United Nations Conference on Trade and Development), perform a valuable role in the international politics of agenda formation. In this book, Weiss concedes that although UNCTAD's group bargaining system has contributed greatly to solidarity among developing countries and launched a multilateral development dialogue between North

and South, it has outlived its usefulness and has become a barrier to a more equitable international economic order. Thus, this book asks, how can multilateral development diplomacy in UNCTAD be restructured to make the most of existing and future opportunities in negotiations between North and South?

Because UNCTAD has failed to translate general principles into specific policy outcomes through negotiations, Weiss begins his analysis with a scrupulously fair, but frank, insider's account of where UNCTAD has gone wrong. Weiss acknowledges that not all the blame rests with UNCTAD's organizational and procedural shortcomings. UNCTAD's recent efforts to improve the lot of the developing world is also constrained by slow world growth, Western intransigence, and the often flawed domestic policies of developing countries. Nonetheless, Weiss demonstrates that the UNCTAD system of group bargaining, which pits a huge block of diverse undeveloped and developing countries against the developed world, impedes successful negotiations as well. Specifically, the current group arrangement leads developing countries to prepare excessive and inclusive lists of demands, adopt radical positions, and offer inflexible, one-sided, and often ill-prepared initiatives. These aspects of the current group system contribute to unfocused negotiations that promote southern solidarity while hiding real differences in the developing world, and, more importantly, impede progress toward global redistribution of wealth.

Weiss offers an imaginative, yet realistic, "alternative image" of multilateral development diplomacy as a way out of the current stalemate. The essential elements of his alternative approach include: (1) flexible coalitions of countries based on interests rather than ideology; (2) concentration on specific issues on which there is some room for compromise and some possibility of building further agreements; and (3) proper analytical and political analysis—spearheaded by a more technically competent and less partisan UNCTAD Secretariat—before negotiations.

The author's recommendations are sound and founded on lengthy personal experience. Moreover, Weiss's insights reflect a balance of pragmatism and optimism that is indeed rare. Nonetheless, the question the author poses is

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unfortunately limited to what can be achieved in UNCTAD, and his analysis is unnecessarily confined to his own and others' observations about UNCTAD.

Equally important are the questions whether multilateral development diplomacy can best be restructured inside or outside the UNCTAD system and a consideration of the relative merits of alternative multilateral, bilateral, and plurilateral approaches to development diplomacy. For example, many developing countries, recognizing that they need the protection of multilateralism and most-favored-nation treatment, have already subordinated group politics in UNCTAD for a more active role in the preliminary stages of the current round of General Agreement on Tariffs and Trade negotiations. Weiss does not address these issues, however.

Similarly, the author's answer to

UNCTAD's problems draws only from his first-hand knowledge and from the prior literature on UNCTAD. Although multilateral development diplomacy may be a recent phenomenon, it is disappointing that in assessing UNCTAD's shortcomings Weiss brings to bear no lessons from prior or contemporary works on conference diplomacy, multilateral negotiations, organizational literature, or even world politics.

In short, this book offers an imaginative and reasonable answer to the question the author has posed. Ultimately, the author's contribution may be constrained by the declining relevance of UNCTAD to the future course of development diplomacy.

WILLIAM J. LONG

*Columbia University*

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## POLITICAL ECONOMY

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**Latin American Political Economy: Financial Crisis and Political Change.** Edited by Jonathan Hartlyn and Samuel A. Morley (Boulder: Westview, 1986. xiii, 386p. \$38.00 cloth; \$16.95 paper).

The field of Latin American comparative politics has been rather dominated by various types of "political economy" approaches. Nevertheless, in a region tormented by crises of development, a slowdown in growth since the 1960s, and present financial chaos, urgent questions remain regarding the nature of the linkages between states and markets, political societies and productive structures, accumulation and legitimation. Political economy should be more than economics with a sprinkling of references to policies, and more than political sociology with a bias towards explanations rooted in classes and economic sectors. This book brings together a number of valuable contributions to a field in which controversy too often rages without evidence.

Hartlyn and Morley have organized these

essays (many first presented to the International Conference on Models of Political and Economic Change in Latin America at Vanderbilt University in November 1983) into five comparative chapters that form the first third of the book, followed by eight case studies: Argentina, Brazil, Chile, Peru, Mexico, Colombia, Cuba, and Nicaragua. This makes the second two-thirds more useful for teaching and reference purposes than as an exercise in new theory building, or comparative theory testing. Some might have preferred the editors to establish a common time frame and theoretical agenda of questions to be addressed by the large number of contributors. The strategy seems rather to have been to survey as much of the region as possible (hence the inclusion of a socialist economy in the case of Cuba), and readers are left to draw their own conclusions from the case studies.

Perhaps the most stimulating ideas emerge from the comparative essays. Though they omit Uruguay, the editors' review of the performance of Bureaucratic-Authoritarian (BA)

regimes convincingly disconfirms the view that authoritarianism leads to faster growth than democracy. They argue for a distinction between countries that have implemented pendular policy swings, and thus experienced stop-go cycles rather than sustained growth (Argentina, Chile, Peru), and those that have pursued more consistent policies and thereby grown steadily (Colombia, Mexico, Venezuela).

Albert Fishlow's comparative essay, while occasionally more econometric than many political scientists will appreciate, provides a very important critique of IMF stabilization policies. A particular merit of Fishlow's approach is that he includes various contrasting Asian cases in his study of adjustments to the oil shocks of 1973 and 1979. An essay by David Felix usefully details how BA regimes adopted very different economic policies, though what determined policy choices remains unclear. Brazil, in particular, committed so many *dirigiste* "deviations" that it bore little resemblance to the neoliberal experiments of the Southern Cone. The original BA model proposed by O'Donnell did not really predict the virulent neoliberal policies of the 1970s. Nevertheless, both types of policy—developmentalist and neoliberal—ended in "financial blowups," causing Felix to conclude with an intriguing sociocultural hypothesis. This is that free trade is less likely to produce sustainable growth in Latin America than in Asia because Latin American elites are far more thoroughly imbued with Western tastes. Import liberalization merely leads to an unsustainable boom in luxury imports, rather than a balanced process of competitive outward-oriented growth. This reviewer (a Latin Americanist) would be interested to hear whether Asianists find this notion plausible.

The country studies (most by nationals of the country under scrutiny) are almost all very strong and provide a lot of detail on the vicissitudes of economic policies and performance over the past decade. In a review of this length there is no room to mention all of them. Perhaps the most interesting is the chapter by Marcelo Diamand, which introduces English-speaking readers to the author's theory that the blame for Argentina's perplexing slide into relative underdevelopment may lie in the strength of its export agriculture. Argentina's very competitiveness in beef and grain led to a

constant overvaluation of the exchange rate, much as oil bonanzas have done in other nations. This provocative hypothesis deserves further elaboration and testing. The book seeks a more explicit focus on the political aspects of economic policy swings in what has been the worst case of "stop-go" in the region. It is an attempt to supplement Diamand's account with the writings of other Argentine economists, such as Guillermo O'Donnell and Marcel

The Brazilian case study by Larissa Moura focuses on the complex interplay between the military's decision to open the political "opening" after 1974 and a time increase growth in order to get through the first oil shock. It is a convincing narrative. Whereas other chapters focus primarily on economic trends, the impressively synthesized essay by Manuel Antonio Garretón offers an elegant structural-historical account of Chilean political polarization and social mobilization in the 1960s, brief socialist experiment and subsequent neoliberal military dictatorship. In many ways this chapter complements Paul Cammack and Phil O'Brien's *Generals in Retreat* (1985), which includes a detailed essay on the Chilean economy under Pinochet by William Smith.

The chapter on Colombia, a comparatively stable democracy since 1958, by Revéiz and Pérez, provides some interesting contrasts to the instability of the other countries, and suggests that the secret of steady growth lay in the ability of certain producer groups to "capture" different state agencies. For those who see "state autonomy" as conducive to economic development this is a provocative finding. Readers are left wondering, however, why the authors currently perceive such a severe crisis and how they explain it. Ayala and Durán's discussion of Mexico makes a welcome effort to link policies before and after the oil bonanza to a discussion of the class forces acting through a hegemonic one-party regime.

Though this book appeared too soon to discuss the crucial antiinflation strategies recently attempted by fledgling democracies in Argentina and Brazil, it provides a great deal of stimulating discussion for those interested in the constraints economies and politics place on one another in a region of the world where extremes sometimes seem to be the norm. The sad fact is that while there is a lot of political economy written about Latin America by



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Latin Americans, policy-makers too often rely on imported theories under pressure from international agencies.

CHARLES GILLESPIE

*Amherst College*

**Pipeline Politics: The Complex Political Economy of East-West Energy Trade.** By Bruce W. Jentleson (Ithaca, NY: Cornell University Press, 1986. 263p. \$29.95).

*Pipeline Politics* is an engaging, well-researched account of Western energy trade with the Soviet Union during the postwar era. The central analytic claim is that for the United States to have coercive power over the Soviet Union, both domestic and international constraints must be minimized. The former are characterized as a function of ideological divisions and group economic interests, the latter result from the divergent foreign policy strategies and economic interests of the United States and its allies. Jentleson proceeds historically, seeking to shed light on the ability of the United States both to obtain the cooperation of its allies and to use energy trade effectively to achieve instrumental objectives in its relationship with the Soviet Union.

The main contention with regard to alliance politics is that over time the intensity of conflicts has increased, while the sources of U.S. leverage have decreased. Consequently, the United States was more successful (though never fully so) in shaping alliance disputes in the 1950s and 1960s than in the 1980s. While perhaps not striking, this argument is nevertheless carefully documented and convincingly demonstrated, with an insightful emphasis on variations in prestige as a factor influencing the ability of the United States to shape alliance outcomes. Equally important, Jentleson shows that the divergence of interests underlying the 1982 pipeline dispute is a structural one, suggesting the possibility of further alliance conflict over this issue. Whether or not the United States, despite its relative decline, continues to possess sufficient leverage in absolute terms to determine alliance outcomes in conflicts of lesser political and economic salience than the 1982 dispute is a question yet to be answered. While not addressing it in detail, Jentleson's analysis suggests that the United States is likely

to encounter great difficulty in obtaining support, except in areas where alliance interests are more convergent, for instance, CoCom-related trade.

Domestic constraints, like international ones, have become more pressing over time. Congressional opposition undermined the Nixon-Kissinger inducement strategy, while pressure from domestic economic interests helped to compromise the Reagan economic warfare initiative. In light of the breakdown of the Cold War consensus, and the failure of a new consensus to emerge, U.S. officials are destined to confront significant domestic opposition regardless of whether they opt for a trade inducement or trade restriction strategy. In general, *Pipeline Politics* goes further than most studies of economic sanctions in building the domestic politics of the sanctioning state into the analysis. This is an important contribution. The next conceptual step would be to provide systematic analysis of the resources and tactics available to administration officials in their struggle to overcome domestic constraints, an analysis parallel to that provided in the international context.

In assessing the impact of sanctions on the Soviet Union, Jentleson highlights the complexity of U.S. objectives, categorizing them as economic warfare, economic defense, deterrence, compulsion, and containment. This disaggregation, drawing upon the example set in recent scholarship on economic statecraft (e.g., David Baldwin's work), rightly reflects that objectives are rarely one-dimensional and consequently that outcomes cannot be characterized in simple terms of success or failure. The evaluation of U.S. efforts is sketchy at points, reflecting the difficulty of obtaining detailed evidence on the impact of sanctions, as well as that of drawing inferences from the evidence that is available. Nevertheless, the overall assessment—that the United States has never been overwhelmingly successful yet was relatively more successful during the Cold War than more recently—is persuasive. With regard to the future, “the prospect that any anti-Soviet sanctions may produce significant gains for American foreign policy in terms of any of its longstanding economic and political-instrumental objectives is exceedingly slight” (p. 245).

*Pipeline Politics* demonstrates the utility of careful historical research, grounded in a sensi-

ble, not overly deterministic analytical framework. It draws upon existing concepts to yield insights on an important issue that, until recently, received relatively scant scholarly attention. It is a solid contribution to the literatures on international economic sanctions and U.S. foreign policy and deserves the serious attention of scholars in those areas.

MICHAEL MASTANDUNO

*Dartmouth College*

**Wayward Capitalists: Target of the Securities and Exchange Commission.** By Susan P. Shapiro (New Haven: Yale University Press, 1984. xix, 227p. \$26.00 cloth; \$9.95 paper).

In the past several years the crackdown by the Securities and Exchange Commission (SEC) on insider trading, financial fraud, and other abuses has dominated the financial news. The timely reissue of Shapiro's contribution to the Yale Studies on White Collar Crime offers a case study of the enforcement strategies of the SEC. The book's superiority over popular accounts of financial crimes is derived from its source material, which consists of interviews with the commissioners and staff of the SEC and confidential data from investigations conducted by the agency between 1948 and 1972.

Shapiro asserts that the vitality of capital markets depends on trust, but that the intangible nature of securities makes those markets vulnerable to fraud and misrepresentation. The thesis of the book is that the intelligence system employed by the SEC constrains the type of information that can be discovered and, hence, the enforcement actions that are undertaken. The book provides a taxonomy of the offenses policed by the SEC and descriptive statistics showing the most prevalent crimes at each stage of detection and prosecution. Though each form of illegal behavior is vulnerable to discovery, the SEC's detection methods have limited accuracy. Most of the SEC's actions are generated by investor complaints and by errors in required filings, a relatively passive source of intelligence. The SEC's typical enforcement action involves few victims and few offenders, with a median cost to the victims of \$100 thousand. The book

concludes that the SEC has little control over its enforcement program because of its "passive dependence on reactive intelligence" (p. 94) and claims that the SEC could be better organized to exploit information about law breaking.

An unstated public interest theory of regulation drives Shapiro's analysis. She regards the SEC as the "preeminent protector of the capital markets in this country" (p. 4) despite recurrent periods of lassitude in its history and its "skewed" enforcement policy. The possibility that some benefit privately from the SEC's enforcement policy and that beneficiaries may influence enforcement policy through the detection mechanism is never addressed. If enforcement is skewed because of the mechanisms of intelligence gathering, knowing the motivation of those who design and control the intelligence gathering apparatus is crucial for understanding agency behavior.

Furthermore, Shapiro examines the detection and prosecution record without questioning the ties between the agency's policymakers and the Congress and the president. The impact of political goals and the hierarchical structure in which the agency operates on the organization of intelligence gathering and enforcement is ignored. It is not surprising, for example, that the SEC relies on reactive intelligence if the agency serves as a barometer of interest group power for congressional or executive overseers. No reference is made to theories of regulation which explain patterns of agency policymaking and which may explain the organizational structure of administrative agencies.

Shapiro's interview and case study approach is embellished with statistics, but no hypotheses are offered or tested. In describing investigations by the SEC, the SEC's information sources, and the offenders and victims, she complements colorful anecdotes with a statistical breakdown of the offenses. Tables containing the summary statistics are unreadable, however, because of multiple classifications. Factor analysis is employed to show the common attributes of the different detection strategies. However, the only artifacts of the empirical procedure are graphs of the detection strategy loadings in factor space and a vague discussion of the results. It is difficult to leap from the textual discussion of results to the important conclusion that the detection strategies

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employed by the agency determine the cases that are prosecuted.

The usefulness of the book to political scientists and economists is limited because of its atheoretical approach to agency policymaking and hypothesis-free empiricism. *Wayward Capitalists* is, however, a rich description of financial crime and enforcement that may be useful as background material to a more rigorous study of the organization of administrative agencies.

MARK J. MORAN

*Case Western Reserve University*

**Private Interest Government: Beyond Market and State.** Edited by Wolfgang Streeck and Philippe C. Schmitter (Beverly Hills: Sage, 1985. x, 278p. \$35.00 cloth; \$14.95 paper).

This new volume in the Sage Series in Neo-Corporatism is a collection of papers that, with two exceptions, were first presented at a 1983 colloquium in Florence sponsored by the European Group for Organizational Studies (EGOS). The opening essay by Streeck and Schmitter argues that "the association" should be viewed—along with the community, the market, and the state—as "a distinctive fourth institutional basis of order"; examines theoretically the logic of "an associative model of social order"; and explains how and why elements of corporative-associative order have emerged in the form of "private interest governments" (PIGs) in particular sectors, industries and policy areas of many polities. This piece, by far the most ambitious in the book and doubtless the one destined to be most widely read, thus places the traditional neo-corporatist paradigm within a broader theoretical context and at the same time clarifies its relevance to current public policy debates, presenting the "regulated self-regulation" that corporatist PIGs can offer as a strategic alternative to either direct state intervention or a neo-liberal restoration of the market.

As Streeck and Schmitter note in the preface, the 11 other contributions in the book "are empirical studies that, in one way or another, marshal positive or, as the case may be, negative evidence for the themes and hypotheses raised in the first chapter." Valid though that contention is, it must be said that

most of the chapters address those themes only in an unsystematic, disjointed, and sometimes peripheral fashion. Since the authors approach their topics from a wide variety of perspectives and are preoccupied with disparate research agendas, readers concerned with the issues highlighted by the editors must be prepared to wade through or skip over debates on such extraneous matters as the "conceptual foundations of accounting" and the alleged failings of "orthodox interpretations" of Burns and Stalker's *The Management of Innovation*. Moreover, readers will be challenged at times to perceive the thematic coherence of essays dealing with six different sectors or issue areas (advertising, accounting, pharmaceutical price regulation, debureaucratization, milk marketing, and the voluntary sector) in national cases ranging from Britain (five chapters) to Holland (two chapters) and Austria and Switzerland (one chapter each). Only one chapter handles its topic comparatively (with an assessment of advertising self-regulation in Belgium, Canada, France, and Britain), while one other presents not the usual intensive case study but rather a typology of modes of governance in the various sectors of the U.S. economy. With such a motley collection of essays, the book could have been greatly strengthened by the inclusion of a concluding chapter drawing connections between the theoretical concerns voiced in chapter 1 and the disparate empirical case studies that follow. Alas, while the editors note in the preface that they originally intended "to close the volume with another essay reassessing the potential and performance of private interest government in the light of the empirical material contained in the collection," this project "had to fall by the wayside" given the press of publication deadlines and the fact that "a major intellectual effort" would have been required to deal with an issue that emerges as a "complex and thorny one, perhaps more so than was originally thought." Readers are thus left to pick up the pieces themselves.

What the editors do not mention is that a different type of organizational scheme might have enabled them to reach at least some narrower, more focused conclusions while also enhancing the volume's analytical punch. Disparate though the chapters are, all but two of them could have been organized into two coherent parts. First, the essays by Boddewyn,

Willmott, Wilson and Butler, Hughes, Sargent, and Grant could have been presented as sectoral studies of "PIGs in Britain." Viewed comparatively, these chapters provide not only complementary perspectives on the multifarious uses of private interest government in the British case but also illuminate a point raised recently by Schmitter in another book (Wyn Grant, ed., *The Political Economy of Corporatism*, 1985): while the concept of *neocorporatism* has "contributed significantly to [a] revival of interest in the state," the empirical studies inspired by it have provided reminders that "*the state*" is in fact "an amorphous complex of agencies with ill-defined boundaries, performing a great variety of not very distinctive functions." As the relevant chapters of this volume show, *the British state* is a remarkably complex, fragmented set of actors ranging from the Department of Health and Social Security to the Charity Commission. Second, the essays by Traxler, Farago, Grant, and van Waarden (and part of the one

by Hollingsworth and Lindberg) could have been packaged as "PIGs in the Dairy Industry: A Comparative Perspective." These chapters together raise an obvious, fascinating question: how is one to explain the emergence of a strikingly similar mode of private interest government in the dairy sectors of polities as diverse as Austria, Switzerland, Britain, Holland, and the United States?

The strength of this volume is that it raises such questions and, in the editors' words, offers "rich material for future cross-national comparisons aimed at an improved understanding of the social and institutional conditions for the emergence and functioning of private interest government." Its central weakness is that it generally stops short of making the comparisons and providing the answers.

JOHN T. S. KEELER

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## ARTICLES

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# IDEOLOGICAL CHANGE IN THE BRITISH CONSERVATIVE PARTY

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**W**e address both a puzzle and a theory. The puzzle is posed by the emergence of "Thatcherism," an un-Conservative ideology that has appeared in an antiideological British Conservative party. We address this puzzle by determining what Thatcherism is and by showing that although it represents a minority viewpoint, it does indeed fit into previous Conservative thinking. The theory in question is the spatial theory of ideological change, which, we argue, is impugned by the circumstances of Thatcherism's construction. We address this theory by investigating potential constituencies at the time of Thatcherism's creation and by examining evidence about the intentions of those who created it. Finally, we seek both to draw out the implications of Mrs. Thatcher's campaign to convert the voters to her views and to explain why the same spatial theory that Thatcherism confounds seem confirmed by equally striking cases in postwar British politics.

**C**ase studies in comparative politics, as Eckstein (1975) convincingly reminds us, can help us to recognize puzzles and also to probe theories. This case study does both. The puzzle to be recognized is that despite the British Conservative party's long-standing distaste for dogma, it has spawned a new ideology, "Thatcherism," an ideology that, moreover, has been characterized by party members as un-Conservative. The theories to be probed are spatial theories of ideological change, which, we shall argue, are impugned by Thatcherism's march away from the ideological center and improved by the conditions that this march suggests.

Thatcherism is indeed a puzzling phenomenon. The Church of England, "the Conservative party at prayer," has declared publicly that it does not par-

ticularly care for Thatcherism. (*Times*, 10 March 1985). The palace, the party's traditional rallying point, has let it be known that the Queen, too, does not care for it much. (*Times*, 20 July 1986). Former and present members of Mrs. Thatcher's Conservative governments have dubbed her "the Immaculate Misconception" and regard her programs as an attempt to overturn the mixed economy and welfare state. In their view, Thatcherism's claims about simply being an exercise in resurrecting the "fundamentals of Conservative belief" are belied by its radical dispositions. The party, they say, is in the grips of a heresy. The new leadership, they say, "worships at the shrine of Hayek," has become "the prisoner of Chicago," prefers conflict to practical reform, and is not really very Conservative at all.

On the theoretical side, Thatcherism is

indeed an extreme and extremely revealing case (Eckstein 1975, 99-104). For more than a decade, Mrs. Thatcher and her associates have self-consciously led the Conservative party away from the electoral center. In so doing, they have confounded formal theories of ideological change that are associated with spatial modeling and function as components of the theories of party competition of Anthony Downs (see Chappell and Keech 1986).<sup>1</sup> Empirical exceptions to these theoretical constructs are not so rare (Frohlich et al. 1978; Miller and Wattenberg 1985; Poole and Rosenthal 1984, 1985; Rabinowitz 1978). Yet Thatcherism is particularly worthy of close examination in this context, for it contradicts the spatial theories so clearly and so completely that it reveals the conditions that must be satisfied for these theories to work. Thatcherism is equally distinctive in its dramatic illumination of the principal alternative to Downsian models: the claim that leaders may not so much push their parties towards the public as attempt to coax the public towards themselves.

Thus we face three interrelated tasks. First, we must determine what Thatcherism is and how it departs from previous Conservative thinking. Next, we must demonstrate that the emergence of Thatcherism does indeed confound Downsian theories about the construction of ideologies. And finally we shall seek to improve these spatial models by drawing out from our case study key conditions for predicting accurately the course of ideological change.

### Data and Measurement

While Margaret Thatcher was developing and propagating her ideology, she was involved in four electoral contests. The first was for the leadership of the Conservative party in February 1975. The other three were the general elections of 1979,

1983, and 1987. For the leadership election, the constituency was Conservative members of Parliament; for the general elections, it was the electorate and, in particular, Conservative supporters. To complete our three tasks, we shall require survey data covering the entire period.

The primary data set consists of survey responses collected on the eve of Thatcherism's appearance, during 1972 and 1973, from British members of Parliament, candidates, and the public. Members of Parliament ( $N = 521$ ) were interviewed by means of tape-recorded discussions. At the same time, a sample of candidates ( $N = 107$ ) was interviewed, candidates who had never themselves been MPs. Public views are provided by a simultaneous opinion survey administered to a probability sample of British adults ( $N = 2,500$ ). Ideally, we would like to examine multiple indicators carrying these preelection data forward from the early 1970s to the present. Unfortunately, very few policy questions of any kind have been replicated in surveys even over the period between the general elections of 1974 to 1987. We must rely therefore on a compilation of data from several sources: the British Election Study's cross-sectional samples for October 1974 ( $N = 2,365$ ), 1979 ( $N = 1,893$ ) and 1983 ( $N = 3,955$ ); the BBC-Gallup election day surveys for 1979 ( $N = 2,435$ ), 1983 ( $N = 4,141$ ) and 1987 ( $N = 4,886$ ); and Gallup opinion poll data gathered throughout the period.

Two distinct types of attitude measures are involved. Specific views about institutions and about economic and social policies are assessed by Likert-type scales. Political ideals such as "free enterprise" or "community" are measured by a technique derived from the work of Milton Rokeach (Rokeach 1968, 1973, 1979; Searing 1978; see Appendix A). Most definitions of political ideology encompass both political values and policy beliefs. But Downs (1957, 96) separates

the two and restricts his definition of ideology to values, to abstract ends and means: "We define an ideology as a verbal image of the good society and of the chief means of constructing such a society." Mrs. Thatcher's approach is much the same. She believes that general political values should hold center stage and has herself devoted more energy to such themes than to detailed policy. (Kavanagh 1987, 249; Riddell 1983, 8-9). We shall follow both Downs and Thatcher by defining Thatcherism and other varieties of British Conservatism in these terms.<sup>2</sup> Still, specific policy beliefs must count as integral components of ideologies, for they put ideologies into practice. Thatcherite values will therefore be linked with Thatcherite policy beliefs, particularly when we consider relationships between Thatcherism and Thatcherite views in the electorate.

### Thatcherism Defined

Rational politicians in two-party systems are not expected by Downsian theories to create highly integrated ideologies. It is in their interest to dilute their messages and generate ambiguity about exactly where they stand. (Downs 1957, 132-36). Yet Mrs. Thatcher despises waffling and spends a great deal of energy making her extreme positions perfectly clear. These positions revolve around three principal political ideals: discipline, free enterprise, and statecraft.

Discipline—obedience to the rules—is the value that Mrs. Thatcher has held the longest. She regrets that since the war it has declined in schools, families, and society at large. Discipline embraces self-discipline and the Victorian virtues of hard work, thrift, and deferred gratification that have, at times, been regarded as her quintessential themes (Riddell 1983, 7-8) and that have often been contrasted favorably by Thatcherites with a postwar

culture of permissiveness, self-indulgence, and irresponsibility (Behrens 1980, 10). The Thatcherite interpretation of discipline emphasizes law and order, the restoration of capital punishment, and a more severe regime of sentences in the court system. Discipline also embraces the notion of a self-reliance and individual enterprise that disdains the creation of welfare benefits for anyone except the really poor. The ideal society, from this perspective, would be a world of autonomous individuals independent enough to take risks in order to achieve and responsible enough to accept the consequences, including the disappointments and the failures. Discipline is seen as the moral handmaiden of free enterprise.

Free enterprise, too, has been characterized as Mrs. Thatcher's central aim. (Utley 1978). Thatcherites believe that by rolling back the state from involvement in the economy, they can reverse Britain's economic decline. Wealth, Mrs. Thatcher argues, is created by private competition, risk taking, and efficient marketing. Thatcherite free enterprise seeks to create financial stability and to reduce inflation by curbing the growth of the money supply and by reducing government borrowing and public expenditure (Burch 1983; Norton and Aughey 1981, 79-89). Rolling back the mixed economy is a radical proposal—and Thatcherites happily accept the adjective. But radicalism goes deeply against the grain of party tradition; for if Conservatives dislike anything, they have since the French Revolution disliked radical change. Still, monetarism—superseded by privatization in the 1983 Thatcher government—helped the Thatcherites establish their legitimacy here by building a bridge between free enterprise on the one hand and the more deeply rooted Conservative ideals of "statecraft" on the other. Monetarism promoted free enterprise; but it also facilitated statecraft because, by freeing the state from the necessity of dangerous battles with the

trade unions, it enabled the state to maintain its integrity and develop autonomy at the center.

Statecraft, the ideal of strong central government, was the last of the three major principles to be put in place. It seems, at first glance, out of place. Thatcherism aims to diminish the government's influence over the economy but, at the same time, to increase its influence over despised institutions like trade unions, over big-spending institutions like nationalized industries and local government, and over privileged and socially "elitist" institutions like universities, the civil service, and the BBC (Kavanagh 1987, 284-92; Riddell 1983, 17). Authoritative leadership is an essential Tory doctrine (Beer 1965, 92-94). This is what Thatcherism offered to other Conservatives. It would pull out of the economy but strengthen its control in other areas (Beattie 1979, 283-85; Bulpitt 1986, 21-26). This is less of a paradox than it might seem: in order to save the authority of the state, governments had to withdraw from the micro-economy; but in order to give free play to the market and private enterprise, they had to reassert their authority over trade unions, local government, and other hostile institutions. To underwrite free enterprise, the government could not afford to be *laissez-faire*.

In fact according to Bulpitt's (1986, 34-39) reading of the first Thatcher government, statecraft was its dominant concern, dominant even over free enterprise and certainly over discipline, although not unconnected with it. This was so because by 1979 the Thatcherites saw as their chief problem the restoration of the autonomy of the center, the restoration, after the "winter of discontent," of a strong central government that would be capable of governing. They were determined that if they were to do one thing during their government, it would be to return to Westminster and Whitehall a public good—strong govern-

ment and leadership.

Discipline, free enterprise, and statecraft form the tripos of Thatcherism. Where does it fit in the pantheon of Conservative values? Where did it fit in the ideologies of the sorts of Conservative politicians who chose Mrs. Thatcher as their leader in February 1975?

### Thatcherism and Conservatism

When Tories like Lord Alport (Letter to the editor, *Times*, 16 October 1985) complain that "Thatcherism is not Conservatism" they mean that Thatcherism conflicts with many of the ideals that have guided the party since the war. Yet Thatcherism may have been the view of a silent majority of Conservative politicians outside the magic circle. This "silent majority" thesis is compatible with spatial models of ideological construction, for it would be consistent with an argument that Thatcherism was developed to appeal to the parliamentary party, the constituency for Mrs. Thatcher's first electoral contest. We turn now to investigate the structure of Conservative values on the eve of Thatcherism, that is to say, in 1972-1973, and to attempt to locate key Thatcherite values in the blueprints.

The Conservative party has long been regarded as highly consensual with few rigid factions and much agreement about bedrock Conservative beliefs (Finer, Berriington, and Bartholomew 1961; Rose 1964). From this perspective, the value-ranking data show much less consensus than anticipated. If we examine the percentage of Conservative politicians who rank the values (see Appendix A) as either very important or very unimportant (in other words, either at the top [1-3] or at the bottom [7-9] of their lists), we find only 3 values out of the total of 36 where 8 out of 10 Conservative politicians concur: they endorse freedom and, not surprisingly, reject socialism and

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**Table 1. Political Values on Which the Monday Club and Pressure for Economic and Social Toryism (PEST) Differ by 20% or More**

Political Values	Percentage Ranking the Value 1-3		
	Monday Club	PEST	Difference
Monday Club exceeds PEST			
Public order	71	39	32
Strong leadership	61	39	22
Strong government	58	33	25
Free enterprise	58	25	33
Patriotism	57	26	31
Self-discipline	44	23	21
Authority	35	3	32
Discipline	26	3	23
PEST exceeds Monday Club			
Community	35	81	46
Compassion	39	80	41
Social progress	38	61	23
Social equality	13	50	37
Number of Cases	24	36	

economic equality. This limited agreement on values might be enough to hold together a "broad-church" or "catch-all" party, but it hardly amounts to an impressive degree of consensus behind Thatcherism or any other variety of Conservatism. This first step in the investigation indicates that there may be many different ways of thinking conservatively. Are the ways so many as to make the party's ideological comforter a patchwork quilt of idiosyncratic views, or are there some general patterns?

### The Antipodes

Antipodes are places situated at opposite ends of the earth. And on the eve of Thatcherism, the Conservative party's antipodes were represented on the Right by the Monday Club and on the Left by Pressure for Economic and Social Toryism (PEST). A comparison of similarities and differences between these two groups will establish the boundaries

of the Conservative ideological map and may provide some glimpses of Thatcherism, for the Monday Club has come to be seen in retrospect as one of Thatcherism's roots within the party.

Table 1 presents those political values on which the Monday Club and PEST differed by 20% or more in ranking as "very important." For the Monday Club, these values were led by public order (defined as maintenance of law and order), followed a long way behind by concerns for traditional Conservative ideals such as strong government, patriotism, and authority. Free enterprise was plainly prominent. But it did not have the flagship role it was destined to play in Thatcherism circa 1979-1983. The Monday Club's focus is still on older Conservative interests in the nature of the state and the community rather than on the sort of economic arguments that would soon dominate political debate in the party. This is reflected in the values with the highest absolute scores, which can be



found in the first column. The magnitude of such scores indicates the value's *centrality* to Monday Club members (see Appendix A). Still, if the Monday Club and kindred spirits were not fully developed Thatcherites before Thatcherism emerged, the principal pillars of Thatcherism were certainly present.

The data for PEST turn our searchlight onto the other ideological pole, in order to determine the range of the terrain within which Thatcherism was to establish itself. Here the touchstones are clearly community (defined as harmonious social relations) and compassion (defined as concern for human welfare). The PEST account is rounded off with social progress, Lord Butler's (1971) favorite term for governmental commitments towards steady social improvement, and with social equality, interpreted along the lines of Ian Macleod's aim, "to see that men had an equal chance to make themselves unequal" (Fisher 1973, 79).

### An Exploratory Analysis

Our earlier findings on the lack of general consensus are consistent with accounts of Conservatism as simply a skeptical orientation toward change rather than a philosophy stocked with substantive political goals. By contrast, Table 1 on the Monday Club and PEST suggests that at least for some sub-groups within the party, Conservatism had considerable substance in the early 1970s and that political thinking in subsections of the party could be quite highly crystallized. We turn now to search for more such subgroups across the party's entire terrain, for varieties of Conservatism and Thatcherism.

For this purpose, an exploratory factor analysis was applied to all 36 values. If there were distinct ideological communities between borders set by the Monday Club and PEST, the factor analysis should give us at least a rough indication

of their presence and a rough impression of their character.

First, there seems indeed to have been very little consensus across the party as a whole. However, neither was there a multiplicity of sharply defined ideological communities. Fourteen factors appeared with eigen values above 1.0; none of them explained much variance; and the drop-off from one factor to another was modest and steady.<sup>3</sup> The path to the next step in the inquiry was provided by an inspection of the correlation matrix, which showed clearly, as did the first factor—albeit less clearly—that by far the strongest correlations in the data (apart from items measuring much the same thing) were negative relationships between the values of community on the one side and the values of economy on the other. Community and economy have long been the respective concerns of Toryism and Whiggery, the two outlooks that Norton and Aughey (1981, chap. 2) have argued—correctly we believe—are the defining pivots for ideological thinking in the party.

The ideological legacies of Toryism and Whiggery have been brought together in the twentieth-century Conservative party's concerns with both traditional values and capitalist development. For Tories, who are devoted to the maintenance of a harmonious and ordered society, the fundamental values are authority and community. By contrast, capitalism, free enterprise, and property are the principal interests of Whigs, because Whigs focus on the economy, on creating wealth through efficient production. Still, in 1972 Thatcherism was not yet clearly seen or anticipated. Its commitments to statecraft and free enterprise seem to be on opposite sides of the fence. The picture needs to be developed further by dividing each side, Tories and Whigs, into their principal subtypes, as in Figure 1. We shall be guided by Norton and Aughey's (1981) analysis here, but mainly on the Whig

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side; for Toryism seems to us to have a somewhat simpler general structure than they allow.

### **Tories: Progressive and Traditional**

After the devastating electoral defeat of 1945, progressive Tories concluded that it was necessary to present the electorate with a program that promised to reduce the extremes of poverty and wealth and to repudiate laissez-faire economics in favor of a system in which the state would act as a trustee for the community. (Behrens 1980, 13; Butler 1971, 132-46). Progressive Toryism has paternalistic roots that help explain why its support for the welfare state is so closely connected with its conception of community and why its strong emphasis on compassion complements dispositions associated with social hierarchy (Norton and Aughey 1981, 66-79). Progressive Tories also value highly the ideas of social progress, effective social reform, rationalism, and social planning because such Tories are determined that their party should be modern and well informed. They seek to adapt Conservative principles to the changing world (Norton and Aughey 1981, 75-79). And for the party to prosper in this modern world, they seek to ensure that it pursues a pragmatic, empirical approach.

Traditional Toryism, by contrast, tends to be more devoted to Old England and more patriotic (Blake 1970, 130). In fact, traditional Tories sometimes feel they live in an alien world and are often very pessimistic about modernity (Norton and Aughey 1981, 66-75). They are cynical about social planning and the progressive Tories' enthusiasm for continuous social improvement. Traditional Tories complain that the values of the counting houses are replacing those of the country house, that modern capitalism, through its alienation and compulsive consumption, is shredding the fabric of traditional

**Figure 1. Patterns of Conservative Ideology on the Eve of Thatcherism**

Tories	Whigs
Progressive	Liberal
Traditional	Corporate

British society (Norton and Aughey 1981, 66-75). Traditional Tories prefer free enterprise to socialism, of course, but economic ideologies are not their main preoccupation. Their primary concern is strong government, leadership, and authority—strength at the center of the political system and the party (Gilmour 1971, 88).

### **Whigs: Liberal and Corporate**

Liberal and corporate Whiggery share an overriding interest in preserving and promoting the institution of private property but differ over the most effective means of doing so. Liberal Whigs prefer the free enterprise approach of small business, which was put away in the Conservative cupboard after the war (Norton and Aughey 1981, 79-86). Liberal Whiggery refused to give up on it, however, believing that the heavy hand of government distorts the marketplace and undermines economic growth. Competition, not government planning, creates real wealth. This Victorian political economy is often complemented by a Victorian morality of hard work and discipline in society and self-discipline and self-reliance among society's members. Liberal Whigs believe that there has been a decline of morality in contemporary society, a decline they attribute not to modernity (which is the position of the traditional Tories who see liberal Whigs as part of the problem) and not to capitalism but rather to a wet collectivism

found in both the Labour party and the progressive wing of their own tribe.

Corporate Whigs are, by contrast, collectivists who believe that government should work with industry to nurture large-scale capitalism. Corporate Whigs, unlike their liberal relatives, are quite sympathetic to many of the values of progressive Tories. They appreciate, for instance, the ideals of community and compassion. But they are much more concerned with producing the wealth and prosperity that they believe will make these humane values easier to achieve. Thus, corporate Whigs are not just compassionate politicians—they are compassionate capitalists. Rationalism, a rigorously reasoned approach to planning, is highly regarded by these technocratically oriented Conservatives. They believe that Britain's economic position can be boosted by a close partnership among government, business, and trade unions. A high-risk entrepreneurial approach is not for corporate Whigs; they prefer a stable economic environment conducive to large capital's long-range planning.

The description of these four types fills in the terrain between the Monday Club and PEST. We have not yet located Thatcherism with satisfactory precision—but we are close. The Thatcherite combination of free enterprise and discipline is squarely in the liberal Whig tradition. What is missing is the link with the third pillar, statecraft. Perhaps it is missing because we are dealing with Weberian "ideal types" that developed before Thatcherism emerged. A confirmatory factor analysis will help assess how our four-cell typology corresponds to the realities of Conservative thinking on the eve of Thatcherism and, through its grounding in this empirical world, may show clearer signs of the new ideology.

### A Confirmatory Analysis

Tories are said to value community above the creation of wealth, and Whigs

to value the creation of wealth above community. The ranking instrument is ideally suited to model these comparative evaluations. Thus for purposes of our analysis, we shall count as Tories all those who ranked community above property, and as Whigs all those who ranked property above community.<sup>4</sup> Next, a confirmatory factor analysis was performed for each of the two groups separately to assess the verisimilitude of our subtypes and to seek a sharper picture than emerged for the party as a whole in the exploratory factor analysis (see Appendix A).

The confirmatory analysis does not attempt to uncover dimensions beneath a wide field of values but firstly to assess our claims about the main factors that structure Tory thinking and Whig thinking; and secondly to assess our interpretation of the values that are relatively most important within each of the four subtypes. This strategy concentrates attention on the first principal component, that is, the component that best depicts linear relationships among the items.


Table 2 reports the first factor emerging from the analysis of the Whigs. This factor has an eigen value of 2.77 and explains 15.4% of the variance. It is substantially stronger than the second component, which tends to look a good deal like the third, and so on. In short, the eigen values and percentages of variance explained drop off more precipitously from the first to the second factor than they do thereafter. Hence, the first principal component can be interpreted as the best single summary of the linear relationships in the data. And it clearly represents the division between liberal and corporate Whiggery.

Liberal positions cluster at the negative pole where free enterprise and authority are found alongside statecraft. The corporate Whig ideals at the positive pole are led by social planning and, as projected by the ideal type, they mix values such as capitalism and compassion, rationalism

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**Table 2. Factor Analysis of Conservative Values (Whigs)**

Political Values	Correlation with First Principal Component	
Authority	-.72	
Statecraft	-.53	
Free enterprise	-.45	
Deference	-.32	
Patriotism	-.31	
Efficiency	-.19	
Autonomy	-.17	
Public order	-.16	
Social hierarchy	.15	
Empirical	.19	
Social progress	.25	
Intelligence	.26	
Caution	.37	
Rationalism	.42	
Compassion	.46	
Capitalism	.46	
Community	.51	
Social planning	.55	



liberal  
Whiggery

corporate  
Whiggery

Note: Eigen value = 2.77. *N* = 97. The 36 value items have been reduced to 18 in this table by means of the additive indexes specified in Appendix A.

and community. Although there is ambiguity about the interpretation of some of these values, and uncertainty about the proper locations of others, there are no values with respectable loadings in the liberal list that were projected for the corporate side, nor vice versa.<sup>5</sup>

Table 3 summarizes the first principal component for the Tories, which carries an eigen value of 3.05 with 17.3% of the variance explained. Again, the component displays acceptable strength, and again it supports handsomely the theoretical expectations.<sup>6</sup> The recovery of the projected Tory patterns is very striking indeed. At the positive end are the key values associated with progressive Toryism: compassion, social hierarchy, empirical approach, and community. And clustered at the negative end are the values of traditional Toryism, notably patriotism, statecraft, and authority. Again, none of the values with respect-

able loadings in the hands of traditional Toryism obviously belong to progressive Toryism, nor vice versa.<sup>7</sup>

One of the most striking features of Tables 2 and 3 is that the value of authority anchors the perspectives of both liberal Whiggery and traditional Toryism.<sup>8</sup> The position of statecraft in second place in both is more intriguing still. Its incorporation into this empirical case of liberal Whiggery, in contrast to its absence from the ideal type, presents the third pillar of Thatcherism we have been seeking. Now we have together: discipline (i.e., "authority" in our data) + free enterprise + statecraft. The obvious difference between liberal Whiggery and traditional Toryism is that the liberal Whigs place, as would be expected, much greater weight on free enterprise and much less on patriotism.<sup>9</sup>

In the same vein, Tables 2 and 3 also clearly suggest a complementary sym-

Table 3. Factor Analysis of Conservative Values (Tories)

Political Values	Correlation with First Principal Component	
Compassion	.62	<p>progressive Toryism</p> <p>traditional Toryism</p>
Social hierarchy	.48	
Empirical	.46	
Social progress	.46	
Caution	.44	
Community	.43	
Intelligence	.31	
Rationalism	.31	
Social planning	.30	
Efficiency	.04	
Capitalism	.00	
Autonomy	-.16	
Free enterprise	-.25	
Deference	-.26	
Public order	-.44	
Patriotism	-.50	
Statecraft	-.53	
Authority	-.69	

Note: Eigen value = 3.05.  $N = 149$ . The 36 value items have been reduced to 18 in this table by means of the additive indexes specified in Appendix A.

pathy between corporate Whigs and progressive Tories, who share a strong interest in community and compassion.<sup>10</sup> This melding of corporate Whiggery with progressive Toryism is not so surprising as its opposite combination, for it is recognizable as the alliance that dominated the party's ruling circles from the early 1950s to 1974.

If throughout the postwar period the dominant alliance in the party has been between corporate Whiggery and progressive Toryism, then Mrs. Thatcher may be said to have built upon an alternative potential alliance linking liberal Whiggery with traditional Toryism. This new alliance seems unexpected, indeed remarkable, because of the natural tension that exists between the commercial party that constructs smart shopping precincts and the country party that preserves nostalgic England. Thatcherism

has made the most of the connection between free enterprise and statecraft. It was designed to develop this new combination of well-established Conservative sympathies. But was it designed to translate such sympathies into votes?

### Thatcherism and the Theory of Ideological Change

We now turn to our second task, that is, to demonstrate that Thatcherism should be particularly likely to fit spatial theories of ideological change but instead confounds them. According to Downs (1957, 96-101), politicians construct ideologies opportunistically as weapons in the struggle for office. They construct ideologies to attract votes, and they adjust them to attract the greatest number of votes. Unlike many partisan ideologies

which evolve piecemeal, Thatcherism was deliberately constructed as Mrs. Thatcher prepared to face two electorates: Conservative members of Parliament, who in February 1975 would elect her as party leader, and the public, which would return her party to power in 1979, 1983, and 1987. Two empirical probes into this theory are feasible, both indirect but together perhaps convincing. The first examines Mrs. Thatcher's electorates to determine whether potential constituencies actually existed to stimulate the new ideological appeals. The second looks to the political leaders, to Mrs. Thatcher and her associates. It examines their statements and interpretations of their statements by informed observers to assess the degree to which they designed their ideological creations to attract votes.

### MPs: Voters and Leaders

Although we cannot determine the exact number of potential Thatcherite MPs on the eve of Thatcherism, a range of estimates can be established by reconstructing the distribution of sympathies for the values at Thatcherism's core: strong government, free enterprise, and discipline. What *proportions* of the Conservative parliamentary party might reasonably be expected to find her appeals appealing?

A map of ideological sympathies can be reconstructed by investigating how many Conservative politicians ranked the core Thatcherite values at the top of the lists. Three soundings have been taken. The first is the proportion of respondents awarding high marks to strong government, the most widely accepted of the Thatcherite pillars. The second sounding takes the proportion of respondents who were sympathetic to both the traditional Tory value of strong government and the liberal Whig value of free enterprise. This is the crucial step, the realization that free enterprise need not be laissez-faire, the

belief that the government should withdraw from the economy and at the same time flex its muscles elsewhere. The third sounding completes the Thatcherite tripos by asking how many politicians valued highly not only strong government and free enterprise but discipline as well.

Results for the Conservative MPs can be found in Table 4. These data suggest that on the eve of Thatcherism the potentially sympathetic spirits constituted a small minority—only about one quarter on the generous criterion of having taken the step of endorsing both strong government and free enterprise. On the more demanding criterion of endorsing all three Thatcherite values, the estimate would be less than 10%. Thus, the estimated range of potential support lay between 10% and 25%.<sup>11</sup> Moreover, according to nearly everyone who has studied the situation, Mrs. Thatcher "was elected leader of the Conservative party in February, 1975 not because she held [Thatcherite] views, but largely despite the fact that she held them" (King 1983, 97). In the early 1970s a march to the Right was favored neither by Conservative party workers, Conservative MPs, nor Conservative peers (Wapshott and Brock 1983, 106).

Let us turn to the leaders. Mrs. Thatcher was herself well aware that a sizable constituency for her views did not exist in the parliamentary party. It was certainly not to attract the greatest number of leadership votes that Thatcherism was created. This can be shown in two different ways. First, Mrs. Thatcher held, albeit in an undeveloped form, major elements of her ideology long before she contemplated standing for the leadership. She drew a preliminary sketch of Thatcherism in a speech at the Conservative party conference in 1968, two years before she was appointed a minister in the Heath government and seven years before the leadership election. This speech expressed skepticism about the welfare state and its tendency to undermine such

**Table 4. Conservative Politicians on the Eve of Thatcherism:  
Strong Government, Free Enterprise, and Discipline, by Subgroups**

Conservative Subgroups	Number of Cases	Percentage Ranking the Value 1-3		
		Strong Government	Strong Government and Free Enterprise	Strong Government, Free Enterprise, and Discipline
Senior ministers	15	47	20	0
Junior ministers (including PPSs)	54	48	17	1
Backbenchers	139	48	24	7
Candidates	46	59	26	13

middleclass values as hard work, self-discipline, and personal responsibility. In the same vein, the way to control prices, she said, was through competition, not governmental regulation. She even foreshadowed monetarism. In fact, the only missing component was "statecraft," which would be added later.

Secondly, during the campaign for the leadership election, an important decision was made to tone down her ideological position and talk about leadership instead. The emerging ideology of Thatcherism was regarded as a liability because it was believed that very few members of the Conservative Parliamentary party were ready to turn against government intervention in the economy or towards Victorian morality (Wapshott and Brock 1983, 106-14). Instead, Mrs. Thatcher's campaign rested on a succession of accidents and a creative stratagem. The accidents were the withdrawals of Sir Keith Joseph and Edward Du Cann from the race (both for personal reasons) and the bungled campaign of William Whitelaw. The creative stratagem was devised by her campaign manager, who went round the House of Commons the night before the poll, winning votes from MPs who did not wish to see her leader but who wanted Heath out and would have to ensure that she had enough votes to trip Heath on the first ballot and thereby trigger a second

where their own preferred candidates would enter (Cosgrave 1978, 70-71).

#### **Publics: Voters and Leaders**

If Thatcherism was not designed to please Mrs. Thatcher's parliamentary colleagues, neither was it designed to move the Conservative party closer to the electorate—even though the party had just lost two general elections in 1974 and had won only one out of the previous five (1970).

To estimate the size of the potential public audience for Thatcherism, it is necessary first to establish how far the electorate thinks politically in terms of Conservative values. What aspects of Tory, Whig, and Thatcherite values in fact structured the thinking of the electorate?

We applied the confirmatory factor analyses undertaken on the party's politicians to Conservative identifiers. The results suggest patterns that are less crystallized than those for the politicians but are nonetheless recognizable reflections of the politicians' views.<sup>12</sup> Although corporate Whiggery fails to appear in the public's vision, both progressive and traditional Toryism emerge, and Thatcherism is foreshadowed in a liberal sector, more plainly perhaps than for the

## Ideological Change

**Table 5. Thatcherites in the Electorate on the Eve of Thatcherism:  
Strong Government, Free Enterprise, and Authority, by Subgroups**

Electorates	Number of Cases	Percentage Ranking the Value 1-3		
		Strong Government	Strong Government and Free Enterprise	Strong Government, Free Enterprise, and Authority
Conservative- attentive public <sup>a</sup>	382	64	30	5
Conservative identifiers	613	59	26	5
Conservative voters <sup>b</sup>	572	60	26	4
All public	1,988	53	17	3

<sup>a</sup>Conservative identifiers who express moderate or high political interest.

<sup>b</sup>Conservative voting intention.

politicians. Voters were aware. But were they sympathetic?

Table 5 reports value-ranking data for the electorate in 1972-73 that are comparable to those reported in Table 4 for MPs.<sup>13</sup> Overall, the results are again similar to those for the politicians: the potential constituency for Thatcherism is restricted to a small audience in both the Conservative and general electorates. Thus while half of the general public endorsed strong government, only 17% endorsed the critical combination of strong government and free enterprise; and only 3% accorded all three Thatcherite political values top marks. The situation is barely more promising among Conservative voters, identifiers, and attentive publics. Thus there were not nearly enough Thatcherites in the electorate to support a convincing Downsian case. Was the same true for specific policies?

To extend our ideological analysis into policy views, we shall work through the Thatcherite political values and seek policy beliefs that are part of the ideological package. For this purpose, a simple additive index was constructed from politicians' responses to Thatcherism's core values of strong government, free

enterprise, and discipline. Correlations between this index and each of the six policy items in Table 6 were significant at the .05 level or better.<sup>14</sup>

Unlike the politicians, however, the public audience does not connect these Thatcherite policy beliefs with Thatcherite political values. Only two of the correlations between the public's Thatcherism index and the six policy beliefs are significant at the .05 level.<sup>15</sup> Yet these are Thatcherite policy beliefs. It is therefore still worthwhile to investigate whether enough voters were sympathetic to such policies to have stimulated Thatcherites to promote them.

Policies, Downs reiterates (1957, 97-98), are created to win votes. Table 6 reports the proportions of the general public and Conservative identifiers who agreed in 1972-73 with the policy positions that Thatcherism would begin to promote a year later. These data bring in the first findings consistent with spatial models of ideological construction. In the area of discipline, Thatcherism did indeed move the Conservative party closer than it had been before to strong, popular, policy opinions. Very large majorities of both Conservative identifiers and the



**Table 6. Proportions of the General Public and Conservative Identifiers Holding Thatcherite Policy Views in 1972-73 (%)**

Items	General Public		Conservative Identifiers	
	Agreeing	Agreeing without Reservation	Agreeing	Agreeing without Reservation
Free Enterprise				
1. Denationalize nationalized industries	34	19	44	25
2. Restrict social service benefits to the really poor	49	29	58	34
Statecraft				
3. Trade unions don't have automatic right to consultation	33	17	44	23
4. Business doesn't have automatic right to consultation	34	15	37	16
Discipline				
5. Death penalty should be reintroduced	78	62	86	72
6. Harsher sentences for violent crimes	85	68	88	73
Number of Cases	1,988		613	

Note: Questionnaire items for entries in this table are presented in Appendix B.

general public favored more "law and order" (items 5 and 6) and supported programs that Mrs. Thatcher would develop.

Still, discipline seems to be the only one of the three Thatcherite pillars whose policy programs were tuned into public opinion. In the area of free enterprise, there is no sign of particularly strong support for denationalization and rolling back the welfare state (items 1 and 2).<sup>16</sup> Much the same picture emerges for the corporatist issues under the statecraft rubric (items 3 and 4). The public was at the time suspicious of trade union power,<sup>17</sup> but there is no evidence of popular support for dismantling functional representation or for intimidating institutions like local government, nationalized industries, universities, the civil service, and the BBC. In sum, the data reported in Table 6 suggest that electoral gains could be extracted from only one of Thatcherism's three core values, discipline. Significantly, these are also the

policy views that Mrs. Thatcher developed long before she became leader of the party and was in a position to think about ideological appeals that might help win the general elections of 1979, 1983, and 1987.

Nor is it the case that Mrs. Thatcher and her associates simply misread public opinion and believed it more receptive than it actually was to free enterprise and statecraft themes. On the contrary, the early Thatcherites were well aware that their views were at odds with most of the electorate. When, after 1974, she and Sir Keith Joseph began to articulate the ideology, they very self-consciously moved away from the electoral center. In his series of public lectures criticizing the postwar consensus, Joseph's central theme was the importance of "getting it right" rather than getting elected. After Mrs. Thatcher became leader in 1975 she set up institutes and seminars and brought in philosophers and policy advisors to

clarify and apply the ideology. There is no evidence whatsoever that this exercise was designed to attract votes.

Indeed, Mrs. Thatcher and Sir Keith Joseph believed that they had a mission to save Britain from itself and therefore embarked on a pedagogic crusade to win public support for the difficult road ahead. One of the few positive roles that Thatcherites give to the state is that of changing public expectations and values. Mrs. Thatcher's own behavior is important in this respect. Not since Gladstone has a prime minister held so many personal political convictions and sought to use the office to persuade the electorate of their truth. Surely she could not have believed that the voters were already convinced.

### Persuading the Public

Mrs. Thatcher's crusade has special relevance to spatial theories of ideological construction because it represents the antithesis of the usual models: instead of politicians moving their own locations towards those of the voters, they try to move the voters towards themselves. This alternative model, which is politician-driven rather than voter-driven, is promoted in some criticisms of the Downsian spatial wisdom. (Clubb, Flannigan, and Zingale 1980; Rabinowitz 1986). Thus we have in Thatcherism a remarkably strong effort by political leaders to reshape the political thinking of the electorate. Did it work? Mrs. Thatcher regards herself as tutor to the British nation. Has the British nation been a good pupil?

The purely electoral evidence is deceptive. Despite Mrs. Thatcher's three successive election victories, the last two with handsome parliamentary majorities, there has been no electoral surge towards the Conservatives under her leadership. In 1979, the Conservative share of the vote, although up from 36% to 44%, was well

below that obtained at previous Conservative election victories since the war. In 1983 it actually fell slightly to 42% where it stayed in 1987. Moreover, Conservative partisanship has weakened since the 1960s. In the three elections of 1964, 1966, and 1970 about half of all Conservative identifiers described themselves as "very strong" identifiers; in 1983 it was down to a quarter, which was less than in the party's disastrous 1974 elections. Thus she has neither swelled the Conservative congregation nor strengthened their faith.

But has she changed their ideological beliefs? "The Great Moving Right Show" was the title of an article in *Marxism Today* where Stuart Hall argued that Mrs. Thatcher and Sir Keith Joseph were succeeding in their crusade to convert the British electorate to a new Conservatism that fused free market doctrines and anticollectivism with "authoritarian populism" (January, 1979; see also Hall and Martin 1983, 19-39). According to this politician-driven model, Mrs. Thatcher and her supporters have indeed reshaped public opinion.

Table 7 reports on trends in public attitudes towards 10 Thatcherite issues (using 13 indicators) between 1974, shortly before Thatcherism emerged, and 1987. Few indicators cover the entire period. The entries here are based on proportions supporting Thatcherite positions and are drawn from three sources: the British Election Studies, the monthly Gallup Political Index and the BBC-Gallup Election Day Surveys.

This table spans the period during which Mrs. Thatcher has worked to re-educate the nation. It suggests that she has succeeded in shifting public opinion to the right on only one out of the 10 issues—denationalization. Her governments' massive sale of British industry, on a scale comparable to the nationalization program of the postwar Labour government, has converted public opinion. From a low point of 24% in 1974, support for dena-

**Table 7. Support for Thatcherite Policy Positions, 1970-87 (%)**

Issue	1970	1974	1979	1983	1987	Trend <sup>a</sup>
Privatization of nationalized industry	33	24	40	42	—	1974-83, +18
Death penalty	81	—	75	63	—	1970-83, -18
Abortion (gone too far)	—	43	44	32	—	1974-83, -11
Abortion (restrict)	41	42 (1973)	36 (1980)	—	—	1973-80, - 6
Pornography	—	64	66	64	—	1974-83, —
Repatriation of immigrants	—	43	34	—	—	1974-79, - 9
Equal opportunities for blacks	—	27	30	20	—	1974-83, - 7
Trade union power	73	81	82	74	—	1974-83, - 7
Illegitimacy of trade unions	29	33	41	28	25 (1986)	1974-86, - 8
Prices vs. jobs	—	60 (1976)	56 (1980)	27	14 (1986)	1976-86, -46
Profits vs. taxes to create jobs	—	—	74	61	53	1979-87, -21
Welfare benefits	—	34	50	20	—	1974-83, -14
Tax cuts vs. social services	—	—	37	24	13	1979-87, - 8

Note: *Don't knows* excluded from percentage base. Question wording is provided in Appendix B.

<sup>a</sup>Plus = more Thatcherite; minus = less Thatcherite.

tionalization rose to 40% by 1979 and to 42% by 1983.

The climate of public opinion on discipline-related issues presents a contrast. The Conservative party has traditionally been the party of law and order. But under Mrs. Thatcher's leadership the party has spoken even more persistently and stridently on these issues. Its 1979 manifesto, for example, promised a renewed debate on the death penalty (albeit on a "free," i.e., unwhipped, vote) early in the new Parliament and two further debates were held in 1983 and 1987: on each occasion Mrs. Thatcher voted for its restoration. And yet the proportion in favor of restoring the death penalty was greater in 1970 (81%) than in 1979 (75%) or 1983 (63%). The majority of British electors have always favored capital punishment and a hard line on crime and disorder. But the years after 1974 do not appear to mark a *resurgence* of tough-minded authoritarianism.

In the same vein, a connection between "permissiveness" and the breakdown of social order is commonly made by Thatcherites. Opposition to the liberalizing legislation on abortion, homosexu-

ality, divorce and censorship ushered in during the Labour governments of 1964 to 1970 is a recurrent Thatcherite theme. And yet Table 7 shows that support for Thatcherite positions on abortion and pornography, which has never been that substantial, did not increase between 1974 and 1983.

Changing public attitudes on issues of immigration and race tell a similar story. While the Conservative party is formally committed to equality before the law and equality of opportunity for all citizens, Mrs. Thatcher and her close supporters are associated with a hard line on black (i.e. Indian and Pakistani) immigration and relative indifference to discrimination and prejudice against black minorities. Mrs. Thatcher's widely publicized (if misinterpreted) remarks about the danger of Britain being "swamped" by too many black immigrants identified her section of the party with the view that blacks threatened the British national character, its law and democracy. Yet support for the repatriation of immigrants fell from 43% to 34% between 1974 and 1979 while the right-wing refrain that equal opportunity policies for blacks had "gone too far"

found an ever-weakening echo among the British electorate (down from 27% in 1974 and 30% in 1979 to 20% in 1983). Thus on matters of racial equality the British electorate remained moderate, edging gradually to a more liberal position during the Thatcher years.

Hostility towards trade unions, a statecraft and free enterprise issue, has ebbed and flowed; but there has been no Thatcherite tidal wave. After the 1974 defeat of the Heath government in the aftermath of the miners' strike, Thatcherites persistently painted trade unions as "the enemy within," whose monopoly powers and legal immunities needed to be ruthlessly stripped. While the Thatcher governments' trade union reforms have been popular, its more ambitious aim of persuading the electorate that trade unions are undesirable or unnecessary institutions has completely failed. For example, Gallup's annual question of 30 years' standing ("Generally speaking, and thinking of Britain as a whole, do you think that trade unions are a good thing or a bad thing?") has consistently produced only minorities saying "a bad thing" (after excluding *don't knows*) and smaller minorities in 1986 (25%) than in 1983 (28%), 1979 (41%) or 1974 (33%).

Mrs. Thatcher's crusade further assaulted the postwar consensus by claiming that it was not practical for governments to maintain full employment in all circumstances. In 1976 and 1979 the public accepted this view. But by the May 1983 election there was a dramatic reversal, which can be seen in Table 7 and which has since proceeded further. Similarly, in 1979, the electorate concurred with the Conservative view that jobs should be created by "allowing companies to keep more of their profits" rather than the Labour view that "it was up to the Government to use tax money to create jobs." But by 1983 all sections of the electorate had shifted leftward on the issue, and in 1987 they lurched further to

the left again. Nor has the electorate adopted a Thatcherite position on the welfare state. Before the 1979 election the party promised a drive against the welfare "scrounger." Since then the Thatcher governments have constantly reiterated the trade-off between taxation and welfare spending.

Once again, however, the electorate shows little evidence of having adopted a Thatcherite position on these issues. For example, between 1974 and 1979 the electorate did move to the right on the issue of whether benefits "nowadays have gone too far"; by 1983 it had reversed direction and was to the left of its original position in 1974. Nor have Thatcherites converted the electorate to the goal of a low-tax, minimal welfare economy. Asked to choose between tax and social service cuts or tax and social service increases, the public was evenly divided—37% apiece—when the Conservatives came to office in May 1979. By 1983 service increasers outnumbered tax cutters by 52% to 24%, and by May 1987 by the even greater margin of 64% to 13%. Mrs. Thatcher may have triumphed in 1987, but Samuel Smiles did not.

These findings from survey and polling data offer little support for the principal criticism of spatial theories, namely that electorates are pliable and that their outlooks can be shaped by political leaders. In the case of Thatcherism, a small number of Conservative leaders decided what they thought the public interest required and set out to persuade sceptical elites and the electorate. But by 1983 informal estimates of the number of Thatcherite MPs still only ranged from King's (1983, 97) 10% to Riddell's (1983, 12) "no more than 20–25 percent," the same range that we estimated for 1972–73. And as for the public, with the notable exception of denationalization, the voters actually sympathized less with Mrs. Thatcher's convictions after a decade of crusading than they did at the

beginning. Thatcherism reminds us that ideologies can arise from personal convictions that have little to do with maximizing votes. It also suggests that it can be far more difficult than Downs's critics think for political leaders to mold people's minds.

### Discussion

Mrs. Thatcher has made her way in the Conservative party by joining together components from two familiar perspectives: liberal Whiggery and traditional Toryism. Thus Thatcherism is recognizably Conservative even if it has neglected the ideas of the Conservative establishment. But Thatcherism is also a minority viewpoint, and as such it challenges spatial accounts of ideological construction (Norton 1987, 31). So strikingly does it depart from spatial models that its performance must convince us all of the existence of very important cases where politicians are more interested in moving electorates toward themselves than in moving themselves towards electorates (see Clubb, Flannigan, and Zingale 1980; Rabinowitz 1986).

Mrs. Thatcher has in fact demonstrated that a basic assumption of spatial models is not universal and is therefore not an appropriate axiom for deductive theories. Rational politicians can deliberately construct ideologies that draw their parties away from, not towards, the electorate. Yet these same spatial models work perfectly well in other postwar British cases. Under what conditions, then, are they most likely to apply? Consideration of Thatcherism in this broader postwar electoral context suggests three such conditions. Politicians are most likely to adjust their ideologies to appeal to electorates (1) after a devastating electoral defeat (2) when the victorious opponent's ideology is thought to be popular, and (3) when politicians' commitments to

their own party's ideology are relatively weak. None of these conditions were satisfied when the Thatcherite counterexample emerged after 1973. But all three were very plainly present in the postwar cases that best support spatial theory: the Conservative party's reaction to defeat in the 1945 general election, and the similar Labour reactions to defeat in the general elections of 1933 and 1987.

Responding to a devastating landslide in the 1945 general election, leading Conservative politicians were quick to learn what they called the new electoral lessons. They set out to adjust the party's goals and overhaul its policy programs accordingly (Hoffman 1964). The ideology of the victorious party, Labour's ideology, appeared to be as popular in 1945 as the Conservative party's appeals appeared outdated. By the beginning of the 1950s the Conservatives had moved towards the electoral center to accept the basic outlines of the mixed economy and welfare state that Labour was putting in place (Beer 1965, 302). Finally, as Lord Butler (1971, 92), the architect of this ideological overhaul, explains in his memoirs, the move was greatly facilitated by an awareness among leading Tories that Disraeli's two nations still existed in England and that it was time to do something about "the injustices and anomalies of the pre-war period"—which Conservatives had administered.

The right conditions were in place, too, for the Labour party's steady retreat from radicalism between 1983 and 1987. The majority of Labour MPs had never supported the party's lurch to the Left after 1979 but had been outflanked by embittered constituency and trade union activists exploiting a "democratic" policy-making process. At the 1983 election, Labour suffered a devastating defeat, its worst since 1918 in terms of share of the vote, despite the severity of the economic recession under Mrs. Thatcher. The subsequent attempts by Labour's new leader,

Neil Kinnock, to rescue the party have been almost purely Downsian: electoral victory, he has declared, must take priority over socialist purity because without winning elections socialism cannot be put into practice.

Electorally damaging policies from the 1983 manifesto have either been reversed (e.g., withdrawal from the EEC, the abolition of the Lords, opposition to council house sales), relegated to the sidelines (e.g., renationalization, rechristened "social ownership"), or, where politically necessary, smothered by creative ambiguity (e.g., the restoration of trade union rights and immunities). The almost exclusive emphasis has instead been on the Labour party's traditionally popular commitment to the welfare state. These shifts of policy position and emphasis, moreover, have not been couched in the socialist rhetoric of equality, social justice, or class war but in the bland centrist language of "mending divisions," "community and caring," or the almost Thatcherite language of "opportunity" and "efficiency" (Kinnock, 1986; Labour Party 1987). Indeed, two of Labour's leading politicians have refurbished democratic socialism in the style of "freedom" (Gould 1985; Hattersley 1987).

Returning to Thatcherism, all three conditions were lacking when contrary to Downsian theory this radical ideology was constructed. As regards the first condition, the Conservative party was defeated in 1974, but *not* overwhelmingly, let alone devastatingly. As for the second, what appealed to the electorate was not the victorious Labour party's ideology so much as the prospect of getting rid of Mr. Heath's government. And finally, the Heath government's U-turns left Conservative politicians irritated and uneasy but *not* less committed to existing doctrines than they had been before. Mrs. Thatcher was therefore not required to present convincing electoral justifications for her radical convictions—convictions

that had less to do with winning elections than with "binding together the faithful and providing a test of membership in the elect."<sup>18</sup>

And yet Mrs. Thatcher won. Despite the unpopularity of her ideology, she won the leadership election of 1975; and she won the general elections of 1979, 1983, and 1987. In the leadership election, Conservative MPs cared more about removing Mr. Heath than they did about her ideology (Riddell 1983, 21; Wapshott and Brock 1983, 106). In the general election of 1979, she benefited from Labour's collapse during the "winter of discontent"; and in both 1983 and 1987 she was re-elected to office because the anti-Conservative vote was divided between two opposition parties (which were themselves internally divided). Thus Mrs. Thatcher's electoral successes and the policy revolutions she has carried out in their wake further require us to make theoretical room for the fact that it is possible in contemporary democracies not only to "get it right" and nevertheless to get elected but even to get elected again (and again) and successfully implement radical political programs that are unpopular with most political leaders and voters.

### Appendix A: The Value-ranking Instrument

The value ranking instrument consists of an inventory of 36 political values arranged in four separate lists of nine values each (Searing 1978). This inventory was assembled from parliamentary debates, political memoirs, journalistic commentaries, and academic attempts at synthesis and was reviewed through pretests with former members of Parliament. The rank order instrument is based on the assumption that individuals hold cognitive value hierarchies, that is to say, that they generally give certain values precedence

over others and that these hierarchies can be roughly reconstructed on the values form. Respondents are asked to examine each list separately and to write the number 1 alongside the ideal that is personally most important to them, the number 2 alongside the next most important value, and so on. The items in these lists were familiar to the politicians, and most respondents found the exercise easy to complete. Nonetheless, it should be emphasized that value hierarchies, particularly those of many Conservatives, are certainly not inflexible and may not even be well crystalized. In fact, the exercise compels respondents to represent their political values with more structure than usually characterizes their thinking about such matters.

In the comparison between the Monday Club and PEST, a value's *distinctiveness* is indicated by the size of the difference between its score for the Monday Club and its score for PEST, a figure reported in the last column of Table 1. On this criterion, free enterprise heads the list, followed closely by public order, authority, and patriotism.

In the exploratory factor analysis, several values were combined into additive indexes and several others were removed altogether in order to reduce the large number of value items and to suppress noise. Thus, on the basis of connotations and correlations, the following items were combined into additive indexes: strong government + strong leadership = statecraft; self-discipline + self-reliance = autonomy; authority + discipline = authority; empirical approach + gradualism = empirical; and, property + capitalism = capitalism. The following values were dropped from the analysis either because they focused on distinctions in the Labour party or were too skewed or both: economic equality, freedom, socialism, privacy, unity, social equality, duty, fellowship, meritocracy, participatory democracy, loyalty,

cooperation, and security (see Searing 1978). After these steps, 18 vetted items remained with which to assess the argument.

## Appendix B: Items for Table Entries

Table 6

1. "Profitable sectors of nationalized industries should be denationalized."
2. "Social service benefits and exemptions ought to be provided only to the really poor rather than to all low-wage earners."
3. "Trade union organizations do not have an automatic right to be consulted about the making of policies which affect their interests."
4. "Business organizations do not have an automatic right to be consulted about the making of policies which affect their interests."
5. "The death penalty should be re-introduced for the murder of police officers."
6. "Criminals convicted of violent crimes should be given longer prison sentences and fewer paroles."

Table 7

*Privatization of nationalized industry.* "Which of these statements comes closest to what you yourself feel should be done?"—"Some of the industries that are nationalized now should be denationalized." *Source: British Election Studies.*

*Death Penalty.* 1970: "Did you want to see the death penalty kept or abolished?"; 1979: "I am going to read out a list of things that some people think a government should do. For each one you can say whether you feel it is: very important that it should be done, fairly important that it should be done, it doesn't matter either

way, fairly important that it should not be done, very important that it should not be done. . . . What is your view about bringing back the death penalty?"—"Very/fairly important that it should be done"; 1983: "Please say whether you agree or disagree with each of these statements, or say if you are not sure either way: . . . Britain should bring back the death penalty"—"Agree." Source: *British Election Studies*.

*Abortion (gone too far).* "Would you say that the availability of abortion on the National Health Service: has gone much too far; has gone a little too far; is about right; has not gone quite far enough; has not gone nearly far enough?"—"Has gone much/a little too far." Source: *British Election Studies*.

*Abortion (restrict).* "Do you think that the law should make legal abortion easier, more difficult or should the situation be left as it is?"—"More difficult." Source: *Gallup Political Index*.

*Pornography.* "Would you say that the right to show nudity and sex in films and magazines: has gone much too far; has gone a little too far; is about right; has not gone quite far enough; has not gone nearly far enough?"—"Has gone much/a little too far." Source: *British Election Studies*.

*Repatriation of immigrants.* "I am going to read out a list of things that some people say a government should do. For each one you can say whether you feel it is: very important that it should be done, fairly important that it should be done, it doesn't matter either way, fairly important that it should not be done, very important that it should not be done. . . . What is your view about sending coloured immigrants back to their own country?"—"Very/fairly important that it should be done." Source: *British Election Studies*.

*Equal opportunities for blacks.* "Would

you say that attempts to give equal opportunities to black people and Asians: have gone much too far; have gone a little too far; are about right; have not gone quite far enough; have not gone nearly far enough?"—"Have gone much/a little too far." Source: *British Election Studies*.

*Trade Union Power.* "Do you think that trade unions have too much power or not?"—"Too much power." Source: *British Election Studies*.

*Illegitimacy of trade unions.* "Generally speaking, and thinking of Britain as a whole, do you think that the trade unions are a good thing or a bad thing?"—"Bad thing." Source: *Gallup Political Index*, no. 296.

*Price vs. jobs.* "Which do you think the government should give greater attention to—trying to curb inflation or trying to reduce unemployment?"—"Curb inflation." Source: *Gallup Political Index*.

*Profits vs. taxes to create jobs.* "Which of these statements comes closest to your view? The best way to tackle unemployment is to allow private companies to keep more of their profits to create more jobs; It is mainly up to the government to tackle unemployment by using tax money to create jobs"—"Allow private companies to keep more of their profits." Source: 1979: *British Election Study*; 1983 and 1987: *BBC-Gallup Election Survey*.

*Welfare Benefits.* "Thinking about the welfare benefits that are available to people today, would you say that these have: gone much too far; gone a little too far; are about right; not gone quite far enough; not gone nearly far enough?"—"Gone much/a little too far." Source: *British Election Studies*.

*Tax cuts vs. social services.* "People have different views about whether it is more important to reduce taxes or keep up government spending. How about you?



Which of these statements comes closest to your view? Cut taxes, even if this means some reduction in government services, such as health, education and welfare; Things should be left as they are; Government services such as health, education and welfare should be extended, even if it means some increases in taxes—"Cut taxes." Source: *Gallup Political Index*.

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1. According to Downs (1957, 114-27), in a two-party system like Britain's, where voters' opinions are unimodal and normally distributed, both parties will tend to converge ideologically toward the center of the political spectrum. To march away from the center would be irrational because it would invite electoral punishment. Downs (1957, 96-113) argues further that politicians do not regard ideologies as ends in themselves but rather as means to other ends, these being "whatever benefits them most." And these benefits, according to Downs's "basic hypothesis," are the "power, income, and prestige that go with office." Thus the ultimate goals are power, income, and prestige; the proximate goal is winning office; and the instrumental goal is the creation of an ideology that will appeal to voters. In short, ideologies are "voter-driven."

2. Although political values are typically considered more salient to politicians than to the general public, the public is expected to hold such values too. Indeed, according to Downs (1957, 98-99), it is for them that politicians create general political ideologies in the first place: the public is expected to find such ideologies useful short-cuts for evaluating parties and thereby avoiding the burdens of investigating the details of a wide range of issues.

3. When the analysis was repeated for members of known ideological communities—the Monday Club and PEST—a relatively strong first factor did

emerge to structure the data. This was defined at one end by authority and discipline, Monday Club favorites, and at the other by compassion and community—PEST's ideals. Conservative politicians whose views are highly crystallized are apparently not in the majority. Thus when the analysis was repeated again, this time for respondents who were members neither of the Monday Club nor of PEST, the percentage of variance explained by the early factors was cut in half, while the eigen values displayed an even more modest stepwise decline than was found for the entire sample.

4. This step produced 149 respondents classified as predominantly Tories and 97 as predominantly Whigs, proportions in line with general impressions of the rough distribution of such views in the party at the time.

5. It needs to be emphasized that even among these values the structure is weak. Although we are primarily interested in the first principal component, an inspection of the complete factor analysis presents a rather flaccid picture: a varimax rotation recovers eight dimensions with eigen values of about 1.00, which together explain only 69% of the variance. Conservative politicians are correct when they claim that the party's ideological perspectives are not highly crystallized, that value clusters are not sharply defined, and that there is much overlapping between one tendency and another.

6. Its strength compared to the second factor is somewhat more impressive than that found with the Whigs. After a relatively sharp drop from the first to the second factor, the decline across the other components is more steady, as is the decline in percentage of variance explained. At the same time, the overall structure is again weak with seven factors at eigen values of about 1.00 explaining together only 62% of the variance.

7. The present analysis was applied to Conservative members of Parliament and candidates together. This has the advantage of producing a large  $N$  and of broadening the elite net to capture some opinion outside Westminster. But members of Parliament were also analyzed separately and the results were much the same: the eigen values and percentage of variance explained were slightly higher for the Whigs and slightly lower for the Tories. Moreover, the correlations between the value items and the first principal component were virtually identical. This reinforces results reported elsewhere that suggest that Conservative MPs and candidates have quite similar value profiles (Searing 1978).

8. To some extent, this is due to the choice of components from which the authority index has been constructed, albeit they are strongly intercorrelated ( $\text{Gamma} = .50$ ).

9. Free enterprise is ranked at the top (1-3) by 66% of the liberal Whigs but by only 23% of the traditional Tories. Patriotism is accorded the same

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## Ideological Change

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importance by 40% of the liberal Whigs but by 84% of the traditional Tories. This analysis of comparative ranking distributions was designed to explore the foundations of similarities and differences between overlapping subtypes. Whigs were partitioned into liberal and corporate according to whether strong leadership or social planning received the higher rank. Tories were partitioned into traditional and progressive according to their relative ranks for patriotism and compassion. The comparison of the performances of the liberal Whigs and the traditional Tories showed that in addition to the divisions over free enterprise and patriotism, they differ over the type of authority they prefer. Liberal Whigs hold firm on discipline while falling a little behind the traditional Tories on authority ('the right to command'). The two viewpoints are in closest accord over strong leadership and strong government, the components of the statecraft index, for which approximately two-thirds from each camp awards top marks (1-3).

10. The obvious and predicted difference here is that the corporate Whigs care much more about capitalism than do the progressive Tories, the proportions giving it top marks being 64% and 18% respectively. But the progressive Tories—again as would be anticipated—are more responsive than the corporate Whigs to social progress (64% to 46%).

11. (Cf. Norton 1987, 31). Potential sympathy for Thatcherism diminishes as we rise in rank from candidates to backbenchers to junior ministers to senior ministers, although this shrinkage is much too modest to characterize Thatcherism as an ideological "peasant's rebellion" (see Riddell 1983, 10). Despite the claims at the time, there is remarkably little difference between frontbenchers and backbenchers.

12. For both Tories and Whigs, the varimax rotations recover six dimensions with eigen values above 1.00, explaining in each case approximately two-thirds of the variance. The eigen values of the first factors (1.54) are considerably weaker than those of the politicians, and they are not substantially stronger than those of the next components. On the Tory side, the progressive Tory values of compassion and community anchor one pole, while the traditional Tory values of patriotism and strong government anchor the other. The most obvious inconsistencies with the configuration for MPs are that public order moves into the progressive territory while free enterprise improves its standing in traditional lands. Turning to the Whig results, free enterprise defined the liberal pole; but capitalism fails to come through to establish corporate Whiggery at the other end.

13. Authority is substituted for discipline, which was not included in the public opinion survey but with which authority was highly correlated among MPs.

14. The items were satisfactorily intercorrelated, with Gammas ranging from .38 to .52. They all en-

compass Thatcherite policy views. Thus item 1 in Table 6—denationalizing profitable sectors of nationalized industries—is in keeping with Thatcherism's free enterprise beliefs and commitment to reducing the size of the state sector. Item 2—the provision of social service benefits to the poor only—is consonant with Thatcherism's aim of shrinking the welfare state in order to reduce tax burden. Thatcherism's conception of statecraft involves an autonomous center and thus brooks no alternative sources of power, especially not the trade unions. Thus the higher the score on the Thatcherism index, the less likely Conservative politicians are to believe that trade unions or business organizations (items 3 and 4) have an automatic right to be consulted about the making of policies that affect their interests. Thatcherism's Victorian morality, which is injected into our index through the discipline value, draws decisive conclusions about law and order. Mrs. Thatcher has been a prominent public advocate of reintroducing the death penalty for the murder of police officers (item 5), as well as of seeing that criminals convicted of violent crimes are given stiffer prison sentences (item 6).

15. The respective significance levels for the items are as follows, with the politicians' result reported first and then the public's: item 1, .0001 and .0006; item 2, .02 and .22; item 3, .04 and .17; item 4, .003 and .35; item 5, .0000 and .08; item 6, .001 and .003.

16. In fact, between 1964 and 1974, the decade preceding the emergence of Thatcherism, public attitudes were not skewed towards denationalization but were evenly balanced between further nationalization and privatization, with the plurality in favor of the status quo (source: British Election Studies).

17. By 1974, the public's widespread belief that trade unions are over-mighty had reached its peak (in that year 81%, excluding *don't knows*), said that "trade unions have too much power," and Mrs. Thatcher's proposals for trade union reform were therefore very popular (source: British Election Studies).

18. James Douglas in a private communication.

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## EXPLAINING SENATE ELECTION OUTCOMES

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**A**ggregate-level data are used in this analysis to explain the outcomes of Senate elections between 1974 and 1986. Using the individual Senate contest as the unit of analysis permits estimating the relative influence of a wide variety of factors on Senate election results including political characteristics of states, characteristics of the candidates, and national political conditions. Of these factors candidate characteristics had the strongest impact on the outcomes of Senate elections. The importance of candidate characteristics has had two major consequences for Senate elections. First, two-party competition has spread to every region of the country: in Senate elections, no state can be considered safe for either party. Second, money is probably now more important than ever, especially for challengers and candidates for open seats.

**B**icameralism is one of the central features of the United States Congress. The framers of the Constitution created two legislative chambers with distinctive political characteristics that affect both the internal organization and decision-making processes of the Senate and House of Representatives (Froman 1967; Ornstein 1981) and the nature of Senate and House elections (Fenno 1982).

The relatively small number of senators, the size and political importance of Senate constituencies, the length of Senate terms, and the special constitutional responsibilities of the Senate all contribute to the political visibility of individual senators. Most House members are well known only to the voters in their own district. Only the Speaker and a few other leaders are national political figures. In contrast, many senators are well known outside of their home states.

The major reason for the relatively high visibility of members of the upper chamber is that media coverage of senators and Senate campaigns is much more

extensive than media coverage of House members and House campaigns (Clarke and Evans 1983; Fenno 1982; Hess 1986). For senators this attention has both advantages and disadvantages. Senators are much more likely to be considered potential candidates for the presidency; however, senators also have much less control than members of the House over the information their own constituents receive about their performance. Moreover, the size of most states' populations limits personal contact between senators and constituents, so voters' impressions of senators are probably based almost entirely on information supplied by the printed and electronic media.

The influence and visibility enjoyed by individual senators have one other political disadvantage: when incumbent senators seek reelection they often attract prominent, well-financed challengers. A large proportion of Senate challengers in recent years had held a major elected office. Several other Senate candidates achieved prominence outside of politics, including three former astronauts, a

former professional basketball player, two former Vietnam POWs, and the sixth husband of actress Elizabeth Taylor. These elected officials and celebrities not only enjoyed high public visibility, but they also usually spent large sums of money on their campaigns. A number of other Senate candidates in recent years have been multimillionaires who were willing to invest large amounts of their personal wealth in their own campaigns. As a result, many Senate challengers have been able to use television advertising extensively in their campaigns.

Of course not all Senate challengers are prominent or well-financed (Westlye 1983). However, the fact that many Senate challengers are well-known public officials, celebrities, or millionaires helps to explain why Senate incumbents have greater difficulty holding their seats than House incumbents. The visibility of the challenger is probably the most important variable influencing an incumbent representative's or senator's chance of reelection (Abramowitz 1980; Hinckley 1980; Mann and Wolfinger 1980).

The visibility of Senate challengers and extensive media coverage of Senate campaigns may also increase voters' awareness of candidates' issue positions and ideologies. National and international issues appear to play a larger role in Senate campaigns than in House campaigns (Fenno 1982). Presidential campaigning for Senate candidates and the involvement of ideological political action committees such as the National Conservative Political Action Committee (NCPAC) have probably increased the ideological content of Senate campaigns in recent years. As a result, voters' ideological preferences appear to have a significant influence on their voting decisions in Senate elections (Abramowitz 1980; Jacobson and Wolfinger 1987; Wright and Berkman 1986). National issues such as the president's performance and the state of the economy may also

have an important influence on voting decisions in Senate elections, although there has been little research on this question (Abramowitz and Segal 1986; Lewis-Beck and Rice 1985).

A variety of factors may affect the outcomes of Senate elections, including the partisan and ideological preferences of the electorate, state population size, the incumbent's voting record, scandals and controversies, intra-party conflicts, the backgrounds of the candidates, campaign spending, and national political conditions. There has been much less empirical research on Senate elections than on House elections. Existing national survey data are not well suited for the purpose of analyzing voting in Senate elections. There have also been very few studies of Senate elections using aggregate-level data because the small number of Senate contests in any election year makes the task of estimating the effects of factors such as presidential popularity or economic conditions much more difficult than in House elections. However, by using the individual Senate contest as the unit of analysis, it is possible to estimate the relative influence of a wide variety of factors on the results of Senate elections, including political characteristics of the states, characteristics of the candidates, and national political conditions.

## Hypotheses To Be Tested

### State Characteristics

The first set of hypotheses to be tested involves the political characteristics of the states. Three characteristics that should affect Senate elections are the partisan composition of the electorate, the ideological composition of the electorate, and the size of the state's population. Although the proportion of citizens identifying with the Democratic and Republican parties and the influence of party identification on voting decisions have both declined in

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## Senate Election Outcomes

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recent years, party labels still provide an important cue for many voters. The proportions of Democratic and Republican identifiers in a state should have a significant impact on the outcomes of Senate elections.

In addition to partisanship, the ideological composition of the electorate may affect the outcomes of Senate elections. Voters generally perceive the Democratic party as more liberal than the Republican party and voters' ideological identifications have had significant effects on their candidate preferences in several recent presidential elections (Holm and Robinson 1978; Levitin and Miller 1979). Given the salience of national issues in many recent Senate campaigns, ideological identifications may also affect voting for Senate candidates: the proportion of liberal identifiers in a state should have a positive impact on the vote for Democratic Senate candidates.

Population size may affect the outcomes of Senate elections in a manner somewhat different from either partisanship or ideology. The size of a state's population should affect not support for the parties but support for the incumbent senator. The larger the population of a state, the more difficult it may be for a senator to cultivate the support of the voters (Hibbing and Brandes 1981). As population size increases, so does political and economic diversity; in addition, the size of a state's population should be inversely related to personal contacts with voters. However, the impact of population size on support for the incumbent may not be linear. As a state's population increases, the ability of an incumbent senator to cultivate support through personal contact with constituents should diminish fairly rapidly and then level off: once a state's population reaches several million, personal contact with constituents becomes impractical and a senator probably must rely on the mass media to communicate with citizens.

### Candidate Characteristics

The second set of hypotheses to be tested involves the characteristics of the Senate candidates. When an incumbent is seeking reelection, the incumbent's past performance is usually the major focus of the campaign: the challenger criticizes the incumbent's record and the incumbent defends that record. Several aspects of the incumbent's performance may affect the outcome of a Senate election, including the incumbent's voting record, scandals or political controversies, health concerns, and intra-party opposition.

The key question concerning the incumbent's voting record is its congruence with the ideological preferences of the state's voters. The greater the distance between a senator's ideology as reflected by his voting record and the ideology of the average voter, the greater should be the challenger's opportunity to win votes by taking a position closer to that of the average voter in the state.

Involvement in well-publicized scandals or political controversies should also reduce support for an incumbent officeholder (Peters and Welch 1980). Although no member of the Senate was indicted or convicted of a crime during the period covered by this study, several senators were the subjects of investigations based on allegations of official or personal misconduct. A larger number were involved in incidents that raised questions about their honesty, personal judgment, or competence. Even though no official action was taken against any of these senators, media coverage of such incidents probably had a negative impact on the incumbents' reputation among their constituents.

Along with scandals and political controversies, questions about a senator's health or physical vigor may also have a negative impact on electoral support for the incumbent. The issue here is not age—senators in their seventies or eighties have

had no difficulty winning reelection—but whether or not an incumbent can vigorously meet the demands of the job.

One additional potential problem for an incumbent senator is intra-party opposition. Serious opposition to an incumbent senator in his own party's primary is relatively rare. When such opposition does emerge, however, it may cause political problems for the incumbent even if he wins the primary. Criticism of the incumbent's performance by a serious intra-party challenger may lead to an erosion of public support that carries over into the general election.<sup>1</sup>

Even if there are weaknesses in an incumbent senator's record and a strong challenger emerges in the opposing party, the incumbent has one additional resource at his disposal: the ability to raise and spend large sums of money on his reelection campaign. An incumbent senator can usually outspend even a prominent and well-financed challenger. However, it is not clear how much good this does. Although there has been almost no research on the impact of campaign spending in Senate elections, studies of campaign spending in House elections have generally found that spending by incumbents has little or no impact on their electoral fortunes; only the challenger's spending has a substantial impact on the outcome of a House election (Glantz, Abramowitz, and Burkart 1976; Jacobson 1978, 1980). The explanation for this finding is that the incumbent is already well known by the time the campaign gets under way, whereas the campaign is the only opportunity for the challenger to get his message across to the electorate.

There is a strong likelihood that campaign spending does not have a simple linear effect on the outcomes of Senate elections. Spending is probably subject to diminishing marginal returns: as a candidate spends more money and becomes better known, the impact of further spending diminishes. Since incumbents

generally begin the campaign with a much higher level of recognition than challengers, their spending should have a lower initial rate of return than that of challengers.

In estimating the impact of campaign spending in Senate elections there is an additional complication that is not present in House elections: the wide variation in state populations. A Senate campaign in California will be much more expensive than an equivalent campaign in Delaware. However, simply dividing spending by population is probably also inaccurate. While many costs of waging a campaign increase with population, other costs are relatively fixed. Broadcasting charges are based on audience size, but other costs, such as producing political advertisements, hiring professional consultants, and conducting polls, are probably not greatly affected by the population of a state. Thus in order to estimate the impact of campaign spending in Senate elections, it is necessary to adjust candidate spending to reflect both fixed and variable campaign costs.

Although a Senate campaign involving an incumbent usually revolves around the record of the incumbent, the ability of the challenger to effectively criticize the incumbent's record also depends on the resources and political skills of the challenger. The next set of hypotheses to be tested in this study involves the characteristics of Senate challengers. Three characteristics of Senate challengers which should affect their ability to conduct an effective campaign are political experience, celebrity status, and campaign spending.

A challenger who has held elected office has several advantages over one who has never won an election. The experience of conducting at least one successful campaign should be an advantage in organizing a Senate campaign. Depending on the importance and visibility of the office, the challenger may begin the campaign with a

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## Senate Election Outcomes

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high level of recognition among the electorate. Holding elected office probably also enhances a challenger's credibility among both the voters and members of the media. A challenger who has held an important elected office will probably receive more extensive and favorable coverage in the media and will be taken more seriously by the voters than a challenger who has never held elected office.

A challenger who has achieved prominence through activities other than holding elected office should enjoy many of the same advantages as one who has held elected office. A famous athlete, entertainer, astronaut, or military hero will begin the campaign with higher public recognition and will probably receive more extensive and favorable media coverage than a challenger who is not a celebrity.

Challengers who are not celebrities and have never held elected office can attempt to compensate for these deficiencies by spending more money on their campaigns. Of course this is much easier for challengers who are independently wealthy and can contribute large sums of money to their own campaigns. Although challenger spending should have a greater impact on election outcomes than incumbent spending, it is probably also subject to diminishing marginal returns. And just as in the case of incumbent spending, challenger spending must be adjusted to take into account both fixed campaign costs and costs that are based on population.

### National Political Conditions

In addition to characteristics of the states and candidates, Senate elections are also influenced by national political conditions. Two variables reflecting the national political climate will be examined in this study: party competence evaluations and whether a Senate contest takes place in a midterm or presidential election year.

Political scientists have long recognized that the outcomes of House elections are influenced by national issues such as the state of the economy and the popularity of the incumbent president (Kramer 1971; Tufte 1975). Although Senate elections may be less predictable than House elections because of the smaller number of contests involved, they appear to follow the same pattern as House elections: the greater the president's popularity and the stronger the economy, the more seats the president's party can expect to win (Abramowitz and Segal 1986; Lewis-Beck and Rice 1985).

Abramowitz, Cover, and Norpoth (1986) have argued that the effects of economic conditions and presidential popularity on congressional elections are mediated by short-term party evaluations. What connects congressional candidates with the performance of the president and the state of the economy is the voter's perception of the relative competence of the Democratic and Republican parties. Including party competence evaluations in our model should capture the effects of economic conditions and presidential performance on Senate elections.

One of the most regular patterns of U.S. electoral politics is the tendency of the president's party to lose seats in the House of Representatives in midterm elections. This pattern is based partially on the fact that the president's party usually has won more than its normal share of seats in the preceding presidential election year. In addition, however, the party controlling the White House may lose support in midterm elections because of negative voting: when the president himself is not on the ballot, voters who are dissatisfied with his performance may take out their discontent on candidates representing his party. Thus, in midterm elections, negative evaluations of the incumbent president may have a stronger impact on voting decisions than positive evaluations



of the incumbent president (Cover 1986; Kernell 1977). This phenomenon may help to explain why the president's party usually loses Senate seats in midterm elections, even though the same seats were not at stake in the preceding presidential election year. Since 1936 the president's party has lost Senate seats in 10 of 13 midterm elections, with an average loss of 4.7 seats.

### Open Seats

In the preceding discussion, I have assumed that a Senate contest involves an incumbent seeking reelection and a challenger. However, almost one-fourth of all Senate contests between 1974 and 1986 did not involve an incumbent. Many of the same factors expected to influence races involving an incumbent should also affect contests for open seats: the partisan and ideological composition of the electorate in the state, the backgrounds and political experiences of the candidates, campaign finances, and national political conditions. However, there is a qualitative difference between a race involving an incumbent and a contest for an open seat. When an incumbent is seeking reelection, one expects the incumbent's performance to be the main focus of the campaign. The challenger's background and qualifications, while important, are usually less salient than the incumbent's record. By contrast, a contest for an open seat generally involves a choice between two equally salient alternatives. As a result, the candidates' qualifications and financial resources may have a stronger impact on the outcome of a contest for an open seat than the challenger's qualifications and finances have on the outcome of a contest involving an incumbent.

### Data and Methodology

Data for this study are based on all contested Senate races between 1974 and

1986. Information regarding campaign spending in Senate elections was not available before 1974. The dependent variable used in the study is the percentage of the major party vote received by the incumbent senator or, in a contest for an open seat, the percentage of the major party vote received by the Democratic candidate. The measures used for each of the independent variables are described in the Appendix.

In order to test the hypotheses presented above, I will estimate two models of Senate election outcomes: an incumbent support model and an open seat model. The incumbent support model can be summarized as follows:

$$IV = a + b_1P + b_2I + b_3CD + b_4D + b_5S + b_6C + b_7H + b_8PC + b_9PM + b_{10}I\$ + b_{11}CPE + b_{12}CC + b_{13}C\$ + b_{14}NPC + b_{15}MT + e$$

where *IV* is the percentage of the major party vote received by the incumbent; *a* is a constant; *P* is the *partisan* orientation of the state's electorate; *I* is the *ideological* orientation of the state's electorate; *CD* is the natural logarithm of the number of *congressional districts* in a state; *D* is the *distance* between the incumbent's ideology as reflected by his voting record and the ideological position of the average voter in the state; *S* is a dummy variable which indicates the presence or absence of a *scandal* involving the incumbent; *C* is a dummy variable indicating the presence or absence of a political *controversy* involving the incumbent; *H* is a dummy variable indicating the presence or absence of questions about the incumbent's *health* or physical condition; *PC* is a dummy variable which indicates whether or not the incumbent had a *primary contest*; *PM* is the incumbent's *margin* of victory in percentage points over his closest *primary* challenger; *I\$* is the natural logarithm of the *incumbent's* campaign *spending* adjusted for state pop-

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**Table 1. OLS Estimates for Incumbent Support Model**

Independent Variable	<i>b</i>	Standard Error	Beta	Significance Level
State partisanship	.14	.03	.22	.001
State ideology	.15	.03	.26	.001
Natural log of state size	-.70	.41	-.07	.05
Ideological distance	-.04	.02	-.12	.01
Scandal	-7.11	2.41	-.13	.01
Controversy	-3.07	1.47	-.09	.025
Health	-8.28	3.00	-.12	.01
Primary contest	-6.75	1.78	-.35	.001
Primary margin	.10	.03	.37	.001
Natural log of incumbent spending	1.31	.62	.11	.025
Challenger political experience	-1.94	.49	-.20	.001
Celebrity challenger	-5.74	2.48	-.10	.025
Natural log of challenger spending	-3.63	.43	-.49	.001
National party competence	.16	.04	.21	.001
Midterm	-1.89	.60	-.15	.001
Constant	69.75	2.18		

ulation and inflation; *CPE* is the *challenger's political experience*; *CC* is a dummy variable indicating whether or not the *challenger* was a *celebrity*; *C\$* is the natural logarithm of the *challenger's* adjusted campaign *spending*; *NPC* is the *national party competence* score for the year of the election; *MT* indicates whether a contest took place during a *midterm* election when the president was a member of the incumbent's party or the challenger's party; and *e* is an *error* term.

The open seat model can be summarized as follows:

$$DV = a + b_1P + b_2I + b_3C\$ + b_4CPE + b_5NPC + b_6MT + e$$

where *DV* is the *Democratic* candidate's share of the major party *vote*; *P* is the *partisan* orientation of the state's electorate; *I* is the *ideological* orientation of the state's electorate; *C\$* is the difference between the natural logarithm of the *Democratic candidate's* adjusted campaign *spending* and the natural logarithm of the *Republican candidate's* adjusted campaign *spending*; *CPE* is the difference between the *Democratic candidate's*

*political experience* and the *Republican candidate's* political experience; *NPC* is the *national party competence* score for the election year; *MT* indicates whether a contest took place during a *midterm* election with a *Democratic* or *Republican* president; and *e* is an *error* term.

## Results

Both models were estimated using ordinary least squares regression analysis. The results for the incumbent support model are presented in Table 1. All of the estimated coefficients are statistically significant and in the expected direction. The overall fit of the model appears to be very good: almost three-fourths of the variance in the incumbent's share of the vote is accounted for by these 15 independent variables. The standard error of 5.0 percentage points in the "predictions" made by the model is also rather impressive. This figure comes close to the expected margin of error of a public opinion poll conducted immediately before an election.

### Effects of State Characteristics

The most interesting results of the regression analysis are the estimated effects of each of the independent variables on the outcomes of Senate elections. Looking at the effects of state political characteristics, for example, we find that the ideological orientation of the electorate was just as important as the partisan orientation of the electorate. Although Democratic candidates enjoyed an advantage in most states because there were more Democratic than Republican identifiers, this advantage was offset by the fact that in all 48 states included in this study there were more conservative voters than liberal voters.

In order to evaluate the relative positions of the Democratic and Republican Senate candidates in a state, it is necessary to consider both the partisan and ideological composition of the electorate in that state: some of the most Democratic states, in terms of partisanship, are also among the most conservative states in the nation. Based on partisanship and ideology, the most favorable states for Republican candidates were Utah and Idaho, while the most favorable states for Democratic candidates were Georgia and Louisiana. With all other factors held constant, a Democratic Senate candidate in Georgia or Louisiana could expect to receive about 3% more of the vote, while a Democratic Senate candidate in Utah or Idaho could expect to receive about 7% less of the vote, than a Democratic Senate candidate in an average state. While these are substantial differences, no state can be considered safe for either party in a Senate election.

As expected, support for the incumbent senator decreased as state population increased. A senator from a state with only one House district, such as Delaware or Wyoming, could expect to receive about 3% more of the vote than a Senator from California, all other factors held constant.

### Effects of Candidate Characteristics

According to my results, a senator whose voting record was inconsistent with the ideological preferences of his or her constituents could expect to pay a substantial price in electoral support for this discrepancy. A very liberal senator representing a very conservative state or a very conservative senator representing a very liberal state could expect to receive 4.3% less of the vote than a senator whose voting record matched the preferences of the constituents.

During the period covered by this study, there were several senators whose voting records demonstrated a marked discrepancy with the ideological preferences of the voters in their states. It is probably no coincidence that most of these senators lost reelection bids to challengers whose views were more in tune with the voters. George McGovern, Frank Church, and James Buckley suffered the same fate as Edmund Burke, who pledged to vote for his conscience rather than his constituents' wishes and discovered that the voters preferred to be represented by a delegate rather than a trustee.

According to the results presented in Table 1, senators who were involved in scandals experienced a substantial loss of electoral support: an average drop of 7.1 percentage points, other factors held constant. Four of the 5 senators accused of being involved in illegal activities were defeated when they sought reelection. Controversies that raised questions about a senator's honesty or competence were much more common than full-blown scandals and far less costly: the average decline in electoral support was only 3.1 percentage points. However, 7 of the 14 Senators involved in political controversies were defeated. Three Senators with significant physical or health problems sought reelection between 1974 and 1986. Only 1 of the 3 was reelected and this issue produced an average decline in elec-

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toral support of 8.3 percentage points.

Senators who had significant opposition in their own party's primary experienced a substantial loss of support in the general election. The estimated coefficients for the primary contest and primary margin variables indicate that to avoid any negative impact from the primary, the incumbent had to defeat his closest challenger by a margin of at least 67 percentage points. An incumbent who barely survived a primary challenge (winning by a margin of less than 1 percentage point) could expect a decline in support of 6.8 percentage points in the general election.

The challenger's characteristics also had a strong impact on the outcomes of Senate elections. Challengers who held elected office had a considerable advantage over those who did not. For example, a challenger who had been elected governor could expect to receive an additional 5.8% of the vote compared with a challenger who had never been elected to public office, all other factors held constant.

The elected office held most often by Senate challengers was U.S. representative. Between 1974 and 1986, 36 current or former members of the House of Representatives sought a Senate seat by challenging an incumbent. Based on the estimated coefficient for the political experience index, membership in the House of Representatives was worth 3.9% of the vote to a Senate challenger. Sixteen of the challengers who had served in the House, or 44%, were successful.

Challengers who were celebrities also enjoyed a decided advantage in Senate elections. In fact, being a celebrity was worth almost as much—an additional 5.7% of the vote—as being the governor of a state. Three of the five celebrity challengers who ran for the Senate between 1974 and 1986 were successful. It appears from these results that public prominence is just as valuable to a Senate challenger

as political experience.

Perhaps the most interesting results in Table 1 involve the effects of campaign spending by incumbents and challengers. Although both coefficients are statistically significant, the estimated coefficient for challenger spending is almost three times as large as the estimated coefficient for incumbent spending. In fact, challenger spending was easily the most important single factor influencing the outcomes of Senate elections.

Since campaign spending was measured by the natural logarithm of total spending adjusted for inflation and state population, it is difficult to interpret the coefficients for the incumbent and challenger spending variables directly. The value of the challenger spending variable ranged from 0 to 5.4 with a mean of 2.7. Thus an average challenger increased his or her share of the vote by 10.0 percentage points as a result of campaign expenditures. The value of the incumbent spending variable ranged from 0 to 6.0 with a mean of 3.7. The average incumbent increased his or her share of the vote by 4.8 percentage points as a result of campaign expenditures. Combining these estimates, the net impact of campaign spending in an average Senate race was to increase the challenger's share of the vote by 5.2 percentage points, even though the average incumbent outspent the average challenger by a substantial margin.

Although incumbents' campaign expenditures had much less impact on the outcomes of Senate elections than challengers' campaign expenditures, the ability of an incumbent to raise an enormous campaign war chest was a decisive factor in several close races. The all-time spending record for a Senate campaign was set by Jesse Helms of North Carolina in 1984. Faced with a strong challenge from Governor James Hunt, Helms spent almost 17 million dollars on his reelection campaign. Helms won the election with 52% of the vote. Based on our estimate of

**Table 2. OLS Estimates for Open Seat Model**

Independent Variable	<i>b</i>	Standard Error	Beta	Significance Level
State partisanship	.10	.09	.12	—
State ideology	.17	.14	.12	—
Campaign spending	4.62	1.26	.41	.001
Political experience	3.14	.77	.43	.001
National party competence	.19	.11	.18	.05
Midterm	-1.16	1.39	-.08	—
Constant	49.64	2.60		

the effect of incumbent spending, Helms's campaign expenditures increased his share of the vote by 7.8 percentage points.

#### Effects of National Political Conditions

National political conditions had a strong influence on the outcomes of Senate elections between 1974 and 1986. Ratings of the relative competence of the Democratic and Republican parties ranged from -9 in 1980 (the most pro-Republican score) to 21 in 1974 (the most pro-Democratic score). Based on the estimated coefficient for the national party competence variable, a Democratic Senate candidate in 1974 would have received 4.9% more of the vote than a Democratic Senate candidate in 1980, all other factors held constant.

Even after controlling for party competence evaluations, Senate candidates representing the president's party received an average of 1.9% less of the vote in midterm election years than in presidential election years. In the four midterm elections between 1974 and 1986 only 71% of Senate incumbents from the president's party were reelected; in the same elections 94% of incumbents from the opposing party were reelected.

#### Results for Open Seats

Almost one-fourth of all Senate contests between 1974 and 1986 did not involve an incumbent. The estimated coefficients for the open seat model are presented in Table 2. Because of the relatively small number of open seat contests included in this analysis, the coefficients should be interpreted cautiously. Although all of the estimated coefficients are in the expected direction, only three are statistically significant and the overall fit of the open seat model is not as good as that of the incumbent support model: the six independent variables account for 55% of the variance in the outcomes of open seat contests and the standard error of 7.4 percentage points for the open seat model is substantially greater than the standard error of 5.0 percentage points for the incumbent support model.

The results presented in Table 2 do indicate that the outcomes of Senate contests for open seats are overwhelmingly based on the backgrounds and resources of the candidates. The two independent variables that had by far the strongest effects on the results of these races were the relative political experience of the candidates and the relative campaign expenditures of the candidates. Other factors such as national political conditions and the parti-

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san and ideological makeup of the state's electorate were much less important.

### Campaign Spending and Senate Election Outcomes

In his study of campaign spending in House elections, Jacobson (1978) suggested that there could be a reciprocal relationship between campaign spending and election outcomes. Of course the outcome of an election cannot directly affect campaign spending. However, expectations of potential contributors regarding the outcome of an election could affect the candidates' fund raising and the same factors which influenced these expectations could affect the outcome of the election (p. 470). If this is the case and if the factors that influenced contributors' expectations were not included in the model, then estimates of the effects of campaign spending might be biased.

Since Jacobson could not directly control for all of the factors that might have influenced the expectations of campaign contributors and the outcomes of House elections, he tried to estimate a non-recursive model that would allow for reciprocal effects between campaign spending and election outcomes. The results did not significantly alter his previous conclusions regarding the relative effects of incumbent and challenger spending. However, there were several problems with the procedures used to estimate this nonrecursive model (Green and Krasno 1986). First, the instruments Jacobson used to obtain the first-stage estimates in the two-stage least squares analysis were only weakly related to challenger and incumbent spending. In addition, the challenger's political experience was used as one of the instruments in the equation predicting challenger spending even though this variable is itself an important determinant of election outcomes, thus violating the assumption that the in-

struments are uncorrelated with the error term.

Instead of attempting to estimate a non-recursive model to test for the possibility of reciprocal effects between campaign spending and the outcomes of Senate elections, I will attempt to control for the effect of contributors' expectations by including a measure of these expectations in the incumbent support and open seat models. Although the expectations of potential campaign contributors cannot be directly measured, there is a measure available that is probably a very good surrogate for contributor expectations: the election forecast published in the special pre-election issue of *Congressional Quarterly Weekly Report*.

Approximately one month before the date of an election, *Congressional Quarterly Weekly Report* publishes a special pre-election report that includes a forecast of every House and Senate race. Each contest is classified on a seven-point scale: the categories are *safe Democratic*, *Democrat favored*, *leans Democratic*, *no clear favorite*, *leans Republican*, *Republican favored*, and *safe Republican*. This forecast, based on information obtained from observers in each state, has been quite accurate. The simple correlation between the CQ forecast (coded on a scale ranging from minus three to three) and the Democratic percentage of the major party vote in Senate elections from 1974 through 1986 is .86.

The CQ forecast should provide a good measure of the expectations of sophisticated election observers such as campaign contributors. By adding this forecast to the incumbent support and open seat models we can control for the impact of expectations on campaign spending. Actually, this approach will almost certainly underestimate the effects of campaign spending since expectations about the candidates' electoral prospects are undoubtedly based in part on the candidates' campaign finances: a candidate who

**Table 3. OLS Estimates for Expanded Incumbent Support and Open Seat Models**

Independent Variable	<i>b</i>	Standard Error	Beta
<b>Incumbent Support Model</b>			
State partisanship	.13	.03	.21
State ideology	.13	.03	.23
Log of state size	-.61	.38	-.06
Scandal	-6.54	2.23	-.12
Controversy	-.61	1.44	-.02
Health	-5.43	2.84	-.08
Primary contest	-4.63	1.70	-.24
Primary margin	.07	.02	.27
Ideological distance	-.03	.02	-.07
Challenger experience	-1.01	.49	-.10
Celebrity challenger	-2.90	2.36	-.05
Log of incumbent spending	.94	.58	.08
Log of challenger spending	-2.68	.44	-.36
National party competence	.11	.04	.15
Midterm election	-1.53	.56	-.12
CQ forecast	2.37	.47	.32
<b>Open Seat Model</b>			
State partisanship	.04	.08	.05
State ideology	.02	.13	.01
Candidate experience	1.46	.78	.20
Candidate spending	2.90	1.17	.26
National party competence	.15	.10	.14
Midterm election	-1.81	1.21	-.13
CQ forecast	3.35	.83	.50

raises a substantial amount of money and is able to wage a strong campaign, thereby increasing his or her visibility and standing in the polls, will be perceived as having a better chance of winning than one who does not.

The estimates for the incumbent support and open seat models including the CQ election forecast are included in Table 3. Although the coefficients for the incumbent and challenger spending variables are reduced by about 25% when the forecast is included in the incumbent support model, both coefficients remain statistically significant. Furthermore, even with the CQ forecast in the model, the challenger's campaign spending remains the most important single factor influencing the outcomes of these elections.

The coefficient for the challenger spending variable in the expanded model is still about three times the size of the coefficient for the incumbent spending variable.

The results for the expanded open seat model are similar, although the CQ forecast emerges as the strongest predictor of election outcomes. The coefficient of the relative campaign spending variable is reduced by slightly over one-third in the expanded model, but it remains highly statistically significant.

These results indicate that while some factors excluded from my models may influence both contributors' expectations and the outcomes of Senate elections, this is probably not a major problem. The technique used here almost certainly overcontrols for the impact of expectations on

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campaign spending and therefore underestimates the impact of campaign spending on election outcomes. The most important conclusion about the effects of campaign spending remains secure: challenger spending has a much stronger influence on the outcomes of Senate elections than incumbent spending.

### Comparing Winners and Losers

The results obtained for the incumbent support model can be used to estimate the relative contributions of each of the independent variables in the model to the difference in electoral support between winning and losing incumbents. This involves calculating the difference between the mean scores of winning and losing incumbents on each variable and multiplying this difference by the estimated regression coefficient for each variable.

Between 1974 and 1986, there were 166 contested Senate races involving incumbents for which complete data were available; 129 of the incumbents (78%) were reelected while 37 (22%) were defeated. The average winning incumbent received 61.8% of the major party vote while the average losing incumbent received 46.2% of the major party vote, a difference of 15.6 percentage points.

Table 4 shows the estimated contribution of each independent variable to the difference in electoral support between winning and losing incumbents. The challenger's campaign spending is by far the most important factor differentiating winning from losing incumbents: almost 30% of the difference in support between winning and losing incumbents is directly attributable to the challenger's campaign spending.

Of the remaining independent variables, those involving characteristics of the incumbent made the largest contribution to the success or failure of incumbent senator's reelection campaigns. Defeated

incumbents were particularly harmed by intraparty opposition, scandals, and political controversies. Other major factors that contributed to incumbent defeats were politically experienced challengers, support for the opposition party among a state's voters, and unfavorable national political conditions.

### Conclusions

Based on the findings presented in this study, candidate characteristics had the strongest influence on the outcomes of recent Senate elections. In Senate contests involving an incumbent, the incumbent's record and the challenger's qualifications and financial resources were the most important factors influencing the outcome; in contests for open seats, the relative experience and financial resources of the candidates were the most important determinants of the outcome.

The challenger's campaign expenditures are the single most important variable affecting an incumbent senator's chance of being reelected. Although an incumbent faced with a well-financed challenger can usually respond by increasing his or her own campaign spending, the incumbent's spending has much less impact on the outcome of a Senate race than the challenger's spending.

Although the candidates' records, qualifications, and financial resources were the most important factors influencing the results of Senate elections, national political conditions also had a significant influence on election outcomes. The impact of national political conditions on the results of Senate elections was not simply due to strategic behavior on the part of candidates and contributors (Jacobson and Kernell 1983).

One of the most important trends in U.S. electoral politics in recent years has been a shift from party-centered to candidate-centered campaigns. This trend



has had at least two major consequences for Senate elections. First, two-party competition has spread to every region of the country. In Senate elections, no state can be considered safe for either party. Second, money is probably more important than ever, especially for challengers and candidates for open seats. The weakening of party loyalties in the electorate means that candidates must increasingly rely on personal appeal to gain votes. Incumbents can use the perquisites of office for this purpose; challengers have to spend money.

In addition to these conclusions about the determinants of Senate election outcomes, the findings underscore the importance of the informational context of electoral decision making. The quality and quantity of information available to voters regarding the characteristics, records, and positions of candidates for office determine what factors will be weighed in their decision-making process. Senate elections differ from House elections not simply because of differences between the Senate and House as institutions but also because of differences in the

**Table 4. Contributions of Variables to Difference in Support  
between Winning and Losing Incumbents**

Independent Variable	Net Impact (Winners - Losers) (%)
State political characteristics	
State partisanship	1.0
State ideology	0.5
State size	0.0
Subtotal	1.5
Incumbent characteristics	
Ideological distance	0.4
Scandal	0.7
Political controversy	0.4
Health	0.4
Primary contest margin	1.1
Subtotal	3.0
Challenger characteristics	
Challenger political experience	1.0
Celebrity challenger	0.4
Subtotal	1.4
Campaign spending	
Incumbent spending	-0.4
Challenger spending	4.6
Subtotal	4.2
National political conditions	
National party competence	0.7
Midterm	0.7
Subtotal	1.4
Total explained difference	11.5
Not explained	4.1
Total difference	15.6

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information available to voters about Senate and House candidates. Media coverage of senators and Senate campaigns and the ability of Senate challengers to use television advertising make it more difficult for Senate than for House incumbents to control the flow of information to constituents between or during campaigns. A competitive electoral environment depends upon the ability and willingness of challengers and the news media to provide voters with critical information about the performance of incumbents.

### Appendix: Measures of Independent Variables

The partisan and ideological orientations of the electorate in each state were measured by using data from CBS News-*New York Times* surveys conducted between 1974 and 1984 and compiled by Wright, Erickson, and McIver (1985). No data were available for Alaska or Hawaii. The partisan orientation of the electorate was measured by the difference between the percentage of Democrats in a state and the percentage of Republicans in a state; similarly, the ideological orientation of the electorate was measured by the difference between the percentage of liberals and the percentage of conservatives. The estimates for less populous states were based on relatively small samples. The estimated partisanship scores for two states—Wyoming and South Dakota—were out of line with both election results and voter registration statistics. We corrected the partisanship scores for these two states by using the average difference in party registration between 1974 and 1986. When the vote for the incumbent was the dependent variable, these measures of partisanship and ideology were transformed to correspond to the party affiliation of the incumbent senator.

The number of House districts in a state was used to measure state population.

The ideological distance between a senator and the average voter in a state was measured by comparing the liberalism or conservatism of the senator's voting record with the ideological orientation of the state's electorate. We measured the liberalism or conservatism of senators' voting records by combining ratings of the liberal Americans for Democratic Action (ADA) and the conservative Americans for Constitutional Action (ACA), using the formula  $L = \text{ADA rating} / (\text{ADA rating} + \text{ACA rating})$ , where  $L$  is a senator's liberalism score. We then subtracted the average liberalism score for all senators (52%) from each senator's score. Similarly, we computed a liberalism score for each state by taking the difference between a state's liberal-conservative score and the average score for all 48 states. Ideological distance was measured by taking the absolute value of the difference between the senator's adjusted liberalism score and the state's adjusted liberalism score.

Campaign spending was measured by adjusting each candidate's reported campaign expenditures for inflation and population size. We controlled for inflation by measuring campaign spending in constant 1974 dollars. The procedure used to control for differences in state population was more complicated. Each candidate's total spending in thousands of 1974 dollars was divided by the number of congressional districts in a state plus a constant. This constant was based on an estimate of the relative effects of fixed costs and population-dependent costs on total campaign spending in Senate elections.

In order to estimate the size of the constant in the population adjustment formula I performed a regression analysis with campaign spending (in 1974 dollars) as the dependent variable and the number of House districts in a state as the independent variable. Assuming that on average the level of campaign spending across different states reflects differences

in the costs of campaigning, the size of the constant relative to the slope of the regression line should reflect the relative size of fixed and population-dependent costs in Senate campaigns. We obtained the following results: for incumbent spending, expenditures = 441,000 + (36,200 × districts); and for challenger spending, expenditures = 280,000 + (29,500 × districts). Thus, for both incumbents and challengers, the constant was approximately 10 times the size of the slope. I therefore used the number 10 as the constant in my population adjustment formula.

Scandals, political controversies, and concerns about the incumbent's health or physical conditions were measured by using dummy variables. The presence or absence of each of these issues was coded on the basis of information contained in

the special pre-election issue of *Congressional Quarterly Weekly Report*. An incident was coded as a scandal if there were allegations in the media of illegal activities involving the incumbent; political controversies involved incidents that were reported by the media and raised questions about the incumbent's honesty, judgment, or competence; health concerns were coded as an issue in the campaign if the incumbent's physical condition was mentioned as a potential political problem in the pre-election report. A brief description of each of these incidents is presented in Table A-1.

In order to measure the extent of intra-party opposition to the incumbent I used a dummy variable to indicate whether or not the incumbent had a primary contest and a second variable which measured the incumbent's percentage

**Table A-1. Special Conditions**

Year	State	Party	Candidate	Type	Description
1974	Colorado	R	Dominick	H	difficulty walking due to back injury
1974	Colorado	R	Dominick	C	handling of contributions from milk producers while chairman of Republican Senate Campaign Committee
1974	Kansas	R	Dole	C	outspoken support for President Nixon as chairman of Republican National Committee
1974	South Dakota	D	McGovern	C	disastrous presidential campaign
1976	Indiana	D	Hartke	C	excessive foreign travel
1976	Maryland	R	Beall	C	secret \$250 thousand campaign contribution from White House in 1970
1976	Nevada	D	Cannon	C	gifts and campaign contributions from corporations concerned with sub-committee
1976	New Mexico	D	Montoya	S	use of staff to manage business properties; space leased to post office in shopping center owned by senator; tax audit blocked
1978	Massachusetts	R	Brooke	S	lied about financial worth in divorce proceedings; mother received \$72 thousand in fraudulent medicaid payments
1978	Michigan	R	Griffin	C	announced retirement from Senate, then changed his mind

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Table A-1 (continued)

Year	State	Party	Candidate	Type	Description
1978	West Virginia	D	Randolph	C	indecisiveness on Panama Canal Treaty vote
1980	Arizona	R	Goldwater	H	hip ailment, difficulty walking
1980	Georgia	D	Talmadge	S	financial misconduct and alcoholism
1980	Washington	D	Magnuson	H	difficulty walking and inability to campaign energetically
1984	Iowa	R	Jepsen	S	membership in "health spa" used for prostitution
1984	Oregon	R	Hatfield	C	wife received \$55 thousand for real estate work for Greek financier backing African oil pipeline supported by senator
1986	Idaho	R	Symms	C	personal eccentricities; alleged drunkenness on Senate floor
1986	New York	R	D'Amato	C	contributions from brokerage firms while chairman of securities sub-committee
1986	North Dakota	R	Andrews	C	use of private detectives to investigate opponents; controversial medical malpractice lawsuit
1986	South Dakota	R	Abdnor	C	"misstatements" regarding farm problems
1986	Washington	R	Gorton	C	vote trading on judicial appointments
1986	Wisconsin	R	Kasten	S	arrest for drunken driving; association with fraudulent real estate deal; failure to file income tax returns

Note: C = controversy; H = health problems; S = scandal.

margin of victory over his closest primary challenger. If the incumbent did not have a primary contest, the victory margin was scored as zero. Margin of victory was used rather than the incumbent's share of the primary vote because it is not as sensitive to the number of candidates running in the primary.

The challenger's political experience was measured by an index ranging from zero to three. A score of zero was assigned to a challenger who had never held elected office; a score of one was assigned to a challenger who had been elected to the state legislature or to a local office representing fewer than 100 thousand people; a score of two was assigned to a challenger

who had been elected to a local office representing over 100 thousand people, a statewide office other than governor, or the House of Representatives; a score of three was assigned to a challenger who had been elected governor of the state.

A challenger was classified as a celebrity if he was a prominent public figure because of his activities before seeking elected office. Brief descriptions of these challengers and their activities are included in Table A-2.

National party competence scores for each election year were taken from the results of the last Gallup poll before the election that asked respondents to compare the abilities of the two major parties

Table A-2. Celebrity Challengers

Year	State	Party	Candidate	Previous Activities
1974	South Dakota	R	Thorsness	Vietnam POW
1976	California	R	Hayakawa	President of San Francisco State University involved in confrontation with student demonstrators
1976	New Mexico	R	Schmitt	astronaut
1976	New York	D	Moynihan	White House advisor, ambassador to India, UN ambassador
1984	Michigan	R	Lousma	astronaut

to handle the most important problem facing the country. A score was obtained by taking the difference between the percentage of respondents favoring the Democratic party and the percentage of respondents favoring the Republican party. Where the vote for the incumbent was the dependent variable, this score was recoded to correspond to the party affiliation of the incumbent senator.

The midterm election variable was coded as one if a Senate contest took place during a midterm election with a Democratic president, minus one if it took place during a midterm election with a Republican president, and zero if it took place in a presidential election year. When the vote for the incumbent was the dependent variable, the midterm election variable was recoded to correspond to the party affiliation of the incumbent Senator.

## Notes

An earlier version of this paper was presented at the 1987 annual meeting of the American Political Science Association. Jeffrey Segal played a major role in the development of the ideas underlying this paper. I would also like to thank Albert Cover and Ronald Rapoport for their many helpful comments and suggestions.

1. Kenney and Rice (1984) found that the relative divisiveness of Democratic and Republican Senate primaries affected the outcome of the general election. They did not distinguish between primaries in the incumbent's and challenger's parties. I would not expect the divisiveness of the challenger's pri-

mary to have the same impact on the general election as the divisiveness of the incumbent's primary. A closely contested primary in the challenger's party could reflect either the absence of a strong contender or a strong field of candidates attracted by the perceived vulnerability of the incumbent. Moreover, I would not expect intraparty divisions in the challenger's party to be as salient as those involving an incumbent. When the challenger's margin of victory in the primary was included in a multiple regression analysis, it had a negligible impact on the outcome of the general election.

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## ELECTIONS, COALITIONS, AND LEGISLATIVE OUTCOMES

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*Predictions of electoral behavior in a multiparty setting should be a function of the voters' beliefs about how parties will perform following an election. Similarly, party behavior in a legislature should be a function of electoral promises and rewards. We develop a multistage game-theoretic model of three-party competition under proportional representation. The final policy outcome of the game is generated by a noncooperative bargaining game between the parties in the elected legislature. This game is essentially defined by the vote shares each party receives in the general election, and the parties' electoral policy positions. At the electoral stage parties and voters are strategic in that they take account of the legislative implications of any electoral outcome. We solve for equilibrium electoral positions by the parties and final policy outcomes.*

Spatial theories of elections and legislatures are now well established if not thoroughly worked out (for recent reviews, see Austen-Smith 1983; Calvert 1986; Shepsle 1986). For the most part, theories of elections and theories of legislatures have developed independently of one another. This is unfortunate because, inter alia, voters are interested in policy outcomes, not policy promises. And policy outcomes are determined within an elected legislature that typically comprises representatives of several districts or political parties. Rational voters, therefore, will take into account the subsequent legislative game in making their decisions at the electoral stage of the process. In turn, rational candidates will take account of such deliberations in selecting their electoral strategy and subsequent legislative behavior conditional on electoral success. So to understand more fully both electoral and legislative behavior—in the sense of being able to explain and predict policy positions, policy outcomes, and coalition structures

—it is necessary to develop a theory of both political arenas simultaneously.

We analyze a sequential model of electoral and legislative decision making in a three-party proportional representation (PR) system. Following an election, parties attempt to form a governing coalition, which subsequently chooses a final policy outcome. The procedure by which a government is formed constitutes a non-cooperative bargaining game among the parties. This structure generates a unique policy prediction given the electoral platforms and vote shares of the represented parties. Thus upon observing the platforms selected by the parties, voters can deduce the policy consequences of any distribution of votes. This implies that voters are able to determine how best to cast their votes. And this in turn allows the parties to condition their choice of platform on the voters' optimal responses and therefore on the ultimate legislative implications.

The rationale for starting with the assumption of PR is twofold: first, it

allows us to examine coalition formation in legislatures using the party as the unit of analysis; second, the discreteness problem induced by any plurality system is absent—that is, under PR, the number of legislative seats won by a party may be treated as essentially proportional to the number of votes that party attracts. And in addition to PR being an important electoral mechanism in most of western continental Europe, there is renewed analytical interest in PR and other alternative rules to simple majority or plurality voting (e.g. Greenberg and Shepsle 1987; Greenberg and Weber 1985; Sugden 1984). But to our knowledge, in all of this work the voters are assumed to vote over *candidates* and not over *final policies*. As the remarks above suggest, this is a misspecification of the choice set. Although candidate characteristics other than policy positions surely matter in elections (Enelow and Hinich 1984), it is inappropriate to ignore the *legislative* implications of electing one candidate over another, as is explicitly done in the sources cited above. Likewise, while there are strategic models of legislative coalition formation and bargaining (e.g. McKelvey, Ordeshook, and Winer 1978; Riker 1962; Schofield 1985), none of these explicitly considers the *electoral* implications of the legislative behavior studied.

The notion that a political model should involve both electoral and legislative stage is of course not new. Downs (1957) examines a fairly informal model in which a legislature is formed via PR, and then simple majority voting within the legislature determines the government. His conclusions are imprecise and vague; and his focus is on showing how any voter's decision calculus is made more difficult when, at the electoral stage, the voter is ignorant of the eventual coalition structure in the legislature. Robertson (1976) analyzes a multidistrict model in which the party whose candidates win the most districts controls the legislature.

Thus it is evidently not sensible for a party to maximize votes; what matters is obtaining a controlling number of seats in the legislature. With only two parties and simple majority voting in the legislature, there is no room for postelection coalition-building. Recognizing this, rational voters vote on the basis of party policy, rather than candidate policy. Still, Robertson does not exploit a rigorous strategic model and his conclusions are correspondingly "broad-brush."

Austen-Smith (1981, 1984, 1986) develops a sequence of multidistrict models in which simple plurality voting in districts generates a legislature and voters vote on the basis of legislative policy outcomes. In Austen-Smith 1981 there are several "Downsian" parties, where all party candidates coordinate their policy positions so as to win control of the legislature; but the issue of coalition formation at the legislative stage is ignored. In Austen-Smith 1984 there are only two parties, all candidates belong to one or the other party, and candidates are free to adopt any policy they wish. Thus the analysis focuses on the mechanism that aggregates candidates' electoral positions into party positions at the legislative stage. Finally, in Austen-Smith 1986 it is assumed that all candidates in a multidistrict, simple plurality election are completely independent. In this case, rational voters at the electoral stage will form estimates of (1) which legislature will form, given any list of candidate positions; (2) which coalition will form, given a legislature; and (3) what will be the policy that is implemented, given a coalition. The legislative stage is not formulated explicitly; rather, each component is treated in an essentially probabilistic fashion.

In contrast, the model developed here provides a structure for solving for the policy outcome from the formation of a given legislature, by positing an institution in which the parties attempt to form a government. The typical approach to pre-



dicting the formation of coalitions and policy outcomes has been with the theory on cooperative games (cf. McKelvey, Ordeshook, and Winer 1978 and Schofield 1985, *inter alia*). This approach avoids identifying which of the possible winning coalitions form and instead generates families of coalition-payoff combinations that satisfy certain stability properties. Since our goal is to allow the parties and voters to "look ahead" to the future consequences of current actions, we prefer instead to adopt an approach that might give unique behavioral predictions at the legislative stage as a function of the results from the electoral stage. Hence we adopt a noncooperative approach to coalition formation that, given the generic nonexistence of the core, necessitates the imposition of some exogenous institutional structure to the problem. Thus we are trading off generality in favor of analytic tractability; this is in the spirit of recent work that examines outcomes as a function of institutions as well as preferences (e.g. Ferejohn and Krehbiel 1987; Shepsle 1979).

The particular institutional feature we have in mind is the widespread convention of first asking the party with the largest number of votes to attempt to form a winning coalition, that is, a government (Inter-Parliamentary Union 1986, *tbl.* 39). If this party is unsuccessful, the party with the next largest number of votes is allowed to try to form a government, and so on. In the event that no government is able to form, a "caretaker" government forms that is assumed to make the choice of legislative outcomes "equitably."

Once the general election results are determined, the mechanism described generates a noncooperative bargaining game in the legislature. Given parties' electoral platforms, this game has a unique equilibrium outcome for any distribution of electoral votes across parties. An outcome in the model is a winning coalition, a legislative policy position

implemented by that coalition, and a distribution of portfolios across the coalition. Because only one policy can be implemented and parties have different "preferences" over what it should be, coalition governments are sustained partly through sharing the benefits of being a member of the government. These benefits are modeled here as portfolios, and a party can be induced to join a government and compromise over the policy choice by offering it some portfolios. Thus parties have an incentive to join a government other than policy implementation alone. And, as we shall see, this incentive is important in supporting equilibrium policy positions.

Voters in the model care about final policy outcomes and not about party platforms *per se* or about the distribution of portfolios in any resulting government. Voters are also presumed to be rational. Given a list of electoral platforms of parties and given the structure of the legislative bargaining game, voters can compute the final legislative policy decision as a function of the distribution of electoral votes. Therefore, given everyone else's voting behavior, each individual will cast his or her vote to promote the final policy outcome he or she most prefers. In a two-party, simple plurality election, this amounts to voting sincerely over the party platforms. In a multiparty election with proportional representation, in which individuals cast at most one vote, sincere voting is typically not rational (Austen-Smith 1987). Furthermore, such voting behavior effectively eliminates any stable set of party policy positions; strategic (rational) voting here is necessary to support electoral equilibria.

In the next section we present the model of electoral and legislative behavior formally. We then provide a characterization of a class of equilibria generated by the induced multistage game. There are three features of the equilibria identified worth anticipating:

1. Given the electoral policy positions, only the rank order of the electoral vote shares matters in determining the winning coalition (government) that emerges from the legislative bargaining process. Assuming no one party receives an overall majority of votes, the government will comprise the largest and smallest legislative parties; the middle-ranked party in terms of votes will be excluded. Thus a party's influence in the legislature is not monotonic in its vote share, and the winning coalition may not be of minimum size (in the sense of Riker 1962) or connected (in the sense of Axelrod 1970).
2. The equilibrium electoral policy positions of the parties are symmetrically distributed around the median voter's most-preferred policy, with one party adopting this position to contest the election. This party, however, receives the fewest votes. As a result, the *expected* legislative policy outcome (i.e., prior to the legislative process being completed) is the median position; but the *actual* outcome in any election period will be skewed away from this point.
3. Not all individuals vote sincerely relative to the party positions in equilibrium. Therefore, the equilibrium party vote shares will not necessarily reflect the distribution of preferences of the electorate. Furthermore, as the minimum number of votes necessary for a party to be elected to the legislature goes down, the number of individuals not voting sincerely goes up.

Advocates of proportional representation often predicate their arguments upon two premises. First, the composition of the legislature will mirror the preference distribution of the population at large; second, legislative outcomes will reflect the relative weights of the elected parties in the legislature (see Sugden 1984, 31-33 for a discussion). The results reported

here suggest that neither of these premises may be well founded. We will take up these issues at greater length later.

## The Model

There are three parties,  $\alpha$ ,  $\beta$ , and  $\gamma$ , where  $\Omega = \{\alpha, \beta, \gamma\}$ , competing on a one-dimensional policy space  $P \subset R$ , for the votes from a finite set  $N$  of individuals. Assume  $|P| < \infty$ , and  $|N| \equiv n$  is sufficiently large ( $\geq 15$ ) and odd. Let  $S(\Omega)$  denote the set of all subsets of  $\Omega$ . At time  $t = -2$  the parties simultaneously announce policy positions  $p_k$  in  $P$ , where  $p = (p_\alpha, p_\beta, p_\gamma)$ ; and at  $t = -1$  the voters each cast a single ballot for one of the three parties. The method of determining a legislature is by proportional representation, where a party needs at least  $s$  votes to gain entrance to the legislature. We assume that  $s$  is odd and  $s \in [3, 1/3 \cdot n]$ . Let  $w_k$  be the proportion of votes party  $k$  receives in the election at  $t = -1$ , and let  $w = (w_\alpha, w_\beta, w_\gamma)$ . If one or more parties receives less than  $s$  votes, we normalize the weights of the remaining parties so that they sum to 1. For example, if only party  $\gamma$  gets less than  $s$  votes, then  $w'_\alpha = w_\alpha / (w_\alpha + w_\beta)$ . For what follows we assume that all parties receive at least  $s$  votes, so that we use the vector  $w$  rather than  $w'$  in establishing legislative influence.

For all coalitions  $C \in S(\Omega)$  let

$$w_C = \sum_{k \in C} w_k$$

and define  $D(w) = \{C \in S(\Omega) : w_C > 1/2\}$ . We assume that  $D(w)$  identifies the set of winning coalitions in the legislature given the vector of seats  $w$ . Also, for all  $k \in \Omega$  let  $D_k(w) = \{C \in D(w) : k \in C\}$  be the set of winning coalitions of which party  $k$  is a member.

From  $t = 1$  on, the parties attempt to form a *government*, or a winning coalition, that will collectively choose (1) a

policy  $y \in P$  and (2) a distribution of portfolios among the parties, which we characterize as choosing a distribution of a fixed amount  $G$  of transferable benefits across the parties; let  $\Delta(G) = \{(g_\alpha, g_\beta, g_\gamma) : g_k \geq 0, \forall k \in \Omega \text{ and}$

$$\sum_{k \in \Omega} g_k = G\}$$

be the set of such distributions. The process by which a government is formed is as follows: At  $t = 1$ , the party with the largest number of seats proposes a coalition  $C_1 \in D(w)$ , a policy  $y_1 \in P$ , and a distribution of benefits  $g_1 \in \Delta(G)$ , where  $g_1 = (g_{1\alpha}, g_{1\beta}, g_{1\gamma})$ . The members of the coalition either accept or reject the proposal. If the parties that accept the proposal constitute a winning coalition, then they form a government, implement  $y_1$ , and distribute  $g_1$ . If not enough parties accept the proposal, then at time  $t = 2$  the party with the second highest number of seats proposes a coalition, a policy, and a distribution of benefits; and again the members of the proposed coalition either accept or reject. If a government has not formed after the  $t = 3$  proposal, then a "caretaker" government is implemented which "equitably" makes the policy and benefits decisions.

Given this description, then, a strategy for party  $k$  consists of three elements: an electoral position  $p_k \in P$ , a proposal  $\Gamma_k \in D(w) \times P \times \Delta(G)$ , and a response strategy  $r_k : D_k(w) \times P \times \Delta(G) \times T \rightarrow \{0,1\}$  specifying whether or not party  $k$  accepts (1) or rejects (0) a proposal that includes  $k$  in the coalition, where this response may be a function of the time [ $t = 1,2,3$ ] at which it is offered. Note that our definition of a proposal is *ahistorical*; while a complete description of a strategy would imply the proposal being a function of past electoral positions, proposals, and responses, the nature of the model eliminates the necessity of carrying around this extra notation. Let  $\Gamma = (\Gamma_\alpha, \Gamma_\beta, \Gamma_\gamma)$ , and  $r = (r_\alpha, r_\beta, r_\gamma)$ . A strategy

for voter  $i$  is a function  $\sigma_i : P \times P \times P \rightarrow \Delta(\Omega)$  specifying the probability that  $i$  votes for each party given their electoral positions. Let  $\sigma_i(p) = (\sigma_i(\alpha), \sigma_i(\beta), \sigma_i(\gamma))$ , where  $\sigma_i(k)$  is the probability that voter  $i$  votes for party  $k$  and  $\sigma(p) = (\sigma_1(p), \dots, \sigma_n(p))$ .

Voters are assumed to be purely policy oriented, with preferences characterized by quadratic utility functions  $u_i(\bullet) = u(\bullet; x_i)$  over the policy space  $P$ , where  $x_i$  is voter  $i$ 's ideal point in  $P$ . It is assumed that  $x = (x_1, \dots, x_n)$  is common knowledge and ordered so that  $\forall i < n, x_i < x_{i+1}$ . Further, assume that voter ideal points are distributed symmetrically about the median voter's ideal point. Let  $\mu = (n + 1)/2$  be the median voter. The assumption of quadratic preferences implies that if the policy outcome from the legislative stage is uncertain but there exists a probability distribution  $q(\bullet)$  over  $P$ , then the *expected utility* for voter  $i$  is

$$E_q[u_i(\bullet)] = -(y^q - x_i)^2 - s^q,$$

where  $y^q$  is the mean and  $s^q$  is the variance of the distribution  $q$ .

Parties will have utility functions defined over  $\Delta(G)$  as well as, at the *legislative* stage, over  $P$ . However, their *ex ante* policy preferences will be a function only of the *difference* between their electoral positions and the final policy outcome. The motivation for this is as follows: Voters and parties are actually engaged in a continuing relationship that spans a number of elections. Voters therefore have the ability to condition future decisions on the past performance of the parties; in particular, they can condition their votes on the degree to which party promises (i.e., electoral positions) differ from the actual policy outcomes as a way of imposing costs on the parties at the legislative stage for deviating from their announced positions. Even if the parties are only concerned with winning elections and collecting the transferable benefits, *future* benefits will be a function of the

current difference between the electoral position of a party and the final policy outcome from the legislature if the voters adopt these "retrospective" strategies. (These strategies are justified as equilibrium behavior in a model of two-candidate competition with repeated elections in Austen-Smith and Banks 1987.) Hence rational parties will take this difference into account when choosing electoral positions and legislative proposals. Since it is in the interest of the voters to adopt these strategies, it seems consistent, in a "single-election" model, to characterize party preferences as we do.

This assumption also implies that if a party receives less than  $s$  votes and hence is not represented in the legislature, its payoff would *not* be a function of the final policy outcome. Thus we assume that party preferences are represented by a utility function taking on the values  $U_k(y, g; p)$  if elected and  $-c$  otherwise, where later we assume that the "cost"  $c$  is sufficiently large. The function  $U_k$  is assumed to be quasi-linear, that is, additively separable and linear in  $g_k$  and quadratic in  $y$ :  $U_k(y, g; p) = g_k - (y - p_k)^2$ , where  $p_k$  is the electoral position of party  $k$ . Again, use of quadratic utility functions implies that the *expected utility* for party  $k$  generated by the distribution  $q(\cdot)$  over  $P$  and  $f(\cdot)$  over  $\Delta(G)$  is

$$E_{f,q}[U_k(\cdot, \cdot; p)] = g_k^f - (y^e - p_k)^2 - s^e,$$

where  $y^e$  and  $s^e$  are defined as above and  $g_k^f$  is the mean value of  $g_k$  with respect to the distribution  $f(\cdot)$ .

A sequential equilibrium to this game will consist of voter and party strategies that are optimal for each participant at every point in time, given the assumed equilibrium behavior of the others. To characterize these equilibria we first determine the equilibrium behavior at the legislative stage for any vectors  $p$  of policy positions and  $w$  of party weights. By the

sequential nature of the decision making at the legislative stage, we initially solve for the optimal proposals and responses at time  $t = 3$ . This then allows us to solve for the optimal behavior at  $t = 2$  as a function of the optimal behavior to occur at  $t = 3$ , and so on. As we shall see, the equilibrium prediction for the legislative stage will in general be unique for any  $(p, w)$ . Therefore, for any  $p$  and set of voting strategies  $\sigma(p)$ , voters can deduce the final legislative outcome. This allows us to analyze equilibrium behavior at the voting stage, for all party positions  $p$ , by solving for the optimal behavior of the voters. In equilibrium, the vector of party weights  $w$  will be a known function of the party positions  $p$ , where this functional relationship will be determined by the voting strategies  $\sigma$ . This then constitutes the basis for analyzing the competition among the parties at the electoral stage as well, since now the legislative outcome is a function only of the positions the parties choose.

We shall initially describe the equilibrium behavior at the legislative stage and then work back to the voting and electoral stages.

## Equilibrium Behavior

### Equilibrium Legislative Outcomes

As described above, in this section the vector of party policies  $p = (p_\alpha, p_\beta, p_\gamma)$  and weights  $w = (w_\alpha, w_\beta, w_\gamma)$  are treated parametrically. It will be convenient to relabel the parties according to their relative positions on the policy space  $P$ . Let  $p_L = \min\{p_\alpha, p_\beta, p_\gamma\}$ ,  $p_M = \text{mid}\{p_\alpha, p_\beta, p_\gamma\}$ ,  $p_R = \max\{p_\alpha, p_\beta, p_\gamma\}$ ; similarly define  $w_L$ ,  $w_M$ , and  $w_R$  as the weights of the *left*, *middle*, and *right* parties, respectively, and let  $\Omega = \{L, M, R\}$ . If the weights of any two parties are equal while the remaining party has less weight, then it is assumed that prior to  $t = 1$  a fair coin is flipped to decide which party will make

## Legislative Outcomes

the  $t = 1$  proposal; a similar assumption holds when the two parties have the lowest weight or when all parties have equal weight. Most of the following analysis will focus on the case where each party has a distinct electoral position; however, the outcomes when some of the positions coincide will be easily derived from our proposition 1.

If only two parties receive the necessary  $s$  votes, then, as discussed in the account of the model, the weights of the parties in the legislature are normalized to reflect this fact. Thus the party with the higher vote share will hold a majority of the seats in the legislature. If the parties have the same vote shares, then it is assumed that the coin flip determines who will hold the majority in the legislature. In what follows we assume that all three parties receive at least  $s$  votes; given the above description of events, the subsequent analysis is easily extended to the case where only two parties are represented.

We assume that  $G$  is sufficiently large ( $\geq |P|^2$ ) that it would always be possible for a coalition to form at any time and sufficiently large that any caretaker government would have the ability to, and in fact would, choose a policy  $y \in P$  and a distribution  $g \in \Delta(G)$  such that the utilities for the parties would all be equal to zero in the event of no agreement at  $t = 1, 2$ , or  $3$ . Note that we could have equivalently assumed that there existed a positive coefficient on the linear term in the parties' utility functions and assume that this coefficient, rather than  $G$ , were sufficiently large.

If any party has a majority of the seats, say, for example, party  $L$ , it is clear that the only equilibrium is for that party to choose  $y_1 = p_L$  and  $g_{1L} = G$ , since it needs no other party to form a government. Furthermore, by the assumption of complete information with regard to the payoffs of the parties, we need only consider minimum winning coalitions, so that all members of a proposed coalition must

agree to the proposal.

Suppose party  $k$  is attempting to form a government. In order to induce party  $j$  to agree to a proposal, party  $k$  must offer  $j$  at least as much as  $j$  could get by rejecting  $k$ 's proposal; that is, party  $j$ 's *opportunity cost* of joining the coalition. Let  $u_j^t$  be party  $j$ 's opportunity cost at time  $t$ . It is clear from the above assumptions that  $u_j^2 = u_M^3 = u_R^3 = 0$ , since by rejecting a proposal at  $t = 3$  each party guarantees a payoff of zero from the ensuing caretaker government. If a government would implement  $(y_3, g_3)$  at  $t = 3$ , then the opportunity cost for party  $k$  at  $t = 2$  would be determined by  $k$ 's utility from the outcome  $(y_3, g_3)$ ; that is,  $u_k^2 = U_k(y_3, g_3; p)$ , since this is the utility  $k$  would receive from rejecting a proposal at  $t = 2$ . Similarly, if  $(y_2, g_2)$  would be implemented at  $t = 2$ , then  $u_k^1 = U_k(y_2, g_2; p)$ .

In general the parties' opportunity costs will depend on the responses of the parties to subsequent proposals. Let

$$\delta(C, y, g, t) = \prod_{k \in C} r_k(C, y, g, t)$$

be the product of party responses to a proposal of  $(C, y, g)$  at time  $t$ ; thus  $\delta(C, y, g, t)$  will be one if all parties agree to the proposal and zero otherwise. Since only minimum winning coalitions will be proposed,  $\delta(C, y, g, t)$  is sufficient to deduce whether a government will form at  $t$ .

**DEFINITION.** Given proposals  $\Gamma$  and responses  $r$ , the opportunity cost of party  $k$  at time  $t$ ,  $u_k^t(\Gamma, r)$ , is

$$\begin{aligned} u_k^3(\Gamma, r) &= 0 \\ u_k^2(\Gamma, r) &= \delta(C_3, y_3, g_3, 3) \cdot U_k(y_3, g_3; p) \\ u_k^1(\Gamma, r) &= \delta(C_2, y_2, g_2, 2) \cdot U_k(y_2, g_2; p) \\ &\quad + (1 - \delta(C_2, y_2, g_2, 2)) \cdot u_k^2 \end{aligned}$$

Given a list  $\Gamma$  of proposals, then, we can inductively define equilibrium responses

for the parties. To determine the equilibrium proposals for the parties, define  $\tilde{U}_k(\Gamma_k, r) = U_k(y, g; p) \cdot \delta(C, y, g, k)$  as the utility for party  $k$  generated by the proposal  $\Gamma_k = (C, y, g)$  if  $\Gamma$  is accepted.

DEFINITION. A legislative equilibrium consists of response strategies  $r^*(\bullet) = (r_L^*(\bullet), r_M^*(\bullet), r_R^*(\bullet))$  and proposals  $\Gamma^* = (\Gamma_L^*, \Gamma_M^*, \Gamma_R^*)$  such that  $\forall t, \forall k \in \Omega$ ,

$$\forall \Gamma, r_k^*(C, y, g, t) = 1 \text{ if } U_k(y, g; p) \geq u_k^*(\Gamma, r^*) \text{ and } 0 \text{ else}$$

$$\Gamma_k^* \text{ maximizes } \tilde{U}_k(\Gamma_k, r^*)$$

The logic of this definition follows from the sequential nature of the actions: since  $r_k^*(\bullet, 3)$  is known for all  $k \in \Omega$ , the optimal proposal from the party with the lowest weight can be explicitly solved. This then generates  $r_k^*(\bullet, 2)$  so that the optimal proposal at  $t = 2$  can be solved, and so on.

The presence of perfect information guarantees that in equilibrium, if there exists a proposal at  $t = 3$  that gives the party with the lowest weight and some other party nonnegative utility, then the equilibrium proposal at  $t = 3$  will be accepted; similar logic holds for  $t = 2$  and  $t = 1$ . Furthermore, if parties  $k$  and  $j$  agree to form a coalition at  $t = k$ , it must be that the proposal  $(y_k, g_k)$  is such that  $y_k$  lies between  $p_k$  and  $p_j$  and  $g_{kk} + g_{kj} = G$ ; in words, the proposal must be Pareto-efficient for the coalition  $C = \{k, j\}$ . Also, if  $j$  accepts  $k$ 's proposal, then it must be that either  $j$  is receiving exactly its opportunity cost, or  $y_k = p_k$  and  $g_{kk} = G$ , since otherwise  $k$  could offer  $j$  less than  $g_{kj}$  or a policy closer to  $p_k$  and still gain  $j$ 's acceptance. The assumption that  $G$  is sufficiently large relative to  $|P|$  implies that in equilibrium the proposal by the party with the highest weight will be accepted at an outcome that either is "first-best" for that party or makes the joining party indifferent between accepting and rejecting the proposal. (This is typical of bargain-

ing models with perfect information; see Rubinstein 1982.) To determine who will join this party and at what outcome, then, we need to analyze the equilibrium proposals at  $t = 3$  and  $t = 2$  decisions to generate the opportunity costs of the parties at  $t = 1$ .

Suppose that  $w_L > w_M > w_R$ , so that party  $R$  has the lowest weight, and hence makes a proposal at  $t = 3$  if no government has yet formed. Given the form of the parties' utility functions, it is clear that party  $R$  will attempt to form a coalition with the party whose electoral position  $p_j$  is closest to  $p_R$ , since the opportunity costs of the other parties are equal. Thus, in this case,  $R$  would attempt to form the coalition  $\{M, R\}$ . Since  $R$  cannot implement  $y = p_R$  and  $g_{RR} = G$ ,  $R$  chooses  $(y_3, g_3)$  to solve

$$\begin{aligned} \max_{y, g} \quad & g_R - (y - p_R)^2 \\ & + \lambda(G - g_R - (y - p_M)^2), \\ & y \in P, g_R \in [0, G]. \end{aligned}$$

Since the utility functions are separable and quadratic in  $y$ , the solution to the above optimization will be

$$\begin{aligned} y_3^* &= \frac{p_R + p_M}{2} \equiv p_{RM} \\ g_{3R}^* &= G - (p_M - p_{RM})^2 \\ g_{3M}^* &= (p_M - p_{RM})^2 \end{aligned}$$

Thus if a government has not formed at  $t = 1, 2$ , then at  $t = 3$  the policy outcome of the government will be the midpoint between the electoral positions of  $R$  and  $M$ , while the benefits  $G$  will be distributed in such a way as to give  $M$  a utility of exactly zero, that is,  $M$ 's opportunity cost. (Note: this solution holds if we weaken the assumption on party preferences from quadratic to symmetric concave over policy.)

Notice that this logic is quite general: for any  $(p, w)$ , the equilibrium proposal at

$t = 3$  will be such that the policy  $y$  is the midpoint between the electoral positions of the party with the lowest weight and the party with the nearest electoral position.

At  $t = 2$ , then, the opportunity costs of the parties will be  $u_L^2 = -(p_{RM} - p_L)^2$ ,  $u_M^2 = 0$ , and  $u_R^2 = G - 2(p_R - p_{RM})^2$ . Thus party  $L$  will accept a proposal that is "first-best" for party  $M$ ,  $y = p_M$  and  $g_M = G$ , since this gives  $L$  utility of  $-(p_M - p_L)^2$ , which is greater than  $-(p_{RM} - p_L)^2$ .

At  $t = 1$ , then,  $u_L^1 = -(p_M - p_L)^2$ ,  $u_M^1 = G$ , and  $u_R^1 = -(p_R - p_M)^2$ , implying that at  $t = 1$  the coalition  $\{L, R\}$  will form, since party  $M$ 's opportunity cost at  $t = 1$  implies that  $L$  could never make a proposal that would keep  $M$  indifferent while making  $L$  better off but there do exist proposals that make both  $L$  and  $R$  better off. If  $d_L \equiv (p_M - p_L) \geq (p_R - p_M) \equiv d_R$ , then, as at  $t = 3$ , the optimal proposal from party  $L$  at  $t = 1$  will be such that  $y = p_{RL}$ , and  $R$  receives sufficient transferable benefits to meet its opportunity cost. If  $d_L < d_R$ , then the optimal proposal would be to choose  $y = p_M$  and  $g_L = G$ , since this gives  $R$  precisely his opportunity cost and no other proposal would make  $L$  better off without making  $R$  worse off. Thus if  $w_L > w_M > w_R$ , the equilibrium policy will either be  $p_M$  or  $p_{LR}$ , depending on the distances between the electoral positions.

Suppose instead that  $w_M > w_L > w_R$ . By the same logic as above, at  $t = 3$  the coalition  $\{M, R\}$  would form, with policy  $y_3 = p_{MR}$ , and benefits  $g_{3M} = (p_{MR} - p_M)^2$ ,  $g_{3R} = G - g_{3M}$ . At  $t = 2$ , then, the coalition  $\{L, M\}$  would form with some policy  $y_2 \in (p_L, p_M)$ , where again the exact policy will be a function of the distances between electoral positions. At  $t = 1$ , then, the opportunity cost of party  $R$ ,  $u_R^1$ , will be less than  $-(p_R - p_M)^2$ ; hence  $R$  will accept a proposal by party  $M$  of  $y_1 = p_M$  and  $g_{1M} = G$ . Thus the equilibrium policy outcome when  $w_M > w_L > w_R$  will be  $y = p_M$ , regardless of the distances be-

tween the electoral positions.

Note that the above analysis applies directly to the symmetric cases where  $w_R > w_M > w_L$  and  $w_M > w_R > w_L$ . The remaining cases, where party  $M$  has the lowest weight, can be analyzed similarly, although in these cases the algebra is somewhat trickier.

The following proposition summarizes the equilibrium coalitions  $C^*$  and outcomes  $y^*, g^*$  from the legislative stage. The (lengthy) formal statement of the proposition can be found in the Appendix.

**PROPOSITION 1.** *Let party  $k$  offer the proposal at  $t = 1$ , party  $h$  at  $t = 2$ , and party  $j$  at  $t = 3$ . (1) If  $k$  has a majority in the legislature, then  $y^* = p_k$ ,  $g_k^* = G$ ; (2) If  $k$  does not have a majority, then  $C^* = \{k, j\}$ ,  $y^*$  lies between  $p_k$  and  $p_{kj}$ , and*

$$\max g_j^* = (p_k - p_{kj})^2 \text{ if } y^* = p_{kj} \\ \text{and } 0 \text{ else}$$

$$g_k^* = G - g_j^*, g_h^* = 0$$

Thus in equilibrium it will always be the parties with the highest and lowest weights that form the governing coalition. The logic of this follows directly from the recursive nature of the analysis: the party with the middle weight is excluded precisely because it would make the  $t = 2$  proposal, thus implying a high opportunity cost at  $t = 1$  and hence a degree of bargaining power vis-à-vis the party with the highest weight exceeding that of the party with the lowest weight. The non-cooperative bargaining model of coalition formation developed here generates a unique coalition prediction, where this coalition is minimum winning but is not of minimum size (Riker 1962) and is not necessarily connected (Axelrod 1970).

The cases in which the party with the lowest weight is in the middle and  $d_L = d_R$  yields a sequential equilibrium predic-

tion that is nonunique. The reason for this is that, at  $t = 3$ , party  $M$  is indifferent between forming a government with  $L$  or with  $R$ , since either will give  $M$  the same payoff. However, the equilibrium payoffs to  $M$  depend on exactly this choice at  $t = 3$ . In particular,  $M$  receives a higher payoff if it forms with the party with the highest weight than it would from forming with the party with the middle weight. The selection we make is the equilibrium with the higher payoff for party  $M$ , since  $M$  could ex ante credibly threaten the party with the highest weight that it would take such an (equilibrium) action at  $t = 3$  if it were called upon to do so.

It is worth remarking that the prediction in proposition 1 that the government will consist of the parties proposing first and last is not a consequence of the assumption of quasi-linear party preferences. As long as the preferences of the parties are increasing in portfolio benefits and single peaked in policy, this result will hold. The assumption of quasi-linear preferences gives us the ability to solve explicitly for what the ultimate policy and benefits will be, which in turn allows us to solve for the optimal voter and party behavior prior to the legislative stage.

### Equilibrium Voting Strategies

Let  $y(w, p)$  be the equilibrium policy outcome from the legislative stage given the vector of weights  $w$  and positions  $p$ . Define  $\Lambda(p) = \{y \in P: y = y(w, p) \text{ for some } w\}$  to be the set of possible equilibrium policy outcomes given  $p$ . The vector of weights  $w$  will be determined by the individual voting behavior; in particular, assuming that all voters adopt pure strategies, for any  $k \in \Omega$ ,

$$w_k = |\{i \in N: \sigma_i(k) = 1\}|/N \\ \equiv v_k(\sigma(p))/N$$

where  $\sigma(p) = (\sigma_1(p), \dots, \sigma_n(p))$ . Thus the probability of any specific policy  $y \in$

$\Lambda(p)$  being the final outcome is a function of voter strategies; let  $\pi(\cdot | \sigma, p): \Lambda(p) \rightarrow [0, 1]$  denote this probability.

DEFINITION. For any  $C \in S(\Omega)$ , with  $|C| \geq 2$  and any  $p \in P^3$ , voter  $i$ 's sincere strategy relative to  $C$ ,  $\sigma_i^C(\cdot)$ , is defined as

$$\sigma_i^C(k') = 1 \text{ if and only if } u_i(p_{k'}) \\ > \max_{C \setminus \{k'\}} u_i(p_k)$$

Notice that if  $|C| = 2$ , an individual who votes sincerely relative to  $C$  at  $p$  does not necessarily vote for the party offering his or her most preferred policy in  $\{p_L, p_M, p_R\}$ .

DEFINITION. A voting equilibrium is an  $n$ -tuple  $\sigma^*(p)$  such that  $\forall p, \forall i \in N, \forall \sigma_i(p)$ :

$$E_{\pi(\sigma^*, p)}[u_i(y)] \geq E_{\pi(\sigma_i, \sigma_{-i}^*, p)}[u_i(y)]$$

Thus, given  $p$ , a voting equilibrium is simply a Nash equilibrium to the game with players  $N$  and payoffs induced by the equilibrium behavior in the legislative game generated by  $p$ . For simple plurality, two-candidate electoral competition, Nash equilibrium is too weak a concept; it admits equilibria that are supported only by weakly dominated strategies. Consequently, in such games voter strategies are additionally required to be undominated. In the three-party proportional representation game developed here, however, no strategy is weakly dominated. The reason for this is that in contrast to the two-party case, the final outcome from the legislative game for any  $p$  is not monotonic in vote shares: given that no party has an overall majority, it is always the largest and smallest parties that form the government. Thus requiring voter strategies to be undominated is vacuous here, even when two of the three parties adopt identical platforms.

An example will illustrate this fact. Suppose  $n = 15$ ,  $s = 3$ , and  $x_i = i$  for all  $i$



$= 1, \dots, 15$ . Suppose also that  $p_L = p_M = 11 < p_R = 12$ . Evidently,  $u_1(p_M) > u_1(p_R)$ . Is voting for  $R$  a dominated strategy for 1? The answer is no. To see this, suppose individuals  $i = 2, \dots, 5$  vote for  $L$ , individuals  $i = 6, \dots, 11$  vote for  $M$ , and  $i = 12, \dots, 15$  vote for  $R$ . All these voters are voting sincerely relative to the positions  $p = (11, 11, 12)$ . Since  $s = 3$ , all parties get elected to the legislature in the absence of 1's vote. If 1 votes sincerely for either  $L$  or  $M$ , then no party has an overall majority; and the legislative weights of the parties are  $w_M > w_L > w_R$  if 1 votes for  $L$ , and  $w_M > w_L = w_R$  if 1 votes for  $M$ . By proposition 1 the final policy outcome from the legislative bargaining process will be  $(p_M + p_R)/2 = 11.5$  if 1 votes for  $L$ , and will be (in expectation)  $1/2 \cdot (p_M + p_R)/2 + 1/2 \cdot p_M = 11.25$  if 1 votes for  $M$ . Now assume 1 votes for party  $R$ . Then  $w_M > w_R > w_L$ , in which case the final policy outcome is surely  $p_M = 11$ . Hence, individual 1 is better off voting for  $R$  in this circumstance than he or she is by voting sincerely.

So eliminating voting equilibria involving dominated strategies buys us nothing. Consequently, we make a selection from the set of voting equilibria that is simple, supports an intuitively reasonable class of equilibrium party positions, and has two desirable properties. First, at any equilibrium set of party positions, every voter is decisive between at least two parties. Thus although we cannot apply the "weak dominance" argument for all electoral positions  $p$ , in equilibrium each voter will have a nontrivial decision problem in that the final policy outcome will be a function of how he or she votes. Second, at any out-of-equilibrium party positions, the voting equilibrium strategies provide incentives for the parties to "move toward" the equilibrium positions.

The voting equilibrium is described formally and in detail in the Appendix. For current purposes it is sufficient to identify the key features of the equilibrium in-

formally.

**PROPOSITION 2.** *A voting equilibrium  $\sigma^*(p)$  is well defined for all  $p \in P \times P \times P$ . It is such that at least one party is penalized (in terms of votes) if, relative to the distribution of voter preferences (1) any two parties are "too close," (2) no party is centrally located, or (3) parties are "too dispersed."*

Together, conditions (1), (2), and (3) ensure that in equilibrium parties will adopt distinct positions symmetrically distributed about the median of the voter distribution. Exactly what constitutes being "too close" or "too dispersed" will become clear once we analyze the parties' strategic choice of electoral policy platforms.

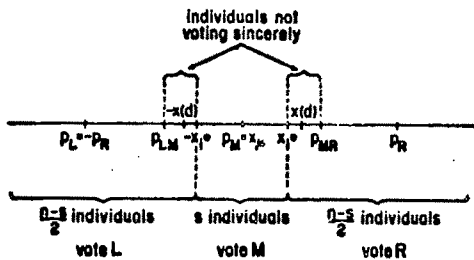
It is worth noting that in the voting equilibrium we select, no individual votes sincerely relative to  $\Omega$  for all  $p \in P \times P \times P$ . Moreover, as we remarked, if individuals are *constrained* always to vote sincerely, then there is no set of party positions that could be an equilibrium: given any set of party platforms  $p$ , there is invariably one party that can unilaterally improve its payoff by deviating from  $p$ . From a theoretical perspective, therefore, strategic behavior on the part of the voters is required to generate stable electoral outcomes. And there exists considerable empirical evidence for strategic voting in legislative elections (Riker 1982).

### Equilibrium Party Positions

We are now in a position to define the equilibrium path of the entire multistage game by analyzing the electoral game among the parties, where the payoffs are those induced by the equilibrium behavior of the voters at  $t = -1$  and the subsequent equilibrium behavior of the parties at the legislative stage. Let

$$\psi_k(p) = E_{\pi(\sigma^*, p)}[U_k(y^*(w(\sigma^*(p))), p), \\ g^*(w(\sigma^*(p)), p; p)]$$

Figure 1. Equilibrium Party Positions



be the (expected) indirect utility for party  $k$  from the electoral positions  $p$  given the equilibrium behavior at the voting stage  $\sigma^*(\cdot)$  and at the subsequent legislative stage  $y^*(\cdot)$ ,  $g^*(\cdot)$ .

**DEFINITION.** An electoral equilibrium is a triple  $p^* = (p_\alpha^*, p_\beta^*, p_\gamma^*)$  such that  $\forall k \in \Omega$ ,  $\forall p_k \in P$ ,  $\psi_k(p^*) \geq \psi_k(p_k, p_{-k}^*)$ . For any  $a \in \mathbb{R}$  define  $\text{int}[a]$  as the smallest integer greater than or equal to  $a$ .

**PROPOSITION 3.** Relative to the voting strategies of proposition 2,  $p^* \in P \times P \times P$  is an electoral equilibrium for any  $s \in [3, n/3]$ ,  $s$  odd, if and only if

- (1)  $p_M^* = x_\mu$
- (2)  $(p_M^* - p_L^*) = (p_R^* - p_M^*) \in [8/3 \cdot (x_{i^*} - x_\mu), 4 \cdot (x_{j^*} - x_\mu)]$ , where  $i^* \equiv \mu + (s-1)/2$ ,  $j^* \equiv \mu + \text{int}[(n-1)/4]$

Under the assumption of a symmetric distribution of voter ideal points, at any  $p^*$  in this class, the equilibrium vote shares  $w(\sigma^*(\cdot))$  are  $w_L = w_R > w_M$ , where party  $M$  receives exactly  $s$  votes. Consequently, from the analysis above, the equilibrium policy outcome from the legislative stage will be either  $p_{LM}^*$  or  $p_{MR}^*$ , with each of these occurring with probability  $1/2$  due to the equal weights of the extreme parties. Further, in none of the equilibria characterized in proposition 3 is it the case that a voter votes for the party whose platform is farthest from his or her ideal point. Note also that by the assump-

tion of quadratic utilities, all of the equilibria in this class are Pareto-inefficient; that is, everyone would ex ante prefer the outcome  $y^* = p_M$ ,  $g_L^* = (1/2) \cdot g = g_R^*$ , since this gives the same mean utility as all the equilibria but at zero variance. The equilibrium that is Pareto-efficient among the class of equilibria is where the extreme parties adopt the innermost positions defined in proposition 3, condition 2.

Figure 1 gives an example of equilibrium party positions along with the associated voting behavior of the electorate. Define  $x(d)$  as the point in  $P$  such that in an equilibrium where  $(p_R - p_M) = (p_M - p_L) = d$ , an individual with  $x(d)$  as an ideal point would be indifferent between voting for  $M$  and giving  $M$  precisely  $s$  votes, and voting for  $R$  and giving  $R$  a subsequent majority in the legislature. Thus  $x(d)$  solves

$$-(x - x_\mu)^2 - d^2/4 = -(d - (x - x_\mu))^2$$

Solving this, we find that  $x(d) = x_\mu + 3/8 \cdot d$ . Proposition 3, condition 2 then implies that  $M$  receives at least  $s$  votes, and  $x(d) \geq x_{i^*}$ ; otherwise voter  $i^*$  would prefer to vote for  $R$ , thereby upsetting the equilibrium. To see that  $M$  receives exactly  $s$  votes, suppose that some voter  $i > i^*$  were voting for  $M$  in an equilibrium described in proposition 3. Then, by switching its vote to  $R$ , party  $M$  still receives at least  $s$  votes, but now party  $R$  will surely make the first proposal in the legislature, thus implying that the policy outcome will be  $p_{RM}$  with probability 1. It is easy to see that this outcome would be preferred by  $i$  to the proposed equilibrium outcome.

## Discussion

From the perspective of positive political theory, little is known about the comparative properties of proportional representation and simple plurality decision-making schemes. What is known is large-

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ly confined to the abstract structures of various aggregate preference relations. For example, we can say how simple majority preference and the single transferable vote match up on desiderata such as "anonymity," "neutrality," or "independence of irrelevant alternatives"; but we have little idea about how strategic agents—candidates for office, voters, and so on—would behave differentially under these mechanisms or what the difference in the final policy outcomes might be in an otherwise fixed environment.

We developed a multistage game-theoretic model of three-party competition under proportional representation. The particular PR mechanism assumed has two parts. First, at the election stage, a fixed standard, or quota, rule determines the composition of the legislature. Second, in the elected legislature, a non-cooperative bargaining process determines the membership of the government, the distribution of portfolios across this membership, and the final policy outcome. The legislative bargaining process is defined both by the relative electoral vote shares of parties—the "weights" of the parties in the legislature—and by the policy platforms they adopt to contest the election. The identified equilibria to this game have three main substantive features:

1. The government consists of the parties with the highest and the lowest weights. Hence, the legislative influence of an elected party is not monotonic in its vote share.
2. Parties' electoral platforms are symmetrically distributed about the median voter's ideal point in the one-dimensional issue space. The party adopting this position to contest the election receives the smallest number of votes, and the remaining parties have an equal likelihood of being first-ranked in the legislature. Therefore, by feature 1, the expected final policy out-

come will be at the median voter's ideal point; but the realized final outcome will lie between the median and either the rightmost, or the leftmost, party's position.

3. Not all individuals vote sincerely (for the party platforms they most prefer). So even with equilibrium party platforms, vote shares will not reflect the true distribution of preferences of the electorate.

The comparison with the two-party, winner-take-all, electoral mechanism in this environment is straightforward. In this case

1. The party with the most votes has monopolistic control of the legislature. Legislative influence, therefore, is monotonic in votes.
2. In equilibrium, both parties adopt the median voter's position, and this is surely the final policy outcome.
3. All voters vote sincerely, whether the parties adopt the equilibrium policy platforms or not.

In sum, the popular conception that in contrast with simple plurality schemes, proportional representation leads to legislatures—and hence to final policy outcomes—that reflect the variety of interests in the electorate seems mistaken. Such a conception rests on the more or less implicit assumption of nonstrategic behavior by the voters and parties that, on both theoretical and empirical grounds, is unwarranted. This said, two caveats should be noted in regard to the model here.

First, the question of entry into the electoral competition is ignored. This is clearly important, since the number of candidates or parties contesting the election will be functionally dependent on the particular electoral and legislative schemes in place. However, allowing free entry, say, is not going to remove the incentives for strategic behavior by the voters in the

election; and the logic of the legislative bargaining process studied here is invariant to the number of parties in the legislature (although the location of the final policy outcome is, of course, sensitive to this number).

Second, the equilibrium location of the three parties' policy platforms depends on the specification of the (equilibrium) voting behavior for any set of platforms. If voting strategies are altered, the associated equilibrium party policies will be altered as well. Sincere voting by everyone is not capable of supporting any equilibrium in party positions, as it is in two-party competition; rather, strategic voting is essential to generate stable outcomes. Moreover, as we argued earlier, no voting strategy is weakly dominated. Suppose we fix party positions and fix some individual's ( $j$ 's) vote arbitrarily; then there exists a distribution of votes by others such that  $j$  voting otherwise makes him or her strictly worse off. Consequently, for any distribution of party platforms there is a multiplicity of undominated voting equilibria; and the selection of exactly which one to adopt in order to solve for the electoral equilibrium in party platforms is somewhat arbitrary. The criterion used here was to insist that in *any* electoral equilibrium, *every* voter must be pivotal, that is, capable of unilaterally altering the final policy outcome from the legislative bargaining process by affecting the rank order of parties' electoral vote shares. This is a non-trivial prerequisite that refines the set of admissible equilibria considerably. But it is clear that work needs to be done on this problem.

## Appendix

**PROPOSITION 1.** *The following constitute the legislative equilibrium coalitions and outcomes:*

1. If  $w_M = \max \{w_L, w_M, w_R\}$ , then  $C^*$

$= \{M, k\}$ , where  $w_k = \min \{w_L, w_M, w_R\}$ ,  $y^* = p_M$ ,  $g_M^* = G$ .

2. If  $w_L > w_M > w_R$ , then  $C^* = \{L, R\}$  and if

a.  $d_L \leq d_R$ , then  $y^* = p_M$ ,  $g_L^* = G$ .

b.  $d_L > d_R$ , then  $y^* = p_{LR}$ ,  $g_R^* = (p_{LR} - p_R)^2$ , and  $g_L^* = G - g_R^*$ .

3. If  $w_R > w_M > w_L$ , then  $C^* = \{R, L\}$  and if

a.  $d_L \leq d_R$ , then  $y^* = p_{LR}$ ,  $g_L^* = (p_{LR} - p_R)^2$ , and  $g_R^* = G - g_L^*$ .

b.  $d_L > d_R$ , then  $y^* = p_M$ ,  $g_R^* = G$ .

4. If  $w_L > w_R > w_M$  and  $d_L \leq d_R$ , then  $C^* = \{L, M\}$  and

a. if  $2d_L \geq d_R$ ,  $y^* = p_{LM}$ ,  $g_M^* = (p_{LM} - p_M)^2 - (p_{RL} - p_M)^2$ , and  $g_L^* = G - g_M^*$ .

b. if  $2d_L < d_R \leq 3d_L$ ,  $y^* = 2p_M - p_{RL}$ ,  $g_L^* = G$ .

c. if  $3d_L < d_R$ ,  $y^* = p_L$ ,  $g_L^* = G$ .

5. If  $w_L > w_R > w_M$  and  $d_L > d_R$ , then  $C^* = \{L, M\}$ ,  $y^* = p_{LM}$ ,  $g_M^* = (p_M - p_{LM})^2 - (p_{RM} - p_M)^2$ , and  $g_L^* = G - g_M^*$ .

6. If  $w_R > w_L > w_M$  and  $d_L < d_R$ , then  $C^* = \{R, M\}$ ,  $y^* = p_{MR}$ ,  $g_M^* = (p_{MR} - p_M)^2 - (p_M - p_{ML})^2$ , and  $g_R^* = G - g_M^*$ .

7. If  $w_R > w_L > w_M$  and  $d_L \geq d_R$ , then  $C^* = \{R, M\}$  and

a. If  $d_L \leq 2d_R$ ,  $y^* = p_{MR}$ ,  $g_M^* = (p_M - p_{RM})^2 - (p_M - p_{RL})^2$ , and  $g_R^* = G - g_M^*$ .

b. if  $2d_R < d_L \leq 3d_R$ ,  $y^* = 2p_M - p_{RL}$ ,  $g_R^* = G$ .

c. if  $d_L > 3d_R$ ,  $y^* = p_R$ ,  $g_R^* = G$ .

Next, we state and prove the formal version of proposition 2. For any  $p \in P \times P \times P$ ,  $k \in \Omega$ , let  $B_k(p) = \{i \in N: u_i(p_k) > u_i(y), \forall y \in \Delta(p)\}$ .

**PROPOSITION 2.** *The following  $n$ -tuple  $\sigma^*$  of voter strategies is a voting equilibrium*

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for any  $s \in [3, n/3]$ ,  $s$  odd:

1. a.  $p_L = p_M = p_R \Rightarrow \sigma_i^*(k) = 1/3, \forall i \in N, \forall k \in \Omega$ .

b.  $p_L = p_M < p_R \Rightarrow \sigma_i^*(R) = 1, i = \mu - 1, \dots, n; \sigma_i^*(k) = 1/2, i = 1, \dots, \mu - 2, k = L, M$ .

c.  $p_L < p_M = p_R \Rightarrow \sigma_i^*(L) = 1, i = 1, \dots, \mu + 1; \sigma_i^*(k) = 1/2, i = \mu + 2, \dots, n, k = L, M$ .

2.  $p_L < p_M < p_R$  and  $|B_k| \geq (n + 1)/2$ , some  $k \in \Omega \Rightarrow \sigma_i^* = \sigma_i^0, \forall i \in N$ .

Now suppose that  $p_L < p_M < p_R$  and  $|B_k| < (n + 1)/2, \forall k \in \Omega$ . Then  $x_\mu \in (p_L, p_R)$ , and

3. a.  $x_\mu > p_M \Rightarrow \sigma_i^*(M) = 1, i = 1, \dots, s; \sigma_i^*(R) = 1, i = s + 1, \dots, n$ .

b.  $x_\mu < p_M \Rightarrow \sigma_i^*(L) = 1, i = 1, \dots, n - s; \sigma_i^*(M) = 1, i = n - s + 1, \dots, n$ .

4.  $x_\mu = p_M$  and  $d_L < (>) d_R \Rightarrow \sigma_i^* = \sigma_{i\{L,M\}}^* (\sigma_{i\{R,M\}}^*), \forall i \in N$ .

5.  $x_\mu = p_M$  and  $d_L = d_R = d < (x_{(2\mu+s-1)/2} - x_\mu) \cdot 8/3 \Rightarrow \sigma_i^*(L) = \sigma_\mu^*(R) = 1/2; \sigma_i^* = \sigma_{i\{L,R\}}^*, \forall i \neq \mu$ .

6.  $x_\mu = p_M$  and  $d_L = d_R = d \geq (x_{(2\mu+s-1)/2} - x_\mu) \cdot 8/3 \Rightarrow \sigma_i^*(L) = 1, i = 1, \dots, (2\mu - s - 3)/2, \sigma_i^*(M) = 1, i = (2\mu - s - 1)/2, \dots, (2\mu + s - 1)/2, \sigma_i^*(R) = 1, i = (2\mu + s + 1)/2, \dots, n$ .

*Proof.*

1a. Suppose  $p = (y, y, y)$ . Then  $\Lambda(p) = \{y\}$ , in which case all voters are indifferent over voting strategies. Hence  $\sigma^*(p)$  as specified is an equilibrium.

1b. Suppose  $p = (y', y', p_R)$ , where  $p_R > y' = p_L = p_M$ . Then  $\Lambda(p) = \{y', (y' + p_R)/2, p_R\}$ ,  $v_R(\sigma^*(p)) \geq (n + 3)/2$ , and  $y(w(\sigma^*(p)), p) = p_R$ . Clearly all  $i \in B_R$  are using maximizing strategies. And given  $\sigma^*(p)$ , no  $i \in N \setminus B_R$  is

pivotal between  $\{p_L, p_M\}$  and  $p_R$ . Hence  $\sigma^*(p)$  is an equilibrium.

1c. An argument symmetric to that used in 1b applies.

2. If  $|B_k(p)| \geq (n + 1)/2$ , for some  $k \in \Omega$ , then  $\sigma_i^*(k) = 1$  is clearly a best response for any  $i \in B_k(p)$ , since  $y(w(\sigma^*), p) = p_k$ . And given  $\sigma_{-j}^*$ , no  $i \in N \setminus B_k$  is pivotal between  $p_k$  and any other possible outcome. Hence,  $\sigma^*(p)$  is an equilibrium.

3a. In this instance,  $v_R(\sigma^*(p)) = n - s > 2n/3$ , so that  $y(w(\sigma^*(p)), p) = p_R$ . Since no individual is pivotal,  $\sigma^*(p)$  is an equilibrium.

3b. Mutatis mutandis, the same is true for this case.

4. Suppose  $x_\mu = p_M$  and  $d_L < d_R$ . Then  $v_M(\sigma^*(p)) \geq (n + 1)/2$  and  $y(w(\sigma^*(p)), p) = p_M$ . If any individual  $i$  is pivotal, then  $\sigma_i^*(M) = 1$  and  $v_M(\sigma^*(p)) = v_L(\sigma^*(p)) + 1 = (n + 1)/2$ . Since  $s \geq 3$ ,  $\sigma_i \neq \sigma_i^*$  implies  $y(w(\sigma_i, \sigma_{-i}^*), p) \leq p_M$ , with strict inequality if  $\sigma_i$  is a pure strategy. But  $[x_\mu = p_M, v_M(\sigma^*(p)) = (n + 1)/2, \text{ and } \sigma_{i\{L,M\}}^*(M) = 1]$  implies  $x_i \geq p_M$ . Hence,  $\sigma^*(p)$  is an equilibrium. A symmetric argument holds when  $d_L > d_R$ .

5. By definition of  $p_M$  and  $d$ , the argument for this case follows immediately from that of case 6.

6. Given  $\sigma^*(p)$ ,  $v_L(\sigma^*(p)) = v_R(\sigma^*(p)) = (n - s)/2 > v_M(\sigma^*(p)) = s$ . Hence  $y(w(\sigma^*(p)), p) \in \{p_{LM}, p_{MR}\}$ , where each occurs with probability 1/2. By supposition,  $d_L = d_R = d$ . Thus for all  $j \in N$ ,

$$Eu_j(y(w(\sigma^*(p)), p)) = -(x_j - p_M)^2 - d^2/4$$

Consider any  $i$  such that  $\sigma_i^*(M) = 1$  and suppose  $i$  switches to  $\sigma_i(R) = 1$ . Then,  $v_R(\sigma_i, \sigma_{-i}^*) = (n - s + 2)/2 > y_L(\sigma_i, \sigma_{-i}^*) = (n - s)/2 > v_M(\sigma_i, \sigma_{-i}^*)$

$= s - 1$ . Hence  $M$  is not elected to the legislature, and  $y(w(\sigma_i, \sigma_{-i}^*), p) = p_R$ . In this event,  $i$ 's utility is

$$Eu_i(y(w(\sigma_i, \sigma_{-i}^*), p)) = -(x_i - p_R)^2$$

Therefore,

$$Eu_i(y(w(\sigma^*(p)), p))$$

$$- Eu_i(y(w(\sigma_i, \sigma_{-i}^*), p)) < 0 \Leftrightarrow$$

$$(x_i - p_R)^2 - (x_i - p_M)^2 - d^2/4$$

$$< 0 \Leftrightarrow$$

$$[(x_i - p_R) - (x_i - p_M)] \cdot [(x_i - p_R)$$

$$+ (x_i - p_M)] - d^2/4 < 0 \Leftrightarrow$$

$$x_i > p_M + 3d/8$$

Similarly, if  $\sigma_i(L) = 1$  for any  $i$  such that  $\sigma_i^*(M) = 1$ ,

$$Eu_i(y(w(\sigma^*(p)), p))$$

$$- Eu_i(y(w(\sigma_i, \sigma_{-i}^*), p)) < 0 \Leftrightarrow$$

$$x_i < p_M - 3d/8$$

By definition of this case,  $d \geq (x_{(2\mu+s-1)/2} - p_M) \cdot 8/3$ . Hence all  $i$  such that  $\sigma_i^*(M) = 1$  are using best responses. Now consider any  $i$  such that  $\sigma_i^*(L) = 1$ . If  $\sigma_i(k) = 1$ ,  $k \in \{M, R\}$ , then, because  $3 \leq s < n/3$ , all parties get elected and  $y(w(\sigma_i, \sigma_{-i}^*), p) = p_{MR}$  with probability 1. Hence

$$Eu_i(y(w(\sigma_i, \sigma_{-i}^*), p))$$

$$= -(x_i - p_{MR})^2$$

and

$$Eu_i(y(w(\sigma^*(p)), p))$$

$$- Eu_i(y(w(\sigma_i, \sigma_{-i}^*), p)) < 0 \Leftrightarrow$$

$$x_i > p_M$$

But  $\sigma_i^*(L) = 1$  only if  $x_i < p_M$ . Hence  $\sigma_i^*(L) = 1$  is a best response for such individuals. By symmetry,  $\sigma_i^*(R) = 1$  is a best response for  $i > (2\mu + s + 1)/2$ . This completes the proof of case 6 (and case 5). QED

**PROPOSITION 3.** Relative to the voting strategies of proposition 2,  $p^* \in P \times P$

$\times P$  is an electoral equilibrium for any  $s \in [3, n/3]$ ,  $s$  odd, if and only if

$$1. p_M^* = x_\mu$$

$$2. (p_M^* - p_L^*) = (p_R^* - p_M^*) \in [8/3 \cdot (x_i - x_\mu), 4 \cdot (x_{j^*} - x_\mu)], \text{ where } i^* \equiv \mu + (s-1)/2, j^* \equiv \mu + \text{int}[(n-1)/4]$$

*Proof (suff.).* Suppose  $p^*$  satisfies proposition 3, conditions 1 and 2. Then  $\sigma^*(p^*)$  is described by the voter strategies of case 6 of the voting equilibrium. Hence  $v_L(\sigma^*(p^*)) = v_R(\sigma^*(p^*)) = (n-s)/2 > s = v_M(\sigma^*(p^*))$ , and  $y(w(\sigma^*(p^*), p^*) \in \{p_{LM}^*, p_{MR}^*\}$ . Let  $|p_M^* - p_k^*| \equiv d^*$ ,  $k = L, R$ . Since each outcome occurs with probability 1/2, proposition 1 yields

$$\psi_L(p) = \psi_R(p^*) = 1/2 \cdot (G - d^{*2}/4)$$

$$- d^{*2} - d^{*2}/4 = G/2$$

$$- 11/8 \cdot d^{*2},$$

$$\psi_M(p^*) = 0.$$

By the assumption on the size of  $G$ ,  $\psi_k(p^*) > 0$ ,  $k = L, R$ .

Consider  $p = (p_L^*, p, p_R^*)$  and suppose that  $p \in (p_L^*, p_M^*)$ . Since  $p_M^* = x_\mu$ ,  $|B_k(p)| < (n+1)/2$ ,  $\forall k \in \Omega$ . Hence case 3a of the voting equilibrium obtains at  $p$ , in which case  $v_M(\sigma^*(p)) = s$ ,  $v_R(\sigma^*(p)) = n-s$ , and  $y(w(\sigma^*(p), p) = p_R^*$ . Therefore,  $\psi_M(p) = -(p - p_R^*)^2 < 0$ . Now suppose  $p = p_L^*$ . Then case 1b of the voting equilibrium obtains, and  $v_R(\sigma^*(p)) > (n+1)/2$ , so that  $y(w(\sigma^*(p)), p) = p_R^*$ . Therefore  $\psi_M(p) < 0$ . If  $p < p_L^*$  then again case 3a of the voting equilibrium obtains, but here  $v_M(\sigma^*(p)) = 0$  so that  $\psi_M(p) = -c < 0$ . A symmetric argument holds for  $p > p_M^*$ . Thus,  $p_M^*$  is a best response to  $(p_L^*, p_R^*)$ .

Consider  $p = (p, p_M^*, p_R^*)$  and suppose that  $p < p_L^*$ . Then case 4 of the voting equilibrium obtains with  $d_L > d_R$ , in which case  $v_L(\sigma^*(p)) = 0$ , and  $\psi_L(p) = -c < 0$ . Suppose  $p \in (p_L^*, p_M^*)$ . Then case 4 again obtains, with  $d_L < d_R$ , in which case  $v_M(\sigma^*(p)) \geq (n+1)/2$ . Therefore  $y(w(\sigma^*(p)), p) = p_M^*$  and  $\psi_L(p) = -(p - p_M^*)^2 < 0$ . Suppose  $p = p_M^*$ . Then case 1b

of the voting equilibrium obtains;  $Ev_L(\sigma^*(p)) = (n-3)/4 < v_R(\sigma^*(p)) = (n+3)/2$ , and  $\psi_L(p) \in \{-c, -(p-p_R^*)^2\} < 0$ . If  $p > p_M^*$ , then case 2 of the voting equilibrium obtains, so that  $v_M(\sigma^*(p)) \geq (n+1)/2$  and  $\psi_L(p) \in \{-c, -(p-p_M^*)^2\} < 0$ . Therefore,  $p_L^*$  is a best response to  $(p_M^*, p_R^*)$ . By symmetry,  $p_R^*$  is a best response to  $(p_L^*, p_M^*)$ .

*Proof (nec.).* We prove necessity by first showing that if  $p \in P \times P \times P$  is such that one of cases 1-5 of the voting equilibrium obtains, then  $p$  cannot be an electoral equilibrium. Let  $\Sigma(\sigma^*)$  be the set of electoral equilibria relative to the voter strategies  $\sigma^*$ .

Let  $p = (y, y, y)$ . Then  $\psi_k(p) = G/3$ ,  $\forall k \in \Omega$ . Consider party  $\alpha$ . If  $y \neq x_\mu$ , choosing  $p_\alpha' = x_\mu$  implies  $|B_\alpha(x_\mu, y, y)| \geq (n+1)/2$ , so that  $\psi_\alpha(x_\mu, y, y) = G$ . If  $y = x_\mu$ , choose  $p_\alpha' > y$ . Then case 1b of the voting equilibrium obtains, in which case  $v_\alpha(\sigma^*(p)) = (n+3)/2 > Ev_k(\sigma^*(p))$ ,  $k \neq \alpha$ . Thus  $\psi_\alpha(p_\alpha', y, y) = G$ . Therefore,  $p \notin \Sigma(\sigma^*)$ .

Let  $p = (p_\alpha, y, y)$  and suppose  $p_\alpha > y$ . Consider party  $\beta$ . At  $p$ , case 1b of the voting equilibrium obtains. Therefore,  $\psi_\beta(p) \in \{-c, -(y-p_\alpha)^2\}$ . If  $p_\alpha \neq x_\mu$ , choose  $p_\beta' = x_\mu$ . Then either case 2 or case 4 obtains. In either case,  $v_\beta(p_\alpha, x_\mu, y) \geq (n+1)/2$  and  $\psi_\beta(p_\alpha, x_\mu, y) = G$ . If  $p_\alpha = x_\mu$ , choose  $p_\beta' > x_\mu$  such that  $(p_\beta' - x_\mu) < (x_\mu - y)/t$ ,  $t \geq 2$ . Then case 4 of the voting equilibrium obtains with  $d_L > d_R$ , so that  $v_\gamma(p_\alpha, p_\beta', y) = 0 < v_\beta(p_\alpha, p_\beta', y) < v_\alpha(p_\alpha, p_\beta', y)$ . Hence for sufficiently large  $t$ ,

$$\begin{aligned} \psi_\beta(p_\alpha, p_\beta', y) &= -(p_\beta' - p_\alpha)^2 \\ &> \max \psi_\beta(p). \end{aligned}$$

Therefore,  $p \notin \Sigma(\sigma^*)$ . By symmetry, the same is true for  $p_\alpha < y$ .

Let  $p = (p_\alpha, p_\beta, p_\gamma)$  and suppose all parties adopt distinct positions. Then we can write  $p = (p_L, p_M, p_R)$ . Let  $p$  be such that case 2 of the voting equilibrium occurs. Suppose  $|B_L(p)| \geq (n+1)/2$ , and consider  $p' = (p_L, p_M, p)$ . By the assumption

of symmetric utilities,  $x_\mu < (p_L + p_M)/2$ . If  $p_L \neq x_\mu$ , choose  $p = x_\mu$ . Then either case 2 occurs or case 4. In both cases,  $v_R(p') \geq (n+1)/2$ , so that  $\psi_R(p') = G > \psi_R(p)$ . If  $p_L = x_\mu$ , choose  $p < x_\mu$  so that  $(x_\mu - p) = (p_M - x_\mu)/t$ ,  $t \geq 2$ . Then case 4 occurs at  $p'$  with  $d_L < d_R$ . Thus we have  $\psi_R(p') > \max \psi_R(p)$  for sufficiently large  $t$ . Therefore, if  $p \in \Sigma(\sigma^*)$  and case 2 of the voting equilibrium occurs,  $|B_L(p)| < (n+1)/2$ ; by symmetry,  $|B_R(p)| < (n+1)/2$ . And if  $|B_M(p)| \geq (n+1)/2$ , the same arguments, mutatis mutandis, apply. Therefore,  $p \notin \Sigma(\sigma^*)$ .

Let  $p = (p_L, p_M, p_R)$  and assume hereafter that  $|B_k(p)| < (n+1)/2$ , for  $k = L, M, R$ .

Suppose that  $x_\mu > p_M$ . Then  $v_L(\sigma^*(p)) = 0$  and  $\psi_L(p) = -c$ . Assume first that  $(p_M + p_R)/2 \neq x_\mu$  and consider  $p' = (x_\mu, p_M, p_R)$ , where the party offering  $p_L$  in  $p$  now offers  $x_\mu$ . Then case 4 occurs and  $\psi_L(p') = G > \psi_L(p)$ . Now assume  $(p_M + p_R)/2 = x_\mu$  and consider  $p' = (p_L, x_\mu, p_R)$ , where the party offering  $p_M$  in  $p$  now offers  $x_\mu$ . Then again case 4 occurs, and  $\psi_M(p') = G > \psi_M(p) = -(p_M - p_R)^2$ . So  $p \notin \Sigma(\sigma^*)$ ; by symmetry the same is true when  $x_\mu < p_M$ .

Let  $p = (p_L, p_M, p_R) = (p_L, x_\mu, p_R)$  and suppose  $d_L \neq d_R$ . Then case 4 of the voting equilibrium occurs and  $v_k(\sigma^*(p)) = 0$  for some  $k = L, R$ . Let  $k = L$  so that  $d_L > d_R$ , and  $\psi_L(p) = -c$ . Consider  $p' = (p, p_M, p_R)$ ,  $p < p_M$  and  $(p_M - p) = (p_R - p_M)/t$ ,  $t \geq 2$ . Then case 4 obtains at  $p'$  and  $d_L' < d_R' = d_R$ . Hence  $v_L(\sigma^*(p')) \geq s$  and, for sufficiently large  $t$ ,  $\psi_L(p') > \psi_L(p)$ . Therefore,  $p \notin \Sigma(\sigma^*)$ ; by symmetry, the same is true when  $k = R$ .

Let  $p = (p_L, p_M, p_R) = (p_L, x_\mu, p_R)$  and suppose  $d_L = d_R = d < 8/3 \cdot (x_{(2\mu+s-1)/2} - x_\mu)$ . Then case 5 of the voting equilibrium occurs. Hence  $v_M(\sigma^*(p)) = 0$  and  $\psi_M(p) = -c$ . Consider  $p' = (p_L, p_M - \epsilon, p_R)$ ,  $\epsilon > 0$ . Then case 3a of the voting equilibrium occurs with  $x_\mu > p_M - \epsilon$ , in which case  $v_M(\sigma^*(p')) = s$  and  $\psi_M(p') = -(d + \epsilon)^2$ . Therefore

$$\psi_M(p') - \psi_M(p) = c - (d + \epsilon)^2 > 0 \Leftrightarrow c - d^2 > \epsilon \cdot (2d + \epsilon).$$

By assumption,  $c \geq 8/3 \cdot (x_{(2\mu+s-1)/2} - x_\mu)^2 > d^2$ , so for sufficiently small  $\epsilon$ ,  $\psi_M(p') > \psi_M(p)$ . Therefore,  $p \notin \Sigma(\sigma^*)$ .

Putting the previous arguments together, we have that  $p \in \Sigma(\sigma^*)$  implies  $\sigma^*(p)$  is such that case 6 of the voting equilibrium occurs. To complete the argument for necessity, note that by the symmetry of voter preferences and the distribution of ideal points

$$[p_M = x_\mu, d_L = d_R = d, \text{ and}$$

$$d \geq 4 \cdot (x_{j^*} - x_\mu)] \Rightarrow$$

$$|B_M(p)| \geq (n+1)/2.$$

QED

## Note

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# EXTENDED DETERRENCE AND THE OUTBREAK OF WAR

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*Successful deterrence, it is argued, requires a combination of military capabilities and bargaining behavior that enhances a defender's credibility without provoking a potential attacker. Hypotheses on the political and military conditions under which extended-immediate deterrence is likely to succeed or fail are formulated and tested by probit analysis on fifty-eight historical cases. The empirical results indicate that (1) the military capability of the defender to deny the potential attacker a quick and decisive victory on the battlefield enhances deterrence; (2) a policy of reciprocity in diplomacy and military actions by the defender contributes strongly to deterrence success; and (3) a past record of backing down under pressure or intransigence in confrontations with the potential attacker increases the likelihood of deterrence failure.*

To threaten or resort to the use of military force is a critical foreign policy decision for the leadership of a country. Policymakers have often believed that national interests extended beyond the defense of their own country to include the security of other states. As a result, when such countries were the targets of an attack, policymakers have threatened military retaliation against the potential aggressor in an attempt to deter the attack. The effective use of extended deterrent threats, however, is not easily achieved.

A potential aggressor's uncertainty as to the willingness of the deterrer to risk war over the defense of another state can weaken the credibility of deterrence. As Schelling (1966, 36) argues, "The difference between the national homeland and everything 'abroad' is the difference between threats that are inherently credible, even if unspoken, and threats that have to be made credible. To project the shadow of one's military forces over other countries and territories is an act of diplomacy."

The security dilemma can lead policy-

makers to overestimate the hostile intentions and threats posed by the military capabilities and actions of other states. In a situation of extended deterrence the intended defensive military actions of the deterrer can be perceived by the potential aggressor as an offensive threat and thereby provoke a spiral of military escalation (Jervis 1976, 58-113).

A deterrer must strike a balance between actions that demonstrate resolve and enhance the credibility of threats and the provocation of the potential aggressor. In this study I will test hypotheses on historical cases of extended deterrence from 1885 to 1984 in an attempt to identify the conditions under which extended deterrence is likely to succeed or fail.

## Concept of Extended Deterrence

In this study cases of extended-immediate deterrence will be analyzed. The concept of *deterrence* will be defined as follows:

1. *Deterrence*. A policy of deterrence seeks to convince an adversary by the threat of military retaliation that the costs of resorting to the use of military force to achieve foreign policy objectives will outweigh the benefits (Howard 1982-83, 315). The deterrer's threat of retaliation may be based on either the military capability to repulse an attack and thereby deny the attacker its battlefield objectives and prevent the loss of territory or the capability to inflict heavy military losses on the attacker in a protracted armed conflict of attrition (deterrence by "denial" versus "punishment"; see Snyder 1961, 14-16).
2. *Extended deterrence*. A confrontation in which the policymakers of one state ("defender") threaten the use of force against another state ("potential attacker") in an attempt to prevent that state from using military force against an ally—or territory controlled by an ally ("protégé")—of the defender. The objective of extended deterrence is to protect other countries and territories from attack, as opposed to preventing a direct attack on one's own national territory.
3. *Extended-immediate deterrence*. A confrontation that is characterized by the following:
  - a. A potential attacker is actively considering the use of force against a protégé of the defender.
  - b. Policymakers of the defender state are aware of the threat directed at the protégé.
  - c. Policymakers of the defender state, either explicitly or by the movement of military forces, threaten the use of retaliatory force in an attempt to prevent the use of force by the potential attacker (Morgan 1977, 38).

An extended-immediate deterrence confrontation entails an overt threat and

counterthreat by potential attacker and defender. In contrast, *extended-general deterrence* refers to an adversarial relationship between states in which the threat of an armed conflict over the security of another country exists but the potential attacker is neither engaged in a dispute that threatens war nor has issued coercive threats or initiated military preparations for the use of force. Extended-general deterrence is maintained by arms races, the formation of alliances, and in the declaratory statements of foreign policy leaders as to their country's security interests. For example, United States troops are deployed in South Korea as a deterrent to a potential North Korean attack and statements by United States leaders of the intent to defend the Persian Gulf are designed to counter a potential Soviet threat to countries in the region.

Fifty-eight cases of extended-immediate deterrence were identified for the period 1885-1984.<sup>1</sup> In a majority of cases both the potential attacker and defender were great powers, while in a small number of cases both states were regional powers. Twenty-four cases were coded as failures of deterrence while the remaining thirty-four cases were coded as successful deterrence (see the Appendix for a complete listing of the cases and discussion of coding procedures).

### Assumptions

The decision-making calculus of the potential attacker is based on certain assumptions.

### Offense and Defense

The objectives of the potential attacker in threatening the use of force against the protégé represent a combination of both offensive and defensive goals. Analytically, it may be possible to draw a relatively clear distinction between these two op-

posing motives; but in the actual calculations of the potential attacker the two are often closely interconnected. Defensive motives determine the minimal goals to be pursued by the potential attacker, while offensive motives define the more ambitious maximum goals to be pursued. The important implication is that in most international disputes and crises there is an opportunity for diplomacy and negotiations to contribute to conflict resolution because the potential attacker is not motivated solely by far-reaching and aggressive aims.

### Force and Negotiation

In deciding whether to challenge the deterrent threats of the defender, it is assumed that the potential attacker will consider two alternative courses of action:

1. reliance on military threats and the use of force to resolve the conflict with the defender and protégé
2. reliance on negotiations and diplomacy to resolve the conflict with the defender and protégé

In game-theoretic terms, these two courses of action define the basic structure of the conflict for the potential attacker, in which the choice is between the coercive strategy of standing firm behind its demands, or the accommodative strategy of cooperating and making concessions to the defender's bargaining position (Snyder and Diesing 1977, 40-41). A number of variants can be identified for each of these two courses of action and the potential attacker may combine the two policies in the initial stages of the confrontation; but once the intentions of the defender are determined, a fundamental decision will be made by the potential attacker as to what policy to adopt. As a result, these two courses of action become policy alternatives at this crucial point in the confrontation with the defender.

### Bargaining Reputation

The potential attacker will be sensitive to how the outcome of the confrontation with the defender will affect its own bargaining reputation. Bargaining reputation can be defined as the willingness of state's foreign policy leadership to risk armed conflict in pursuit of political goals and to refuse to concede to the demands of adversaries under coercive pressure. Bargaining reputation is important because policymakers very often believe that their actions and behavior in a particular confrontation will be interpreted by other states as a general indicator of their resolve (see Snyder and Diesing 1977, 187-88). The political leadership's bargaining reputation has both domestic as well as international consequences. Domestically, the leadership's political stature and influence within the elite and broader population will depend in part on its record of managing difficult foreign policy issues and international crises. A successful record will enhance the leadership's political stature and authority, whereas a record of weakness and setbacks in foreign policy will diminish the leadership's political position. Internationally, a strong bargaining reputation will increase a state's position of leadership, influence with allies, and the credibility of coercive threats directed at adversaries; while a weak bargaining reputation will have the reverse impact. The important implication for a potential attacker is that the desire to protect its bargaining reputation will be an important factor in a decision whether to challenge the defender's deterrent threat.

### Costs and Gains

The final decision of the potential attacker will be based on a comparison of the likely costs and benefits of the two alternative policy choices. The course of action with greater expected gains (or

fewer losses) will be adopted by the potential attacker.

### Hypotheses on Extended Deterrence

To be deterred the potential attacker must conclude that the defender's deterrent actions are both credible and stable. A *credible* deterrent depends on whether the defender possesses the military capabilities sufficient to inflict high military costs on the attacker in an armed conflict (battlefield casualties, destruction and depletion of arms and equipment, damage to and potential loss of territory) and the intention to use those capabilities if necessary. A *stable* deterrent does not increase the potential attacker's fear of a preemptive military strike and thus provoke rapid military escalation and avoids engaging the bargaining reputation of the potential attacker to such an extent that it makes it more difficult for the potential attacker to back down from its coercive bargaining position.

Four sets of variables can be identified that are likely to play a critical role in determining the credibility and stability of the defender's deterrent actions. These four sets of variables are hypothesized to play a crucial role in the potential attacker's calculus of deterrence:

1. the balance of military forces between potential attacker and defender
2. the interests at stake for the defender in protecting the protégé
3. the past behavior of the defender in confrontations with states
4. the bargaining behavior of the defender during the immediate deterrence confrontation with the potential attacker

### The Military Balance

Immediate deterrence is likely to succeed when the potential attacker estimates

that the probability of success is relatively low and the costs high in resorting to the use of sustained military force to achieve foreign policy objectives. The potential attacker is likely to consider three alternative options for using military force:

1. limited aims strategy
2. rapid offensive attack strategy
3. attrition strategy (George, Hall, and Simons 1971, 15-21; Mearsheimer 1983, 23-66).

The objectives of a limited aims strategy is to seize control of disputed territory held by an opponent. The key to success for the potential attacker is to achieve tactical, if not strategic, surprise in the use of force.<sup>2</sup> The potential attacker is then in a position either to overwhelm the underprepared forces of the opponent or to occupy the disputed territory before the opponent has the opportunity to send in reinforcements and build up local defenses. The goal of the attacker is to limit, if not avoid, direct engagement with the military strength of the opponent and instead use force where local superiority prevails.

The second military option for the potential attacker is the strategy of rapid offensive attack in which the objective is to defeat the opponent's armed forces decisively. The goal is not to minimize armed conflict with the adversary and to take control of disputed territory but to engage the armed forces of the adversary in a series of large-scale battles so that its armed forces can be defeated. The assumption of the potential attacker is that if the armed forces of the adversary can be defeated in a rapid and decisive manner, the adversary will be forced to surrender before it can mobilize the economy and civilian population for war or before allies can effectively intervene in support of the target of the attack.

The third military option available to a potential attacker is the strategy of attri-

tion. The objective is similar to that of the rapid offensive attack: the decisive defeat of the armed forces of the adversary. The principal difference between the two strategies, however, is the time required to achieve military victory. With a strategy of attrition the attacker does not anticipate a series of rapid victories but a protracted conflict in which the goal is to wear down and outlast the adversary by being able to withstand heavy military losses better.

One factor that is likely to have an important impact on the success of each of these military strategies is the balance of military forces between attacker and defender. To counter a limited aims strategy, defending forces should be in a position to repulse the armed forces of the attacker so they cannot seize disputed territory. The best defense then is for the defender to have strong forces in forward positions at the point of attack. In a confrontation where the attacker is considering a rapid offensive attack, the outcome of large-scale battlefield engagements is critical. Deterrence success will depend in part on the strength of the standing armed forces that each side can mobilize and deploy on the battlefield in the initial weeks and months of combat. When the attacker is considering a strategy of attrition, the long-term balance of forces is critical to the success or failure of deterrence. The outcome of a protracted conflict will be determined in part by the capacity of each side to sustain the commitment of large armies on the battlefield for an extended period of time. To provide such support a country must be able to maintain domestic support for the war, have the bureaucratic-administrative capacity to mobilize the population and economy for war, the productive capacity to be able constantly to replace the equipment and arms destroyed, and the population base to absorb heavy casualties.

Military and political leaders considering an attack generally seek to achieve

their objectives in a rapid and decisive manner through the use of force. A number of recent studies have argued persuasively that military organizations are biased towards doctrines and strategies that call for the rapid and decisive use of military force (Snyder 1984, 118–22, Van Evera 1984, chap. 7). Institutional interests and incentives—such as the desire to increase the size and amount of expenditures for armed forces; to increase autonomy from civilian intervention in operational planning; and to reduce the uncertainty of the battlefield and provide a structured environment for planning and training—contribute to a strong bias for rapid victories by means of preplanned offensives. For example, Betts (1977, 3–15), in his study of the influence of the military on U.S. crisis decisions since 1945, argues that the weight of military advice has generally counseled against the use of force unless the United States was in a strong military position to defeat the adversary in a quick and decisive manner. Contemporary Soviet doctrine for conventional war in Europe calls for a blitzkrieg offensive against NATO forces. Political leaders have a strong interest in avoiding costly armed conflicts and, instead, in sanctioning the use of force when a rapid and decisive victory is anticipated. In a prolonged war high levels of battlefield casualties and the potential for substantial economic dislocations and burdens being imposed on the population create very real risks of a loss of domestic political support. For example, Arthur Stein (1980) has reported that popular support for United States participation in World War II, the Korean War, and the Vietnam War declined steadily over time. The desire to avoid protracted and costly wars is also reflected in Mearsheimer's study (1983, chaps. 5, 8) of a number of historical cases of conventional deterrence. Mearsheimer found that military leaders were often under considerable pressure from political leaders to devise

plans of attack that projected a quick victory.

If the potential attacker's policymakers believe that military force can be successfully employed in a quick and decisive attack, then the probability of deterrence success will decrease. Conversely, if the policymakers estimate that military force cannot be successfully employed in a quick and relatively low-cost attack, then deterrence is more likely to prevail. Calculations regarding the outcome of a protracted war are not likely to have a critical impact on deterrence outcomes since policymakers do not generally initiate an armed conflict with the intention of engaging in a war of attrition.<sup>3</sup> The immediate balance of forces (defense against the limited aims strategy or the rapid offensive attack designed to defeat the protégé before the defender can intervene) and the short-term balance of forces (defense against the strategy of rapid offensive attack in which the attacker seeks to defeat both the defender and protégé) should be positively related to the success of extended-immediate deterrence.

**HYPOTHESIS 1.** *The probability of extended deterrence success increases as the balance of military forces improves for the defender. The immediate and short-term balance of forces will have a greater impact on deterrence outcomes than will the long-term balance of forces.*

The deterrent value of nuclear weapons is a controversial and important question. The credibility of a nuclear threat will depend on the political-military situation in which they might be employed. For purposes of self-defense and to deter the use of nuclear weapons by another state against one's own territory the threat of nuclear retaliation is likely to function as a credible deterrent, given a secure second-strike capability (Howard 1984, 95-98; Jervis 1984, 126-46). However, in this

study, of the fifteen cases in which the defender possessed nuclear weapons, the potential attacker in fourteen cases was a nonnuclear power. (The only exception is the 1979 case of the People's Republic of China as the potential attacker and the Soviet Union as defender of Vietnam.) The critical question, then, is what extended deterrent value do nuclear weapons exert when the potential target is a nonnuclear power? In such situations it is argued that the deterrent impact will be minimal for a number of reasons. First, the use of nuclear weapons would risk provoking strong criticism and denunciation both internationally and domestically (in twelve of fifteen cases the nuclear power defender was the United States, United Kingdom, or France). Secondly, nuclear use would be likely to increase the incentives for horizontal nuclear proliferation, which the nuclear powers have consistently opposed. Thirdly, the likelihood of collateral damage to civilians would act as a strong constraint. In summary, the combination of salient political, military, and ethical questions about the immediate and long-term consequences of nuclear use by the defender against a non-nuclear power raise serious doubts as to the credibility of such a decision.

**HYPOTHESIS 2.** *The possession of nuclear weapons and the latent threat of nuclear use by the defender will not have a significant impact on extended deterrence outcomes when the potential attacker is a nonnuclear power.*

### Interests at Stake for the Defender

The potential attacker's uncertainty as to the intentions of the defender are inherent in a situation of extended deterrence. If the policymakers of the potential attacker are to believe that the defender is willing to incur the costs of armed conflict in defense of the protégé, they must be convinced that the issues at stake are vital

enough to the defender to justify those costs. If the potential attacker does not believe that the defender has important interests at risk in defending the protégé, then the credibility of extended deterrence will be questioned (George and Smoke 1974, 560-61).

The value of the protégé to the defender can be determined in part by the political, military, and economic ties that link the defender to the protégé. The stronger those ties, the more important they are to the defender to maintain and protect. The presence of a military alliance between defender and protégé indicates that the defender has reputational interests at stake. The credibility of the alliance commitment for the defender can be linked to the defender's concern to maintain a reputation for honoring agreements with states and a reputation for a willingness to use force in support of allies when threatened. The failure to honor an alliance if the protégé is the target of an unprovoked attack risks signaling to other states that the defender is reluctant to use force if its own security is not directly threatened. As a result, not only may the defender's security commitments to other states be questioned, but the defender may also subsequently find it more difficult to secure allies in support of its own defense. In the period from 1816-1965, 76% of allied nations that were attacked received armed support, whereas only 17% of nonallied states received such support (Bueno de Mesquita 1981, 113). Huth and Russett (1984, 521; Huth and Russett, n.d.) report that alliance ties increased significantly the likelihood that the defender would go to war to defend a protégé if attacked.

**HYPOTHESIS 3.** *The probability of extended deterrence success will increase if the defender has an alliance with the protégé.*

Military arms transfers from defender to protégé also indicate that reputational

interests are at stake for the defender. The greater the degree to which the protégé relies exclusively on the defender for arms imports, the more likely it is that the defender will be viewed by other states as developing a sphere of influence and closer political ties with that country. Military arms transfers do not represent a formal security commitment but the defender's reputation for supporting friendly countries and willingness to use force in defense of other countries can become intertwined with the security of a protégé that depends largely on the defender for military aid. A tacit alliance may therefore evolve over time in which the supplier (defender) is recognized by other states as the principal protector of the recipient (protégé).

**HYPOTHESIS 4.** *The probability of extended deterrence success will increase as the protégé's reliance on arms transfers from the defender increases.*

The protégé may also be economically important to the defender. High levels of foreign trade between defender and protégé indicate some of the potential economic costs to be incurred if the protégé is forced by the attacker to cut its ties with the defender. Exports from the defender to the protégé contribute to higher levels of domestic employment and output, while imports from the protégé may represent important sources of raw materials or products that cannot be produced as efficiently domestically.<sup>4</sup>

**HYPOTHESIS 5.** *The probability of extended deterrence success will increase as the protégé's share of the defender's foreign trade increases.*

### **Bargaining Behavior of the Defender**

The potential attacker's uncertainty as to the defender's intentions and sensitivity to threats and challenges indicate that the defender's bargaining behavior can play a

critical role in the success or failure of deterrence. An effective bargaining strategy by the defender can reduce the potential attacker's uncertainty and thereby enhance credibility, whereas ineffective strategies can reduce credibility or provoke the potential attacker. The optimal bargaining strategy will enhance the credibility of the defender's deterrent actions while avoiding the provocation of the potential attacker. There are two principal dimensions to the bargaining process in the confrontation between the potential attacker and defender: (1) diplomacy and negotiations and (2) military preparations and movement of forces.

For both dimensions of the bargaining process three general strategies are available to the defender ranging from conciliation and minimal military actions to an unyielding position in negotiations and maximum military preparations. The three strategies of military escalation by the defender are

1. *Policy of strength.* The defender responds to the military actions of the potential attacker with greater-than-equal levels of military preparedness.
2. *Policy of tit for tat.* The defender responds to the military actions of the potential attacker with equal levels of military preparedness.
3. *Policy of caution.* The defender responds to the military actions of the potential attacker with less-than-equal levels of military preparedness (Leng and Wheeler 1979; Snyder and Diesing 1977, 209-43).

The three strategies of diplomacy and negotiations for the defender are

1. *Bullying policy.* The defender adopts an unyielding position in support of its demands and does not reciprocate accommodative initiatives by the potential attacker.
2. *Firm-but-flexible policy.* The defender adopts a mixed policy of refusing to

concede to the repeated demands and threats of the potential attacker while also proposing to compromise based on reciprocal concessions.

3. *Conciliatory policy.* The defender adopts a policy of proposed concessions despite the lack of reciprocation by the potential attacker (Kaplowitz 1984; Leng and Wheeler 1979; Snyder and Diesing 1977).

Caution in military actions by the defender will minimize the risks of provocation due to the security dilemma and thereby enhance deterrence stability. Similarly, conciliation in negotiations by the defender will avoid engaging the potential attacker's bargaining reputation, which will also enhance deterrence stability. The critical problem, however, with these two strategies is that they assume, or are designed to deal with, a potential attacker motivated by defensive goals only. As a result such policies fail to recognize that the potential attacker is also likely to be considering more ambitious offensive objectives; thus deterrence stability is increased but at the expense of deterrence credibility. The reverse problem exists when the defender adopts a bullying negotiating strategy or a policy of strength in military preparedness. An unyielding diplomatic stance clearly signals the defender's intention to support the protégé and possibly risk armed conflict; but the defender's refusal to propose or consider some form of limited concessions is likely to increase the potential attacker's concern that a retreat will be viewed by other states as capitulation under pressure. A policy of military strength will put the defender in a favorable military position to intervene if deterrence should fail; but it may also increase the chances of deterrence failure by provoking either further escalation by the potential attacker and fears of a pre-emptive strike or the potential attacker's determination to avoid signs of retreat



under maximum coercive pressure. Diplomatically and militarily, these two strategies are optimal for enhancing credibility if the potential attacker is motivated solely by offensive aims; but if the security dilemma and defensive objectives are also an important component of the potential attacker's calculus, then such policies risk provocation.

The optimal strategy for balancing credibility and stability is to pursue a policy of reciprocity. Diplomatically, reciprocity signals to the potential attacker the determination of the defender to support the protégé while at the same time signaling the defender's intention to not coerce or force the potential attacker to accept a settlement that is clearly one-sided. Proposals for negotiations and limited compromise may therefore offer a way for the potential attacker to back down in the confrontation with minimum damage to its bargaining reputation. Tit-for-tat military escalation signals both the defender's intention to use force if the protégé is attacked and the increased capability to act promptly if necessary but does not place the potential attacker in a highly vulnerable military position.

**HYPOTHESIS 6.** *A policy by the defender of tit for tat in military escalation will increase the probability of extended deterrence success in comparison to alternative policies.*

**HYPOTHESIS 7.** *A firm-but-flexible policy of diplomacy and negotiations by the defender will increase the probability of extended deterrence success in comparison to alternative policies.*

### Past Behavior of the Defender

The bargaining behavior of the defender provides a direct means by which to signal to the potential attacker the defender's intentions to support the protégé. The potential attacker, however, may also draw on the past behavior of the

defender in international confrontations to help determine the defender's willingness to risk armed conflict or to concede under coercive pressure. If security commitments are interdependent (Schelling 1966, 55-59), a defender's past actions in a confrontation should have an impact on the potential attacker's assessment of the defender's likely behavior in the extended-immediate deterrence conflict. If the defender has backed down in a previous conflict and conceded to the demands of the opponent in order to avoid an armed conflict, the credibility of the defender's extended deterrent actions are more likely to be questioned. If the defender, however, did not concede under coercive pressure then the credibility of the defender's extended deterrent actions should be enhanced.

The work of Robert Jervis (1976, 239-70) suggests that historical events have their greatest impact on policymakers when they or their country is directly involved in the event. Jervis argues that personal and direct experience of this kind can prove to be a powerful determinant of a policymaker's image of an opponent. The past behavior of the defender therefore can be divided into two sets of cases for purposes of analysis: (1) previous cases of attempted extended deterrence in which the identity of the potential attacker varies from case to case and (2) only those previous conflicts in which the defender confronted the same potential attacker as in the current case of attempted deterrence.

**HYPOTHESIS 8.** *The past behavior of the defender in those confrontations in which the current potential attacker was directly involved will have a greater impact on deterrence outcomes than in those cases in which the current potential attacker was not directly involved.*

Consistent with the logic underlying the hypotheses on bargaining behavior, a

past record of firm-but-flexible bargaining by the defender should increase the probability of deterrence success in comparison to a past record of conciliation or bullying. A past record of making concessions under coercive pressure and an unwillingness to risk armed conflict should contribute to the weakening of the defender's future credibility; while a past record of intransigence and bullying will have already imposed domestic political as well as international costs on the potential attacker. As a result, the potential attacker should be even more determined to protect its bargaining reputation and avoid another retreat before the defender (see Leng 1983).<sup>5</sup>

*HYPOTHESIS 9. In past confrontations between defender and potential attacker firm-but-flexible bargaining by the defender will enhance the deterrence actions of the defender in the next confrontation between the two states. If the defender, however, adopted a bullying or conciliatory strategy, then the probability of deterrence failure in the next confrontation will increase.*

### Measurement of Variables

The dependent variable was coded as either deterrence success (one) or failure (zero) in accordance with the coding rules outlined in the Appendix.

To measure the conventional balance of military forces a number of different dimensions affecting military capabilities could be analyzed: force-to-space ratios, the quantity and quality of weaponry, the mobility of forces based on logistical support and command and control capabilities, and the capacity of states to mobilize economic and manpower resources efficiently (Epstein 1987; Mearsheimer 1983, chap. 6; Organski and Kugler 1980, chap. 2; Posen 1984-85). In this study, due to the limits of data availability, military capabilities will be measured as simply the

ratio of defender-protégé forces to potential attacker forces. Three different operational measures were constructed. First, the immediate balance of forces includes those ground forces (number of troops) of the defender-protégé and potential attacker that can be deployed in the battlefield at the very outset of an armed conflict. The *immediate balance* is defined as those land forces of the potential attacker in a position to initiate an attack directly and those land forces of defender-protégé in a position to repulse such an attack directly. To be in a position to engage in combat directly the opposing troops must be either concentrated and mobilized for combat at the point of likely attack or capable of mobilization for immediate combat from forward positions to the point of attack. Primary as well as secondary sources were utilized for each case to collect this information. Second, the *short-term balance* is defined as the capacity of the potential attacker and defender-protégé to reinforce and augment the immediate balance of forces through the mobilization of each country's standing ground and air forces and first class of trained reserves.<sup>6</sup> The third measure is the *long-term balance* of forces, which is defined as the capacity of each side to build up its existing armed forces and to maintain that increased level of fighting strength by the mobilization of the economy and population for a protracted war. The composite national capabilities index (Singer, Bremer, and Stucky 1972) was adopted by multiplying each state's existing military capabilities (share of world total military personnel and expenditures) by the sum of that state's industrial and demographic size (share of world total steel production, industrial fuel consumption, urban, and total population).<sup>7</sup> The deterrent value of nuclear weapons for the defender was recorded as one for those cases in which the defender possessed nuclear weapons and zero for the remaining cases.<sup>8</sup>

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## Extended Deterrence

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The military and economic ties between defender and protégé were measured as follows: First, the existence of an *alliance* between the two states was coded with a value of one, the absence of an alliance was coded with a value of zero. Second, *military arms transfer ties* was defined as the share of arms imported by the protégé from the defender as a percentage of total arms imported over a three-year period prior to the case of extended deterrence. The percentage share was calculated on a 10-point scale ranging from 1 (0%–10%) to 10 (91%–100%). Third, *economic ties* were measured as the protégé's share of the defender's total merchandise exports and imports over a three-year period (Huth and Russett 1984, 511–13).

The following categories were utilized to code the outcomes of previous cases of attempted extended deterrence for the defender:

1. *Success*. Defender stands firm in support of protégé and potential attacker refrains from attack.
2. *Failure but armed support*. Deterrence fails, and protégé is attacked; but defender does intervene with military force to protect protégé.
3. *Capitulation*. Deterrence fails, but defender does not intervene with military force to protect protégé.

Categories 1 and 2 represent indicators of a past record of firmness for the defender, while category 3 indicates a record of weakness. In the statistical analysis a value of one was recorded for each case with one of the three possible outcomes listed above, while a value of zero was recorded for each case in which there was no past attempt at extended deterrence for the defender.

The following categories will be utilized to describe the outcomes of the most recent past confrontation between the defender and potential attacker:

1. *Diplomatic put-down*. The defender

adopts a bullying bargaining strategy or forces the potential attacker to make critical concessions in order to avoid an armed conflict, or both.

2. *Stalemate*. The defender and potential attacker avoid a direct military confrontation, but firm-but-flexible bargaining by the defender fails to achieve any agreement with the potential attacker resolving the underlying issues in dispute.
3. *Diplomatic defeat*. The defender retreats under the pressure of the potential attacker and concedes on the critical issues in dispute in order to avoid becoming involved in a direct military confrontation (Leng 1983; Maoz 1982, 226–31).

To determine which outcome applied to the past behavior of the defender, a detailed summary of diplomatic negotiations was recorded for each past case and then examined to determine to what extent the defender had conceded to the demands of the potential attacker.<sup>9</sup> The distinction between a put-down by the defender and a stalemate is based on whether the potential attacker was forced to make critical concessions or the defender rebuffed proposals for compromise. In the case of a stalemate both sides tacitly agree not to press their demands further and the immediate confrontation is allowed to deescalate as neither side is forced to back down and accept the demands or position of its opponent. The defender adopts a firm-but-flexible bargaining strategy; but the potential attacker is not interested in a compromise settlement, and therefore the issues in contention remain unresolved. To test the hypothesis that a record of past conciliation or bullying with the potential attacker reduces the likelihood of deterrence success, those cases in which the defender has either backed down and accepted a diplomatic defeat or the potential attacker has done so will be examined. To test the

hypothesis that a record of firm-but-flexible bargaining enhances the likelihood of deterrence success, those cases in which the past behavior of the defender is coded as a stalemate will be analyzed.

For purposes of statistical analysis the three different outcomes of past confrontations between the defender and potential attacker will be treated as dummy variables. For each case that the past behavior of the defender is coded as either *diplomatic put-down*, *stalemate*, or *diplomatic defeat*, a value of one will be recorded for that case. The excluded category (a value of zero) will be those cases in which there is no past confrontation between defender and potential attacker.

To operationalize the diplomatic and military strategies adopted by the defender a detailed chronological summary of the negotiating process and military actions of each state was compiled for each case. To determine which diplomatic strategy was pursued by the defender the record of the negotiating process was broken down into a series of sequential exchanges between defender and potential attacker from the beginning to the end of the dispute. The coding rules of Leng and Wheeler (1979, 657-71) were then adopted to identify which of the three strategies the defender had pursued.<sup>10</sup> A value of one was recorded for each case in which the defender adopted a firm-but-flexible negotiating strategy and a value of zero for those cases in which alternative strategies were adopted. For example, in the Anglo-Russian dispute over the northern Afghan border in 1885 the British pursued a firm-but-flexible strategy by proposing to the Russians that the Panjdeh territory would be conceded only if Afghanistan was to retain control over disputed territory that was strategically vital. In contrast, in the confrontation between Russia and Japan over Korea and Manchuria in 1903-4, the Russian government refused to negotiate over Russian

rights in Manchuria; while at the same time it sought to place severe constraints on Japanese rights in Korea (Huth n.d., chap. 6).

To determine which strategy of military escalation was pursued by the defender the summary of military actions was broken down chronologically into a series of moves and countermoves by each side. The military moves of defender and potential attacker were then categorized according to the following scale of escalation:

1. symbolic show of force, display of military presence
2. demonstration of military capabilities
3. build-up of military forces
4. positioning of forces for immediate use
5. preparation of forces for immediate use
6. mobilization of forces for war

For each round of move and countermove the response of the defender was coded as either *greater than*, *equal to*, or *less than*, the military actions of the defender. The coding rules of Leng and Wheeler were then adopted to identify the overall policy of military escalation pursued by the defender.<sup>11</sup> For those cases in which the defender adopted a policy of tit for tat a value of one was recorded, while a value of zero was assigned to those cases in which a policy of strength or caution was adopted. For example, Libyan armed intervention in the Chadian Civil War in 1983 and preparation to advance on the capital was reciprocated by the airlifting of French troops to the Chadian capital and their deployment along the so-called red line north of the capital. In contrast, in the Czech crisis of 1938 German mobilization of ground and air forces for an attack against Czechoslovakia was countered only by precautionary naval measures on the part of the British and, as a result, Britain was not in a position to defend Czechoslovakia if it was attacked. A tit-for-tat policy would have required that

the British expeditionary force be sent to France and then be deployed and mobilized to carry out with French forces a counteroffensive along the Franco-German border (Huth n.d., chap. 5).

### Data Analysis

The hypotheses formulated for empirical testing represent a mixture of complementary as well as competing explanations for the success or failure of deterrence. For example, hypotheses on the military balance and strategies of military escalation supplement each other by focusing both on the capabilities of the defender to protect the protégé and on indicators of the defender's intention to employ those capabilities if necessary. At the same time, a number of different factors can influence the potential attacker's estimate of the defender's intention to use force. For example, is the defender's reputation for standing firm or backing down under the threat of armed conflict more important than the specific interests at stake in determining intentions or are the immediate diplomatic and military actions of the defender even more critical in signaling intentions?

The model specified for empirical testing included four variables: the military balance, the value of the protégé to the defender, the bargaining behavior of the defender, and the past behavior of the defender. These four sets of variables represent two fundamental components in the potential attacker's calculus of deterrence: (1) the military capabilities of the defender to protect the protégé and (2) the likely intention of the defender to use force to protect the protégé. Probit analysis was utilized to determine which of the alternative indicators for each set of variables provided the greatest degree of relative explanatory power. The hypotheses as formulated in the preceding section represent the expected results of the

empirical testing. Multiple equations were then tested with at least one indicator from each set of variables included in each equation. Those indicators that did not reach the 95% level of significance were deleted from the equation. In order to recheck the final results of the probit analysis each of the previously deleted indicators was reentered individually into the final equation and tested to make sure that the indicator remained insignificant. Close attention was directed to the potential problem of multicollinearity among the indicators of the independent variables. Auxiliary r-square values for the indicators in each equation tested were checked and multicollinearity was a problem with auxiliary r-square values below .3 in almost no cases.

The final equation is presented in Table 1. Overall, the results of the probit analysis indicate generally strong support for the hypotheses as formulated. The principal findings can be summarized as follows:

1. The balance of forces had an important impact on deterrence outcomes. As Hypothesis 1 predicted, the immediate and short-term balance of forces had a positive impact on the probability of deterrence success, whereas the long-term balance of forces had no significant impact. The most effective military deterrent seems to be the capacity of the defender to repulse an attack and deny the adversary its military objectives from the very outset to early stages of an armed confrontation. Conventional deterrence based on a denial capability is much more effective than the threat of punishment in a protracted armed conflict. In addition, the possession of nuclear weapons by the defender (Hypothesis 2) did not have a significant impact on deterrence outcomes when the target was a non-nuclear power.
2. Strategies of crisis bargaining play an

Table 1. Probit Analysis of Deterrence Outcomes

Explanatory Variable	Estimated Coefficient	Standard Error
Constant	-1.67	—
Military balance		
Immediate balance of forces	.55	.28*
Short-term balance of forces	.83	.36**
Bargaining behavior		
Firm-but-flexible diplomacy	.97	.45**
Tit-for-tat military escalation	.98	.46**
Reputation from past behavior <sup>a</sup>		
Defender backs down	-1.15	.64*
Defender intransigence	-.86	.49*

Note: Percentage of predictions correct = 84; number of cases = 58.

<sup>a</sup>Those cases in which the defender and potential attacker were adversaries in a previous confrontation.

\*Significant at the 95% confidence level, one-tailed test.

\*\*Significant at the 97.5% confidence level, one-tailed test.

important role in the success or failure of deterrence. As argued in Hypotheses 6 and 7, the adoption of a policy of tit for tat in military escalation and a firm-but-flexible diplomatic strategy are the most effective bargaining strategies in contributing to deterrence success. These results indicate that bargaining behavior based on a policy of reciprocity can signal to the potential attacker the defender's determination to protect the protégé, without provoking the potential attacker. These results support a growing body of experimental and comparative historical research which concludes that reciprocity is a robust strategy for inducing cooperation among adversaries (Axelrod 1984; Gochman and Leng 1983; Leng 1984; Leng and Gochman 1982; Patchen 1987).

3. Consistent with Hypothesis 8, the past behavior of the defender in the most recent confrontation with the potential attacker had a significant effect on deterrence outcomes; whereas the past behavior of the defender in deterrence cases with other states did not. Hypothesis 9 was partially supported

by the empirical evidence: a record of past conciliation and of backing down under pressure or of bullying by the defender reduced the likelihood of deterrence success. A past outcome of firm-but-flexible bargaining and stalemate between defender and potential attacker, however, did not increase the probability of deterrence success, contrary to the expectations of Hypothesis 9.

4. The only set of hypotheses not supported by empirical testing were those linking high levels of political-military and economic ties between defender and protégé to deterrence success (3-5). The reason would seem to be the strong impact of bargaining behavior on influencing the potential attacker's estimate of defender intentions and interests at stake. High levels of arms transfers and foreign trade between defender and protégé represent an indirect basis for estimating the defender's interests at stake. The bargaining behavior of the defender, however, provides a more immediate and direct indicator of the defender's intent to support the protégé, which the

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**Table 2. The Impact of the Balance of Military Forces on Deterrence**

Balance of Military Forces	Change in Ratio of Defender-Protégé Forces to Potential Attacker Forces	Percentage Change in Probability of Successful Deterrence
Immediate	.25- .50	5
	.50- .75	5
	.75-1.00	6
	1.00-1.50	9
	1.50-2.00	8
	2.00-3.00	10
	3.00-5.00	6
Short-term	.25- .50	6
	.50- .75	5
	.75-1.00	4
	1.00-1.50	7
	1.50-2.00	6
	2.00-3.00	8
	3.00-5.00	9

Note: The marginal impact of each variable is calculated by changing its value while holding all other variables in the model (Table 1) at their mean value. The change in the location on the cumulative standard normal distribution is then converted into the percentage change in the probability of successful deterrence.

potential attacker is likely to infer reflects the importance of the interests at stake for the defender. Alliance ties have little independent deterrent impact, it would seem, since the importance of the alliance for the potential attacker rests largely on the immediate and short-term military contribution of the defender to the protégé, which is reflected in the balance of forces. The long-term political and military costs to the defender of not honoring an alliance commitment do not seem to be that salient to the potential attacker.

In order to help interpret the results in Table 1 additional tables are presented that report the marginal impact of changes in each variable while holding all other variables in the equation at their mean value. In Table 2 the impact of changes in the immediate and short-term balance of forces is reported. For example, a change in the immediate balance of forces from a one-to-four disadvantage to an equal balance of forces for the defender increases the probability of deterrence

success by 16%; while the change from an equal balance to a three-to-one advantage for the defender increases the likelihood of deterrence success by 27%. Similar results are obtained when the short-term balance of forces is examined.

The results presented in Table 3 indicate that crisis bargaining plays a critical role in determining the success or failure of deterrence. These results lend strong support to the argument that despite the potential problems of misperception and cognitive biases (Jervis, Lebow, and Stein 1985, 183, 204-11; Lebow 1981, 277) in a crisis or confrontation, policymakers can still effectively signal and clarify intentions by means of diplomatic and military actions. For example, the adoption of a firm-but-flexible diplomatic strategy increased the probability of deterrence success by 32%. A strategy of tit for tat in responding to the military escalation of the potential attacker increased the chances of success by 33%.

The defender's past behavior also plays a critical role in deterrence outcomes. If the defender was forced by the potential

**Table 3. The Impact of Bargaining Strategies and Past Behavior on Deterrence**

Strategies and Behavior	Percentage Change in Probability of Successful Deterrence
Bargaining strategy	
Firm-but-flexible diplomacy <sup>a</sup>	+32
Defender military escalation	
Tit for tat <sup>b</sup>	+33
Past confrontation outcome	
Defender backs down <sup>c</sup>	-42
Defender intransigence or putdown <sup>c</sup>	-32

Note: The marginal impact of each variable is calculated by changing its value while holding all other variables in the model (Table 1) at their mean value. The change in the location on the cumulative standard normal distribution is then converted into the percentage change in the probability of successful deterrence.

<sup>a</sup>As opposed to a bullying or conciliatory strategy.

<sup>b</sup>As opposed to exceeding or failing to match escalation of the potential attacker.

<sup>c</sup>As opposed to stalemate or no past confrontation.

attacker to concede to its demands under military pressure in a previous confrontation, then the deterrent threats and actions of the defender lacked credibility in the next dispute between the two states. As the results in the bottom third of Table 3 indicate, if the defender has backed down, the probability of deterrence success decreases by 42%. A reputation for unyielding firmness and bullying, however, is not to the advantage of the defender (just as it is not an effective bargaining strategy for immediate deterrence). If the defender adopted a bullying strategy (diplomatic put-down) in the last encounter with the potential attacker the probability of deterrence success also decreased—by 32%. The reason seems to be that having been bullied previously, the potential attacker was less likely to retreat in the future and suffer further damage to its bargaining reputation. For example, the decision by Russia to go to war in defense of Serbia in 1914 was strongly influenced by the diplomatic setbacks that Russia suffered at the hands of Germany in previous confrontations in 1909 and 1912-13 (Huth n.d., chap. 6).

## Conclusion

The defense of allies from external threats is an enduring feature of the competition between states for spheres of influence in the international system. Extended deterrence, however, can be a very difficult and demanding task for foreign policy leaders. In this study we have found that a potential attacker's calculus of military costs seems to focus on whether military force can be used to achieve decisive results at relatively low cost. Extended deterrence is greatly strengthened, therefore, if the defender has the military capability to deny the attacker a quick victory and turn the conflict into a protracted struggle. While a number of recent case studies have questioned whether the military balance is a powerful explanatory variable in the analysis of deterrence outcomes (Jervis, Lebow, and Stein 1985, 215-16; Stein 1987, 348, 351), the results of this study strongly suggest that the balance of forces is an important variable in a potential attacker's calculus of deterrence.

Uncertainty in estimating the intentions



of the defender in a situation of extended deterrence is a critical problem confronting a potential attacker. One way in which the defender can signal its intentions and reduce uncertainty is through the bargaining process. A tit-for-tat policy of military escalation not only signals the defender's willingness to use force if necessary, it also has an important impact on the immediate balance of forces. The movement and positioning of military forces by the defender in many cases of extended deterrence erased a local imbalance of forces in favor of the potential attacker and established a formidable tripwire deterrent capability in which the use of force by the potential attacker would necessarily result in the direct engagement of the defender's armed forces. As a result, the ability of the potential attacker to achieve quick and decisive results with the use of force was greatly reduced and the military costs increased (Huth n.d., chap. 5).

Deterrence success, however, is not only a question of the defender possessing and demonstrating the intent to use powerful military forces. The critical role that a firm-but-flexible diplomatic strategy played in deterrence success strongly suggests that the political costs of inaction are also an important factor in the potential attacker's calculus of gains and losses (also see Russett 1967; Jervis, Lebow, and Stein 1985, chaps. 3, 5, 9). The emphasis in deterrence theory has been on the requirements of the defender to possess strong military forces and to demonstrate the intent to use force. It is important to recognize, however, that estimates of only the military costs of using force provide a partial and incomplete explanation of deterrence outcomes in a number of cases. The political costs for the potential attacker of not pressing ahead with the

use of force, defined in terms of bargaining reputation, need to be recognized as central to successful crisis management. By combining firmness with conditional proposals of limited compromise, the immediate confrontation can be deescalated and the potential attacker can retreat from its threat to use force without reinforcing the appearance of capitulation due to coercive pressure. The credible threat of military force is required to convince the potential attacker that the military costs of changing the status quo are high, but skillful diplomacy is also required to assure the potential attacker that the political costs of continued peace are acceptable.

### Appendix

The fifty-eight cases are listed in Table A-1.

The following criteria were utilized in selecting them: First, the initial threat of force by the potential attacker has to be explicit, and the target of the threat clearly identifiable. Coercive actions by the potential attacker included:

1. verbal threat or warning that force would be used
2. reinforcement and movement of military forces to positions near the border or off the coast of the protégé.
3. concentration and alerting of military forces on the border or off the coast of the protégé

Second, the threat of military retaliation by the defender had to be clearly directed at, and perceived by, the potential attacker. Deterrent actions by the defender included the same actions 1, 2, and 3, as directed at the potential attacker (adopted from Gochman and Maoz 1984, 587-89).

**Table A-1. Cases of Attempted Extended-Immediate Deterrence, 1885-1984**

Case	Year	Potential Attacker	Protégé	Defender	Outcome <sup>a</sup>
1	1885	Russia	Afghanistan	Britain, India	S
2	1885-86	Bulgaria	Serbia	Austria-Hungary	S
3	1886	Greece	Turkey	Britain	S
4	1894	Japan	Korea	China	F
5	1897	Greece	Crete	Britain, Turkey	S
6	1898	France	Egyptian Sudan	Britain	S
7	1902-3	Germany	Venezuela	United States	S
8	1903-4	Japan	Korea	Russia	F
9	1903-4	Colombia	Panama	United States	S
10	1905-6	France	Morocco	Germany	F
11	1905-6	Germany	France	Britain	S
12	1906	Turkey	Egypt	Britain	S
13	1908-9	Serbia, Russia	Austria-Hungary	Germany	S
14	1911	Italy	Tripoli	Turkey	F
15	1911	France	Morocco	Germany	F
16	1911	Germany	France	Britain	S
17	1912-13	Serbia, Russia	Austria-Hungary	Germany	S
18	1913	Romania	Bulgaria	Russia	S
19	1913	Bulgaria	Greece	Serbia	F
20	1913	Serbia	Albania	Austria-Hungary	S
21	1914	Austria, Germany	Serbia	Russia	F
22	1914	Russia, Serbia	Austria-Hungary	Germany	F
23	1914	Germany	France	Britain, Russia	F
24	1914	Germany	Belgium	Britain	F
25	1921	Panama	Costa Rica	United States	S
26	1922	Turkey	Greece	Britain	S
27	1935	Italy	Ethiopia	Britain	F
28	1935-36	Japan	Outer Mongolia	Soviet Union	S
29	1937	Soviet Union	Manchukuo	Japan	S
30	1938	Soviet Union	Manchukuo	Japan	F
31	1938	Germany	Czechoslovakia	Britain, France	F
32	1938-39	Italy	Tunisia	France, Britain	S
33	1939	Germany	Poland	Britain, France	F
34	1940-41	Soviet Union	Finland	Germany	S
35	1946	Soviet Union	Iran	United States	S
36	1946	Soviet Union	Turkey	United States	S
37	1948	Soviet Union	West Berlin	United States, Britain	S
38	1950	China	Taiwan	United States	S
39	1950	United States	North Korea	China	F
40	1954-55	China	Quemoy-Matsu	United States	S
41	1957	Turkey	Syria	Soviet Union	S
42	1961	Iraq	Kuwait	Britain	S
43	1961	North Vietnam	Laos	United States	S
44	1961	India	Goa	Portugal	F
45	1961-62	Indonesia	West Irian	Netherlands	F
46	1964-65	Indonesia	Malaysia	Britain	F
47	1964-65	North Vietnam	South Vietnam	United States	F
48	1964-65	United States	North Vietnam	China	S
49	1967	Israel	Syria	Egypt	F
50	1967	Turkey	Cyprus	Greece	F
51	1970	Syria	Jordan	Israel	S
52	1971	India	Pakistani Kashmir	China	S
53	1974	Turkey	Cyprus	Greece	F
54	1975	Morocco	Western Sahara	Spain	F

## Extended Deterrence

Table A-1 (continued)

Case	Year	Potential Attacker	Protégé	Defender	Outcome <sup>a</sup>
55	1975	Guatemala	Belize	Britain	S
56	1977	Guatemala	Belize	Britain	S
57	1979	China	Vietnam	Soviet Union	F
58	1983	Libya	Chad	France	S

<sup>a</sup>S = successful deterrence, F = failure of deterrence.

*Successful* immediate deterrence is defined as those cases in which the potential attacker does not attack the protégé or engages in only the limited use of force against the protégé and fails to force the defender to capitulate to its demands under the threat of force and escalation to full-scale war. To conclude that deterrence has succeeded can be difficult when there is a lack of documentary evidence. Diplomatic historians and country experts were consulted, as were secondary sources in order to distinguish between bluffs and actual threats to use force. In some cases, as well, a number of factors were likely to have contributed to the decision by the potential attacker to not use force; but by drawing on expert advice and secondary analyses I gained reasonable confidence that the defender's deterrent actions played a central role in that decision. Although the coding of deterrence success in some cases cannot be established definitively, it is essential to have cases of success and failure to compare if hypotheses and propositions on deterrence are to be tested systematically.

There were a total of six cases (nos. 5, 7, 28, 43, 51, and 58) of successful deterrence in which the attacker engaged in the use of limited force against the protégé. In five cases the combined total of battle fatalities between the regular armed forces of protégé and attacker was less than one hundred and in one case (no. 51) fatalities were between one hundred and two hundred. In all of the cases the attacker failed to achieve its primary political-military objectives and deescalated the use of force

in response to the threatened intervention of the defender. The critical distinction between extended-immediate deterrence success and failure is whether the attacker decides to commit its armed forces to sustained combat against the protégé and defender in pursuit of its foreign policy objectives. As a result, the limited use of force by the attacker is not a failure of immediate deterrence if the attacker steps back from the escalation that is considered necessary to attain those objectives. A clear-cut distinction between any use of force and no use of force as the dividing line between the success or failure of immediate deterrence is misplaced since the critical question for the attacker is whether to take the necessary military steps to achieve its goals or to try and settle for some form of a negotiated settlement. Deterrence *failure* is therefore defined as those cases in which the attacker commits its armed forces to sustained combat against the protégé and defender or in which the defender conceded to the principal demands of the attacker under the threat of armed conflict.

## Notes

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1. The cases listed in the Appendix represent an extended and revised version of the data set that was used in an earlier study (Huth and Russett 1984).

2. Strategic surprise occurs when a defender fails to react to intelligence information and does not undertake precautionary military preparations for defense. Tactical surprise occurs when the defender receives warning of an attack and initiates military preparations but has insufficient time to mobilize fully and position forces for optimal defense.

3. The response of states that are the target of an attack is a very different question. To defend their own country's territory policymakers are very likely to resist an attack even if a protracted war of attrition is required to defeat or at least punish the aggressor. This does not seem to hold true for the defense of allies (see Huth and Russett n.d.).

4. Conceptually, high levels of private foreign investment by the defender in the protégé's economy would also be a good indicator of economic interests at stake. Data on foreign investment, however, are sketchy and unavailable for many cases in the period prior to World War II.

5. There is very little systematic empirical work that tests the impact of past behavior. The work of Snyder and Diesing (1977, 187-88) and Huth and Russett (1984, 517) suggest that past behavior is not very important. Blechman and Kaplan (1978, 110-11), however, find some relation between the previous use of force by the United States in a region and positive outcomes in the short term.

6. The short-term balance of forces is adjusted to reflect the impact of distance between defender and protégé and potential attacker and protégé if necessary. The loss-of-strength gradient is taken from Bueno de Mesquita (1981, 105).

7. Data for this indicator were obtained from the Correlates of War National Capabilities Data Set. The operationalization of the long-term balance-of-forces indicator fails to capture the political capacity dimension and as a result the indicator assumes that both sides will be able to mobilize available economic and manpower resources. This measurement error produces a bias toward overestimating the capabilities of the defender, which would enhance the deterrent value of the long-term balance-of-forces indicator.

8. The deterrent value of nuclear weapons could also be tested by examining cases in which explicit nuclear threats—verbal or the alerting or movement of nuclear capable forces—occurred. There are, however, only two cases in this study of such explicit threats—Berlin, 1948 and Quemoy-Matsu, 1955—and therefore such a test is not attempted.

9. On a random sample of twenty cases the

intercoder reliability score was  $r = .96$ .

10. The intercoder reliability score on a random sample of twenty cases was  $r = .91$ .

11. The intercoder reliability score was  $r = .88$  on a random sample of twenty cases.

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# REPUTATION AND HEGEMONIC STABILITY: A GAME-THEORETIC ANALYSIS

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*We develop and explicate a game-theoretic model in which repeated play, incomplete information, and reputation are major elements. A significant advance of this model is the way it represents cooperation under incomplete information among rational actors of different sizes. The model is used to formalize certain aspects of the "theory of hegemonic stability." It shows that the "dilemma" or "limits" of hegemonic stability look like natural attributes of games where reputation is involved, unifying both "benevolent" and "coercive" strands of hegemony theory. An example, drawn from recent developments in the Organization of Petroleum-exporting Countries, shows how our model of reputation guides the study of hegemonic regime construction. We conclude by comparing the nature of cooperative behavior under conditions of complete and incomplete information.*

*We* here address two closely related purposes. One is the development and explication of the strategic nature of "hegemonic" relations among rational nation-actors, using a game-theoretic model in which repeated play, incomplete information, and reputation are major elements.<sup>1</sup> The model itself has broader applicability. In general, it describes a "large" actor's ability to affect distributional outcomes in a situation where actors compete over shares at the same time that they cooperate to produce a good.

Our other purpose is to illustrate game theory as "applied theory," used to model features of situations and derive observable implications. We consider the collapse of oil prices in late 1985 as a game of

incomplete information. Within our model, central features of that episode include the different production capacities of Saudi Arabia and other oil producers, uncertainty about future world demand for oil, and certain internal costs known only to the Saudis. Our analysis shows that in hegemonic situations there can be instability, because reputational considerations predict that in equilibrium actors will follow "mixed strategies" of challenge and acquiescence, and that cooperation and information spreading will be severely limited.

The significant advance of our model is its ability to represent cooperative behavior when rational actors have asymmetric information and different capabilities. *Information* is asymmetrically dis-

tributed in that an actor may know its own true costs and payoffs better than others do. *Capabilities* differ to such an extent that, even if at cost to itself, only one "large" actor is able to punish (inflict costs on) others. Within this structure we show that there exists an equilibrium of cooperation under a large actor's reputation for toughness. This demonstration is important since analysts can view observable behavior corresponding to an equilibrium as something reasonable people could do and persist in doing.

Thus we bring together a number of previously separate perspectives. Our model accords a major role in international relations to reputation and threat, as any student of diplomacy would want. It stresses coercion, but treats regimes as chosen cooperative behavior. Most important, it shows what is gained by taking a more complex model from the realm of economics and applying it to a developing area of theory in political science, the (hitherto informal) "theory of hegemonic stability." In fact, many of the insights of hegemony theory turn out to be features of a repeated game of incomplete information. In formalizing this theory, our model shows that the "dilemma" (Stein 1984) or "limits" (Snidal 1985a) of hegemonic stability can be reinterpreted as natural attributes of games involving reputation. Snidal's distinction between "benevolent" and "coercive" strands of hegemony theory is thus subsumed within a unified model. This offers hope of advancing the promise that Ruggie (1985) ascribes to game theory that "both conflict and cooperation can be explained by a single logical apparatus."

This accommodation of the central concerns of hegemony theorists (unequal resources) and game theorists (strategic interaction) proceeds as follows. The first section introduces the theory of hegemonic stability and shows how certain features of the hegemonic situation—particularly asymmetric distributions

among actors of information and other power resources—lie largely outside existing game-theoretic analyses of international regimes. The second section describes games of incomplete information and shows how reputation addresses problems of hegemony. It then develops a model of reputation, demonstrates that it produces a Bayesian equilibrium under mixed strategies, and describes possible extensions of the model. The third section uses the example of the recent collapse of oil prices and subsequent behavior of the nations in the Organization of Petroleum-exporting Countries (OPEC) to elaborate on the model. The conclusion provides further examples, stresses desirable extensions, and characterizes differences in effective strategic choice and cooperation between hegemonic and nonhegemonic situations.

### **Hegemonic Stability, Uncertainty, and Reputation**

#### **Regime Analysis, Game Theory, and Hegemony**

The interest in game theory among students of international relations is closely connected with the analysis of regimes. Regimes are agreed-upon, semivoluntary, ongoing, long-run joint maximizing strategies that induce seemingly selfless behavior. Broader than formal agreements, regimes include principles (stating purposes), norms (general injunctions or definitions of legitimacy), rules (specific rights and obligations), and procedures (formal indications of means rather than substance). Regimes prescribe actions for members and persist when members follow these prescriptions. Examples include cartels, trade agreements, financial systems, and the like.

Game theorists have sought to interpret regimes as cooperative solutions to repeated collective action games. Rational

actors (that is, actors who maximize long-run net benefits and choose courses of action based on their own preferences and expectations of how others will behave) under appropriate circumstances have sufficient incentives to cooperate with each other. Among such actors, structured, regimelike interactions would appear and persist. Whether cooperation ensues in such games depends on the structure of payoffs, the probability of repeated interaction, and the number of players (Oye 1985).

Hegemonic stability theory holds that spontaneous cooperation in these repeated games seldom occurs and that stable regimes depend instead upon the fostering actions of a dominant state. The hegemonic state provides an international order that furthers its own self-interest, although the resulting cooperation may work to the advantage of other states as well (Snidal 1985a, 587). This theory is applied to such diverse cases as Dutch-led world trade in the seventeenth and eighteenth centuries and British trading regimes in the nineteenth century by Gilpin (1981), Keohane (1981, 1984), Kindleberger (1973, 1981), Krasner (1976), and Stein (1984). A small part of the conflict between the hegemonic stability theory and game theory of regimes reflects a debate over the effects of large numbers on the robustness of cooperation (Oye 1985, 15). More importantly, hegemony theory typically stresses a variety of features—size asymmetries, distributional conflict, costly coercion, learning and uncertainty, and instability—that have proved difficult to model in game-theoretic analyses of regimes. Although some formalization of strategic elements of hegemony is desirable, the game theory of international relations could profit by incorporating important insights of hegemony theorists. Our incomplete information analysis of hegemony is formulated to reflect these insights, which we now elaborate.

### Significant Features of Hegemony Theory

*Asymmetry of Size.* The central feature of hegemony theory is that the hegemon is “big” relative to others. Substantively, it may be big in markets, big in capital, big in resources, or big in military power (Keohane 1984). Theoretically, it has a size advantage. Unfortunately, the game-theoretic analysis of regimes has been restricted to equal-actor, complete information games, which impose symmetry on issues, for instance, “by treating a very large state and very small ones as equal partners in a Prisoners’ Dilemma” (Snidal 1985b, 47). Our game-theoretic analysis begins by assuming the hegemon’s size advantage, which we treat as superiority in the possession of information and in the ability to inflict costs on others.<sup>2</sup>

*Distributional Conflict.* Hegemony theorists repeatedly observe that while all hegemonic regimes contain a big actor and many small actors, sometimes the big actor appears to get a good deal (Snidal’s “coercive” strand, Kindleberger’s “dominance”), at other times the small ones do (“benevolent” strand and “leadership”). This is because hegemonic stability theories, like other political economy efforts to model the role of enforcement in assisting voluntary cooperation through contracting, seek to explain the sharing of any generated surplus or gains from cooperating. Such possible “rent-sharing” arrangements between a “big” enforcer (a “hegemon”) and many small others (the “allies”) form a continuum. One end we could call an *empire*, where the allies’ share is some level of net benefit below which they would resist or rebel, and the hegemon gets the remainder. The other is an *alliance*, in which the allies get most of the benefits (say, the provision of order) while the hegemon may hardly fare better than with no regime at all.

In between are the most interesting



situations, in which the gains from cooperation are shared. Here, the hegemon would always prefer to extract a larger contribution (net of any cost of extraction) from the allies, who (for any level of private benefit secured by the availability of order) in turn would always prefer to contribute less for the order itself. How good a deal each gets determines whether the outcome more closely resembles empire or alliance. The key is the hegemon's ability to force subordinate states to make contributions, which is based on its size advantage. Indeed, as Snidal (1985a, 588-90) points out, if the allies receive net benefits, "they may recognize hegemonic leadership as legitimate and so reinforce its performance and position."

*Coercion and Costs.* The hegemon's being recognized as legitimate is one way to reduce the cost of extracting contributions from the allies. This highlights the important role of the cost of coercion in the theory of hegemony.<sup>3</sup> The cost of coercion depends on what is done to coerce. In trade, if the hegemon enters an ally's markets competitively, it may have to bear the cost of subsidizing its own exports. In defense, withdrawal of military protection forces the hegemon to bear the costs of finding alternate sites and arrangements for refueling, training, and staging of exercises. Effective substitutes may not be available. Our model brings out clearly the way in which the *cost of punishing* is the key to the hegemon's *strength*. We distinguish between "strong" hegemonies, who coerce at low cost, and "weak" hegemonies, who coerce at high cost but might in appropriate situations nevertheless benefit from coercion. Both sorts are distinct from allies, who are assumed incapable of coercing profitably.

*Reputation and Uncertainty.* But how does a hegemon get to be "legitimate" and thus collect contributions without actually carrying out costly coercive actions?

One way is for a weaker hegemon to pretend to be stronger by "investing in reputation": bearing some initial cost to establish a reputation for "toughness" which subsequently (if believed) allows cheaper coercion. Indeed, this reputation seeking appears to be a common feature of hegemony, whether in military matters, tariff wars (Stein 1984, 371), or finance (Lipson 1985). Naturally, strategic misrepresentation can backfire when bluffs are called and future credibility requires carrying out costly threats (Downs, Rocke, and Siverson 1985, 133; Ward 1987). Our model formulates more precisely some reputation-seeking strategies and the conditions under which they are rational.

*Hegemonic Decline.* Finally, a regular feature of hegemonic regimes is their propensity to decline, a degree of instability remarkable in something theorized about for its stability. But there doesn't seem to be much doubt about why they decline. Oye (1985, 5) cites a study by McKeown that shows how unanticipated downturns in the business cycle alter the benefits of open trade policies, and Stein (1984, 349) attributes instability to differential rates of economic growth and rapidly changing markets. But a hegemonic regime founded on reputation contains its own source of instability, since reputation requires some uncertainty about future costs of coercion. Repeated interaction may allow allies to learn more about the hegemon's nature, eroding the reputational basis of an existing regime. Of course, costs in turn may also change with unanticipated changes in a wider world (of trade, or whatever) beyond the control of the hegemon or regime, producing the appearance of instability.

*Summary.* We identify a regime with equilibrium behavior in which actors maintain cooperation. Cooperation need not entail hegemony, and thus Keohane (1984) can be right in claiming that hegemonic stability does not explain the

Bretton Woods regime. Snidal (1985a) is also right to say that declining hegemony may be replaced by either cooperation or conflict. A formal model of hegemony should address this aspect of the problem and also have room for costly coercion, learning, uncertainty, and decline. We believe that strategic reputation building is the key to models of hegemonic stability. We shall see below (1) how the theory of games of incomplete information can address all these points; (2) how asymmetric information coupled with costly coercion provides possible incentives to invest in a reputation for toughness; and (3) how reputation in turn can produce regimes that look more or less like empires or alliances but that contain within them the seeds of their own unraveling.

### The Game Theory of Reputation and Leadership

#### Games of Incomplete Information

Our model is a repeated game of asymmetric incomplete information. Most game-theoretic applications in international relations (and in political science generally) have employed games of complete information; that is, the strategies available to all players and the resulting payoffs for all players are assumed to be common knowledge. Phenomena incompatible with these assumptions, such as commitment, bluffing, and reputation, are usually described as "situational features" not analyzable within the game-theoretic model.<sup>4</sup> For subjects like regime stability, in which incomplete information and reputation may be central, such analyses become informal just at the point where game-theoretic rigor should, ideally, play its most important role: the analysis of complex strategic interaction.

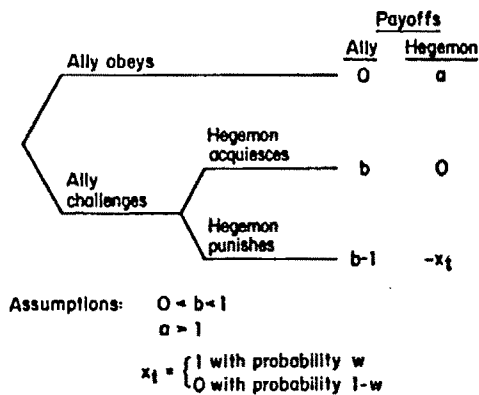
Game theorists, beginning with Harsanyi (1967-68), have devised tools to bring these phenomena under the same set

of modeling assumptions used in more traditional game-theoretic approaches. In games of incomplete and asymmetric information, the players do not know the entire structure of payoffs. Indeed, players have *different* information. Each, for example, may know only its *own* payoffs. With *repeated interaction*, these information conditions are precisely what create the possibility of reputation, as well as learning and adjustment (Snidal 1985b; Wagner 1983).

Consider, for example, the "empire" pole of the continuum of hegemonic arrangements. Here, a coercive hegemon exploits allied states by forcing behavior costly to them and allowing them minimal benefits in return. Coercion, involving military force or economic sanctions, may be costly to the hegemon as well. The costs to the hegemon will vary from one situation to another, and may not be precisely known to the vassal states. As a result, the hegemon can sometimes threaten punishments whose true costs, if carried out, would outweigh the expected benefits from the coerced behavior. If the allies' uncertainty means that the threats do not always have to be carried out, the results may profit the hegemon, which builds a reputation for being more able to punish than it actually is.

At the opposite pole, that of "alliance" hegemony, reputation is equally important. Suppose first that several nations would be nearly equal beneficiaries from providing a collective good but that one of them (the hegemon) is able to provide selective incentives, perhaps through military or economic actions unrelated to this collective good. These incentives are costly to the hegemon; and, again, the costs to the hegemon vary and are not perfectly known to the hegemon's allies. The allies know their net benefits from both the collective and selective goods and know that the hegemon has an incentive to mislead them by misrepresenting its costs. They are tempted to challenge the regime, that

Figure 1. The Hegemony Game



is, to dump some steel, reduce an exchange rate, reduce defense expenditures. But the hegemon can gain benefits for itself (and the allies) by occasionally applying selective incentives, even if costly, masking its true pattern of costs and counteracting their temptations to challenge.

In either of these settings, or any in between, we have a hegemon threatening other states with punishment for not contributing. Uncertainty about the cost of those punishments presents the hegemon with a chance to maximize its benefits by threatening, promising, sometimes punishing or rewarding, creating a reputation for willingness to punish. The most successful, stable regimes of this sort will be the ones in which the hegemon optimizes its use of these costly threats to create the desired reputation. Of course, if a hegemon could almost always punish without cost, regime maintenance would be easy and stable hegemony common. The assumption underlying our approach is that hegemon must typically depend on *costly* punishment. Thus by focusing on reputation building, we analyze decision making at the margin that is most relevant to regime stability.

### A Game of Reputation under Incomplete Information

In this section we examine a purely game-theoretic approach to the problem of reputation building. The model allows us to derive the choices, deceptions, guesses, and learning processes of rational players in a setting of incomplete information. It yields a sequential equilibrium, which we exploit to analyze the nature of reputation and of rational reputation building.<sup>5</sup>

*Assumptions.* Our model assumes that in each of two periods, a hegemon faces a potential challenge from a single ally.<sup>6</sup> In period  $t$ , ally  $t$  can either *obey* (adhere to the behavior required in the regime or alliance) or *challenge* (go against the hegemon's wishes). If ally  $t$  challenges, the hegemon can either *punish* that ally or *acquiesce*. Ally 2 learns the outcome of period 1 (obey; challenge and acquiesce; or challenge and punish) before making its own choices in period 2. Figure 1 displays a single period of this game in extensive form.

As the figure shows, an ally who obeys receives zero. An ally who challenges receives  $b$  ( $0 < b < 1$ ). Punishment exacts a penalty of one, so an ally who challenges and is punished receives  $b - 1$ . Thus each ally prefers most to challenge and not be punished; challenging and being punished is the worst outcome. (The value of  $b$  could be assumed different for each ally without changing any of our results.)

In each period the hegemon receives a payoff of  $a$  ( $a > 1$ ) if the ally obeys, and zero if the ally challenges. If the hegemon chooses to punish a challenge, the cost of punishment in period  $t$  is  $x_t$ , where  $x_t$  is one (costly) with probability  $w$  and zero (cheap) with probability  $1 - w$ .<sup>7</sup> The hegemon learns the value of  $x_t$  at the beginning of period  $t$ , before either player makes a move in that period, but faces the problem in period 1 of choosing a rational

strategy for that period without knowing whether punishment will be costly in period 2. This is a key feature of our approach: unlike other models in which the hegemon is always strong or weak, here there is some uncertainty for the hegemon as well.

The value of  $w$  might be thought of as the "expected weakness" of the hegemon. The higher the value of  $w$ , the "weaker" the hegemon, that is, the more likely punishment is to be costly. We assume that the hegemon knows the true value of  $w$ —how weak he or she is on average—but not whether an individual punishment episode will be costly until that period arrives. Allies have an initial, subjective estimate of  $w$  summarized by a random variable  $W$ ; but they never learn the hegemon's true costs. As the game proceeds, allies can try to make inferences about  $w$  from observing the hegemon's interactions with other allies; but they only observe acquiescence and punishment, not whether punishment was costly.

So that the allies' Bayesian updating of their beliefs about  $w$  be relatively simple, we assume also that  $W$  has the beta distribution. Intuitively, their beliefs depend on two parameters  $\alpha$  and  $\beta$ , where  $\alpha$  counts costly punishments and  $\beta$  counts cheap ones;  $\alpha/(\alpha + \beta)$  is the ally's expected probability that the hegemon will face a costly punishment.<sup>8</sup> In equilibrium, by observing the hegemon's behavior in case of a challenge in period 1, ally 2 can update  $W$ , that is, modify  $\alpha$  and  $\beta$  using Bayes' rule, assuming that the hegemon follows its equilibrium strategy.

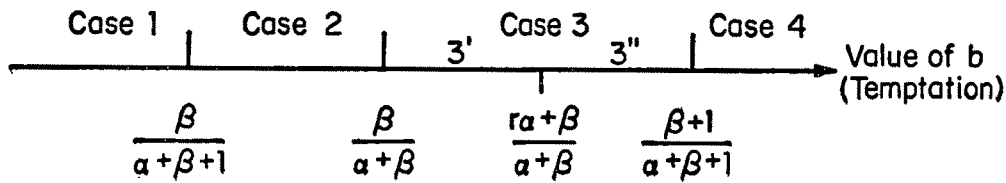
*Equilibrium.* The solution to such a game of incomplete information is called a Bayesian (Nash) equilibrium (Harsanyi 1967–1968), which is an internally consistent combination of actions and beliefs such that neither player would wish to change its actions unilaterally, *given* the assumed incompleteness of information. A Bayesian equilibrium for our game then

includes the following elements: (1) a move (obey or challenge) to be made by ally 1 that, given the hegemon's strategy and ally 1's beliefs about  $W$ , maximizes ally 1's expected payoff; (2) a rule for ally 2 for responding to the outcome of the first period, which maximizes ally 2's expected payoff given the hegemon's strategy and given ally 2's updated beliefs about  $W$ , using Bayes' rule to take into account the outcome of period 1; and (3) a strategy that maximizes expected payoff for the hegemon, given the allies' strategies, describing how the hegemon would respond in period 1 if ally 1 challenged and how to respond if ally 2 challenges, given the outcome of period 1, for *each* possible value of the hegemon's private information ( $w$ ,  $x_1$ , and  $x_2$ ). In this and the next section we describe such an equilibrium and discuss certain of its properties. Formal proofs of these results are outlined in the Appendix.

In this equilibrium, the hegemon always punishes in period  $t$  ( $t = 1$  or  $2$ ) if  $x_t = 0$  and never punishes in period 2 if  $x_2 = 1$ . The latter is required because there is nothing to be gained by punishing in the final period. (Whether there is ever such a "final period" in the real world is an issue to which we return subsequently.) Beyond this, each player's behavior depends upon the value of  $b$ , the allies' payoff, relative to the allies' beliefs as described by  $\alpha$  and  $\beta$ . In effect, this is a comparison between the *temptation* to challenge and the *danger* (i.e., the estimated probability together with the ally's confidence in that estimate) that the hegemon can punish costlessly.

To illustrate the reasoning behind this equilibrium, suppose for a moment that there were only one period of play and that the ally believed the hegemon would punish if this were cheap and not if costly. Then the ally would decide what to do by making an expected value calculation. It could challenge and get away with it (if punishment were costly), receiving  $b$  with probability  $\alpha/(\alpha + \beta)$ ; it could challenge

Figure 2. Temptation, Danger, and the Four Cases of Equilibrium Strategy



and be punished, receiving  $(b - 1)$  with probability  $\beta/(\alpha + \beta)$ ; or it could obey, receiving zero. Summing, the expected payoff from challenging is then  $b - \beta/(\alpha + \beta)$ ; so the ally in such a case would challenge whenever  $b > \beta/(\alpha + \beta)$ . Repeat play opens up the possibility of reputation and deterrence and so complicates the calculation. In fact, the equilibrium comprises four separate cases, illustrated in Figure 2.

*Case 1.*  $b \leq \beta/(\alpha + \beta + 1)$ . The allies' benefit from challenging is so small relative to the probability of costless punishment that they *never* challenge. Even if ally 1 did (irrationally) challenge, the hegemon would not need to punish in order to deter a rational challenge from ally 2. Formally, in equilibrium the hegemon never punishes when  $x_1 = 1$  and neither ally challenges.

*Case 2.*  $\beta/(\alpha + \beta + 1) < b \leq \beta/(\alpha + \beta)$ . Here the temptation is somewhat greater, but still ally 1 is afraid to challenge. With no revision in initial beliefs, therefore, ally 2 also obeys. However, if ally 1 had irrationally challenged and the hegemon had acquiesced, ally 2 would be willing to challenge.<sup>9</sup> To deter this, a hegemon always punishes a challenge from ally 1, even if costly.

*Case 3:*  $\beta/(\alpha + \beta) < b < (\beta + 1)/(\alpha + \beta + 1)$ . This is substantively the most interesting case, because here the hegemon must actively establish a reputation. If  $x_1 = 1$ , the hegemon punishes a challenge from ally 1 with probability  $r$ , where

$$r = \frac{\beta\{1 - [b(\alpha + \beta + 1) - \beta]\}}{\alpha[b(\alpha + \beta + 1) - \beta]} ;$$

that is, the hegemon pursues a mixed strategy. The allies' behavior depends upon which of the following two subcases applies:

*Case 3'.*  $b \leq (r\alpha + \beta)/(\alpha + \beta)$ . Ally 1 obeys. Here, ally 1 is deterred, not by the high probability of a costless punishment but by appreciation of the hegemon's willingness to engage in reputation building. However, ally 2, who will only be punished if it is costless, always challenges. The hegemon has not had a chance to build a reputation.

*Case 3''.*  $b > (r\alpha + \beta)/(\alpha + \beta)$ . Here, ally 1 challenges. If the hegemon acquiesces, then ally 2 also challenges. If ally 1 is punished, then ally 2 pursues a mixed strategy, obeying with probability  $1/\alpha$ . In other words, the punishment sometimes deters ally 2; the lower the hegemon's incentive to deter, the more often this deterrence is successful. Ally 2 has a suspicious nature.

*Case 4.*  $b \geq (\beta + 1)/(\alpha + \beta + 1)$ . Symmetrically with case 1, here the temptation is so high that the allies always challenge. Even if the hegemon punishes ally 1, ally 2 cannot be deterred, so the hegemon never applies costly punishments.

The strategies specified constitute a Bayesian equilibrium to the hegemony game. It is also a *sequential equilibrium* (Kreps and Wilson 1982a). Among other things, a sequential equilibrium is "sub-

### Hegemonic Decline and Other Results

In order to incorporate the hegemon's uncertainty about future costs, we were able to describe the equilibrium only for a two-period game. However, there are reasons to believe that important properties of our results would still hold in a game with more periods. Such an extension will offer significant insights into the relationship between reputation and the decline of hegemonic regimes.

*A Multiperiod Model?* Kreps and Wilson (1982a) modeled reputation building by a predatory monopolist in a setting similar to ours, allowing for an arbitrary finite number of repetitions. Our results resemble theirs closely, with two important differences. First, if the Kreps-Wilson monopolist ever once fails to punish, then the jig is up: all other players know for certain that the monopolist cannot really afford to punish and (as a result) will never do so again. In our model, a hegemon who fails to punish in period 1 may yet punish in period 2 if  $x_2 = 0$ . Second, our model, unlike theirs, exhibits situations in which the hegemon is so well off that it need not worry about reputation, as well as circumstances in which reputation building is hopeless. In other respects, the Kreps-Wilson results correspond closely to ours.<sup>11</sup> These similarities indicate that if our model could be extended to arbitrary finite length, it ought to exhibit behavior similar to the Kreps-Wilson model. For instance, depending on parameter values, the hegemon's reputation may endure for the entire game, or with the approach of the final period the allies may become bolder. If this happens, the hegemon may resist at first, temporarily reestablishing its reputation. In other cases, the hegemon may give up early, and endure challenges for the rest of the game.

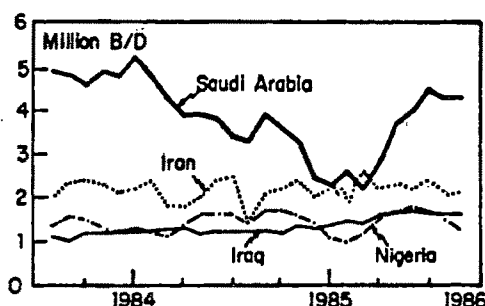
*Hegemonic Decline.* These results and extensions open up several ways in which

a hegemonic regime could decline after having at first succeeded. (We assume throughout what follows that the nations involved are in a situation corresponding to case 3".) First, the very fact of repeated play can reduce the possible scope of a hegemon for reputation building, even without allies having been initially wrong in their estimates of  $w$ . Repetition reduces the variance of  $W$  and increases their confidence in their estimates.<sup>12</sup> In this sense, a reputational model of a hegemonic regime contains within it possible sources of the regime's ultimate disintegration without requiring further arguments about unanticipated exogenous change.

Moreover, the use of mixed strategies by the players may bring about decline. In our two-period model, ally 2 may challenge even though the hegemon punished a challenge by ally 1. In a multiperiod model (or indeed the Kreps-Wilson model), the same scenario could unfold at any time. The hegemon may punish several times, establish a reputation that deters some challenges, then be (randomly) challenged, and (randomly) fail to punish, incurring further challenges from which it may be difficult or impossible to recover. As we have seen, equilibrium behavior requires mixed strategies; therefore a rational hegemon must leave itself open to this kind of hegemonic decline.

Finally, the onset of the late periods of the game may bring about the decline of the regime through the hegemon's loss of reputation. With no updating, ally 2 has a smaller chance of being punished than does ally 1, simply because only a costless punishment will be meted out in the final period. This makes ally 2 more willing to challenge, other things equal. This would be an ongoing process. Challenges become more and more attractive in the later periods, and under some conditions obedience breaks down several periods before the last. If there were anything resembling an "end of the game" in real life, this would provide an additional

Figure 3. OPEC Crude Oil Production



Note. We are grateful to J. L. Johnston, senior economist, Amoco Oil Corporation, for supplying this figure.

over 4 mbd. A year later OPEC was still producing at nearly the same output level, but Saudi production had been halved. By contrast, the combined production of Norway and Britain (not members of OPEC) had reached 3.5 mbd by 1985, of which half was exported. Their exports had been negligible (less than .5 mbd) five years before.

In December 1985, the Saudis achieved a majority decision in OPEC to abandon their joint price-stabilizing production quotas and threaten a price war with non-OPEC producers, particularly the North

Sea producers, Britain and Norway. Saudi oil production initially doubled to over 5 mbd in summer 1986, though following further OPEC negotiations it has since fallen closer to 3 mbd. There were scattered production cuts by other countries (both OPEC members and nonmembers but not Britain or, until recently, Norway), and the price of oil fell from above \$30 to below \$10 per barrel, recently fluctuating below \$20.

### Interpretations

What might the Saudis have wanted? Some (including, apparently, Saudi propagandists) claimed they sought to create structural economic changes to increase world demand for oil. This could be true, but only as a means to increase their own revenues. Others argue that they want to maximize market share (editorial, *Wall Street Journal*, 30 January 1986). Increasing market share plays a role in our account, too, but is not a motivation independent of price; for the Saudis alone can have the whole world market at two dollars a barrel for 15 years. Alternately, journalist Henry Jacoby ("A Shock That OPEC Won't Overcome," *New York Times*, 26 January 1986) interpreted the present situation as a "classic prisoners' dilemma," with OPEC and non-OPEC members able to benefit jointly from reduced production but unable to make binding agreements to forego production. Thus, he argued, the competitive outcome will be worse for all producers than continuing OPEC quota cooperation; and the squabbling over prices and production reflects their inability to enforce cooperation.

We believe instead that the episode represents an effort to maintain an oil producers' regime under Saudi hegemony. Hegemony theorists would argue that no stable oil producers' regime was possible unless backed by a "large" power. In market terms the Saudis are certainly the

Table 1. World Oil Production, 1980 and 1985

Source of Oil	Millions of Barrels per Day	
	1980	1985
Exports		
Saudi Arabia	10	3
Rest of OPEC	15	11
Non-OPEC	4	8
Total exports	29	22
Indigenous production	21	22
Total world demand	50	44

Note. We are grateful to J. L. Johnston, senior economist, Amoco Oil Corporation, for supplying figures from which these data were calculated.

largest oil-producing nation. However, their position as swing producer (accepting cuts in production to prop up prices) meant that OPEC in late 1985 closely resembled, in our terms, a hegemonic "alliance." Could Saudi Arabia use its superior reserve position to alter the distribution of benefits and restore its previous position within OPEC? This depends on whether it is willing to bear the short-term costs of very low oil prices in order to maximize revenues in the long run, and whether other nations (including those in OPEC) are convinced of this willingness. If so, the Saudi action is a credible threat and deters entry or excess production by some other countries after prices rise.<sup>14</sup>

First, the 1985 price fall is not an isolated episode. As Ahrari (1986) points out, the Saudis have acted similarly on two previous occasions, 1977 and 1981. On those occasions, he argues, their initiatives failed because market conditions remained tight. This is consistent with our overall stress on viewing the problem as one of repeated interaction, and highlights the importance of the hegemon's uncertainty about the price elasticity of world demand for oil.

Equally important, the Saudis can gain more from reputation than under other cooperative outcomes. For example, published estimates using conventional analysis of supply and demand showed that the Saudis could maximize their foreign earnings at about \$65 million per day by producing 6 mbd at \$15 per barrel, under conditions approximating competition without quotas (*New York Times*, 19 February 1986).<sup>15</sup> Other journalists and analysts of the *Wall Street Journal* argued that OPEC would eventually reestablish quotas and speculated that the Saudis would produce about 4 mbd at \$22 a barrel. (Above this price, apparently, the incentives for competitive entry by many small producers multiply rapidly.) This latter arrangement would give them earn-

ings of \$60 million per day. At this lower production level, their reserves would last longer. Indeed, given the scale of their reserves, the Saudis should prefer such an outcome to competition without quotas at discount rates for future earnings below about 3% per annum. Thus, the Saudis' better strategy might be to compete by producing 6 mbd, contrary to observed behavior, though possible data errors and uncertainty over discount rates make this a tentative conclusion.

However, this ignores the possibility that the Saudis could deter future exploration and production by establishing a reputation for toughness, which they could do in either case. They would have to withstand a long enough period of low oil prices to persuade other nations (or oil companies) to defer exploration and investment through the *threat* of future punishment (further periods of low prices) without actually having repeatedly to carry out the threat. It would be worth it to dissuade enough exploration and investment to add even as little as 10% to the long-run price of oil. With such an improvement, the Saudis might produce 6 mbd at \$17 per barrel under competition or 4 mbd at \$25 under quotas.<sup>16</sup> The extra \$10 million per day thus gained from reputation would exceed the likely difference (without reputation) between competition and quotas.

### Costs of Coercion

With their enormous low-cost reserves, the Saudis' ability to withstand periods of low prices provides the possibility of establishing a reputation for toughness from which they can subsequently profit—if they can bear the costs of driving prices down to get that reputation. Before the fact, the Saudis may have believed they could afford this strategy. Recently announced accounts show that the Saudis had a budget deficit of \$13 billion, or about \$36 million a day, while earning



about \$50 million per day from oil in 1986 (equal to the earnings on about 4.5 mbd at \$13 a barrel). Two points follow from this. First, this rate of earnings is \$10–\$15 million per day below the industry analysts' quoted estimates of what the Saudis could have made without establishing a reputation. However, such a shortfall sustained for 6–12 months could be made up by exploiting reputation in a couple of years. Certainly the decisions to reduce exploration and investment by others would come quickly, while the price benefits to the Saudis would last longer.<sup>17</sup>

Second, the principal cost of coercion to the Saudis is the domestic dislocation produced by budget deficits or low government revenues through the impact of cheap oil. The Saudis are unable to generate enough revenue with oil at \$10–\$15 per barrel to meet projected domestic expenditures, needing to produce 7 mbd to do so even at the upper end of this price range. In the short term, they would draw against their massive (\$100-billion) cash reserve built up in the flush years after 1978 while attempting to establish a reputation. In the long term, they apparently need to produce about 5 mbd at \$22 per barrel (on which they net nearly \$88 million a day) to balance the budget. Our calculations above show that this was unobtainable under any nonreputation strategy but just possible (perhaps after a year or two) at the price-quantity combinations arising under an established reputation for toughness. In any event, the Saudis twice announced "deferral" of a budget in 1986, announcements treated by some analysts as evidence of "weakness" (high cost of punishment). While there is no evidence of serious domestic unrest (elite or popular), Saudi planners privately stress that they expected demand for oil to rise faster at lower prices and thus the revenue impact to be smaller.<sup>18</sup>

### Success, Failure, and Mixed Strategies

Even with this difficulty, the Saudis have done relatively well so far. The August 1986 OPEC agreement accorded them an improved quota. A number of allies, both inside and outside OPEC, lined up production cuts. The speed with which they fell into line reflects the cost of oil production and desperation for foreign currencies. Gabon and Ecuador have extremely high production costs, Mexico is deep in debt, and Egypt imports food with oil earnings. These countries were among the first to cut production. Other higher-cost producers (Algeria, Qatar, Libya, Venezuela, and Nigeria) clamored for cuts to shore up prices. Those who lost significant revenues only when oil prices fell below \$15 a barrel, like Indonesia, Iran, Iraq, and the United Arab Emirates (*Wall Street Journal*, 11 February 1986), were less vocal.

The key "allies" were Britain and Norway, who neither cut production nor indicated willingness to discuss it. There the lower oil price meant foregone government revenues and possible public borrowing or increased taxes (both countries), reduced public investment (Norway), and a lower exchange rate (Britain). Both countries voiced similar claims. British spokesmen claimed that 90% of North Sea oil can be produced at five to eight dollars a barrel (*Wall Street Journal*, 11 February 1986). The Norwegians claimed that oil would flow at five dollars a barrel, conceding that exploration would stop at that price (*Wall Street Journal*, 24 January 1986). Later, they said that they would follow Britain's lead (*Wall Street Journal*, 11 February 1986). These claims, founded in fixed costs of production with no further recovery of old investment (*New York Times*, 7 February 1986), represent an effort to call a bluff. They are the real equivalent of our model's "challenge," in the belief that

Saudi production could not continue at low prices.

In fact Britain and Norway had less to lose in the short run than might have been expected. Norway's public investment loss will be felt slowly and was certainly anticipated by government planners. A lower exchange rate would find much support among British manufacturers. The Thatcher government would not want to raise domestic revenues in the runup to a general election but had two years left at the time.<sup>19</sup> The British also lacked control over their offshore oil industry and may well have felt that any cuts they made would have been too small to affect world prices. Of course, it is also possible that impressing other OPEC members was more important to the Saudis than any actual cuts obtained in North Sea production. Though the Saudis did relatively well in the August 1986 negotiations, they subsequently adopted a softer position, acquiescing in production cuts to help keep prices up. Within our model, this would represent continuing application of a mixed strategy, under the assumption that (domestic) costs of coercion had become too large to allow continued punishment. As we noted above, some evidence is available to support this interpretation.

In sum, potential Saudi gains from hegemony make their initiative comprehensible. In spite of early predictions that "the British will talk, they know the Saudis can force the price down" (*Wall Street Journal*, 6 February 1986), a British challenge was not necessarily irrational. Since equilibrium hegemonic regimes are possible and the Saudis have enormous low-cost reserves, they may succeed in establishing a hegemonic regime. If such a regime forms, the Saudis should do well relative to expectations based on perfect competition or alliance behavior. If the regime doesn't form now, the discussion of hegemony in the previous section suggests that the Saudis will try again. How-

ever, if a hegemonic regime does form as a "mixed strategy equilibrium," uncertainty about world trends will lead to sporadic challenges from allies (especially as conditions change) and possibly ultimate collapse.

### Conclusion

We analyzed a game-theoretic model containing many of the central features of hegemonic stability theory of regimes: asymmetry, distributional conflict, costly coercion, uncertainty, and instability. Our results depend crucially upon the assumptions that (1) there is incomplete information about the true abilities of the hegemon and (2) the hegemon sometimes faces costs in coercing others to cooperate. Hegemonic instability in the model is a natural consequence of uncertainty and mixed strategies, which render reputation both exploitable and challengeable. The model incorporates size differences among participants by virtue of asymmetric information and asymmetric abilities to coerce.

Although the model uses specific assumptions about the nature of uncertainty, it is important to notice how general in spirit these assumptions really are. The important fact is that the hegemon knows more about its own costs than allies know about the hegemon's costs. No actor, not even the hegemon, has perfect foresight about future costs or actions. We have employed fairly general distributional forms, and there is no reason to believe that these alone account for the qualitative results we derive. Our strongest assumption, really, is that the hegemon and allies are all completely cognizant of the information structure of the game, so that despite the gaps in their knowledge they can act strategically against one another. The result of this strategic interaction is expressed in our results on equilibrium in the hegemony

game. Since the equilibrium is *Bayesian*, we have accounted for rational learning by the players upon observing one another's actions. The expectations of each player are consistent with the actions of the other players. This equilibrium is *robust*, though not unique.

### Implications for Hegemonic Stability

Our results have several important implications for hegemonic stability theory. Most notably, although ours is a result about "stability" in the sense of equilibrium, it demonstrates exactly how and in what circumstances hegemonic regimes might break down or appear unstable. In situations like cases 1 and 2, stability is complete and lasting. In case 4 the hegemon gets no respect and punishes only when it is costless to do so. In a model with a more complex information and cost structure, such behavior might appear as intermittent cooperation by the allies, depending on the information they have about the hegemon's costs in specific situations. Indeed, one can extend this analysis to predictions of declining severity of punishments over time as a hegemon's reputation becomes better established, as well as predictions of the relative durability of hegemonic and non-hegemonic regimes.<sup>20</sup>

More interestingly, in case 3 the appearance of instability results from the incentive for a rational ally to challenge the hegemon. In addition, the hegemon punishes challenges using a mixed strategy; and the ally uses a mixed strategy to decide whether to respond to a previous punishment. Thus, even though the general costs and information parameters of the game remain constant, the equilibrium strategies call for purposely erratic behavior by hegemon and allies. Such behavior is necessary to keep one's opponents guessing and (for the hegemon) to take optimal advantage of the allies' learning process. The result is that the

regime will appear to follow a rocky road, with allies sometimes challenging the hegemon and at other times obeying and with the hegemon sometimes applying costly punishments and sometimes acquiescing. Equilibrium behavior in the hegemony game necessarily implies hegemonic instability.

It is particularly useful to realize that the behavior involved in this instability is in no way attributable to "mistakes" of any kind on the part of any of the actors. Only by contemplating mixed strategies can the actors be satisfied that their own plans are consistent with their expectations about their opponents. Allies occasionally challenge and endure punishments, and the hegemon occasionally neglects to punish, randomly but not irrationally or unexpectedly. The nature of hegemonic instability is thus predictable in the context of the game-theoretic model.

### Effective Choice in Hegemonic Relations

Several authors, notably Keohane (1984), have drawn an analogy between regimes and cooperation in the repeated two-player prisoner's dilemma game. But this game, even with added assumptions of incomplete information (Kreps et al. 1982), does not reflect the effects of size differences or asymmetric abilities to punish. Thus many of the results of our model of reputation stand in marked contrast to the behavior that Axelrod identifies as "effective choice" in the prisoner's dilemma (1984, chaps. 6, 9).

First, a successful prisoner's dilemma player should practice a "nice" strategy, that is, never be the first to defect. In contrast, a successful strategy for an ally in the reputation game must involve challenging the leader according to a mixed-strategy plan. Failure to do so allows the hegemon to take too much advantage of the allies by commanding their obedience, even when they ought to have the

best response for  $q$ , given  $r_1$ , again by maximizing ally 1's expected utility.

Step 3, calculating ally 2's best response functions, is a bit more involved. First, calculate  $p_2^O$ ,  $p_2^A$ , and  $p_2^P$  using  $r_1$ ,  $\alpha$ , and  $\beta$ . Notice that ally 2 must use the previous moves, along with  $r_1$ , to update the subjective distribution of  $W$  according to Bayes' rule. Next, for each outcome  $h \in \{O, A, P\}$  use the calculated values of  $p_2^h$  to write ally 2's expected utility from challenging with probability  $s_h$ . Finally, for each  $h$  maximize ally 2's expected utility with respect to  $s_h$  given  $r_1$ .

Finally, step 4 is to look at the four cases of the range of  $b$ , verifying in each case that the strategies given in the text simultaneously satisfy all the best response functions calculated in steps 1-3. This proves that the given strategies form a Bayesian equilibrium.

In proving that this Bayesian equilibrium is a sequential equilibrium, only case 2 causes a problem: if ally 1 unexpectedly challenges *and* if the hegemon unexpectedly acquiesces, then the denominator in Bayes' formula is undefined and we cannot calculate  $p_2^A$  and derive ally 2's best response. This is remedied by setting  $p_2^A = \beta/(\alpha + \beta + 1)$ ; such a belief is proved consistent by using the following sequence of strictly positive strategies for the hegemon: for each  $n = 1, 2, \dots$ , let  $r_1^n = 1 - 1/n^2$ , and let  $r_1^n = 1 - 1/n$ . (For definitions of these terms see Kreps and Wilson 1982b.)

There are other, more complicated equilibria in this game that also exhibit reputation building. All of them, however, involve mixed strategies. Alternative equilibria may arise in four ways: First, trivially, at the boundaries of cases 1, 2, and 4, some mixed-strategy equilibria may also arise due to the indifference of a player between two pure strategies. Second, there are "mirror-image" equilibria in which ally 2 sets  $s_A < s_P$  in cases 2 or 3, and the hegemon must acquiesce in order to "deter" ally 2. Such perverse

equilibria can be ruled out by putting conditions on the "reasonableness" of beliefs in equilibrium (see Calvert 1987).

Third, the hegemon is free to make  $r_1$  (or  $r_2$ , for that matter) depend upon  $w$  rather than being constant.<sup>23</sup> Such a strategy would require that both allies also use different strategies, even if the expected value of  $r_1$  is still  $r$ . To see why, consider ally 1's expected payoff when the probability of punishment, when costly, is  $q_1(w)$ :

$$E\{[1 - q_1(W)]Wb + [1 - W + Wq_1(W)](b - 1)\}.$$

Since  $E[Wq_1(W)] \neq EW \cdot E q_1(W)$ , the payoffs to ally 1's responses can be changed while keeping  $E q_1(W) = r$ . Similar reasoning applies to ally 2. This gives rise to a large set of alternative equilibria; however, all of them involve the use of mixed strategies by ally 2. Although the hegemon's strategy is technically pure, it looks to any observer, including the analyst and ally 2, like a mixed strategy since the true value of  $w$  is unobservable.

Fourth, since the hegemon is indifferent about applying a costless punishment in the final period, there are other equilibria in which  $r_2 < 1$ . We might imagine that, there being no real final period, the hegemon really would have a positive incentive to punish costlessly. This amounts to adding a (small) positive payoff  $\epsilon$  for such "costless" punishments. Such a payoff eliminates the possibility that  $r_2 < 1$ , but now, as it turns out,  $r_1$  is *forced* to depend on  $w$ . Equilibrium can still be derived using a similar proof, but the details are considerably more complicated (contrary to the conjecture in Calvert 1987).

## Notes

An earlier version of this paper was delivered at the 1986 annual meeting of the Midwest Political Science Association, Chicago. The authors benefited from the comments of David Baron, John Fere-



(sale price — production cost). Then in this scenario the Saudis will earn  $5 (= 6 - 1) \times \$13 (= 15 - 2)$ , or \$65 million per day.

16. Under competition with reputation the Saudis make \$75 million a day ( $5 \times \$15$ ). Under quotas with reputation they could make \$69 million—( $4 - 1) \times (\$25 - 2)$ , or  $3 \times 23$ . Again, since lower production lasts longer, we can treat the Saudis as indifferent between the present values of these two alternatives.

17. For instance, virtually all U.S. oil companies reduced capital spending for exploration drastically in 1986. Moreover, many decisions are irreversible. For example, the reopen price of many stripper wells that were profitable at \$30 per barrel is at least \$50 per barrel. Indeed, Sheik Yamani himself took the trouble to stress this point (*New York Times*, 25 March 1986).

18. A complication is that the Saudis have only one instrument of punishing, driving down the world price of oil, so they cannot target retaliation against individual opponents. In our model, the punishment of an ally harmed only that ally. Also, some of the Saudis' usual "friends" also suffer considerably from low oil prices and may threaten violence against the Saudis. Moreover, at prices below approximately \$15 a barrel, oil competes favorably with European coal and U.S. natural gas industries, multiplying the number of "enemies" the Saudis face.

19. Moreover, their oil royalties are only collected on production above a fair recovery price, probably in the low twenties for most North Sea oil, so significant revenues were unlikely to reaccrue at any oil price likely in the near term.

20. We are grateful to Jim Morrow (personal communication) for these suggestions, as well as the example of Warsaw Pact relations for the possibility of declining severity over time.

21. For instance, it was a year after the budget deferral before figures became public. Moreover, it was hard to tell how much oil the Saudis were producing. On one day in early March 1986, the *Wall Street Journal* and *New York Times* contained claims (respectively) that Saudi production had declined and increased. Innovations in netback contracting (which provide for payment to crude oil producers at rates dependent on the ultimate market prices of refined products) meant that the Saudis (who observe their own portfolio and cash flow) knew far better than anyone else at what price their oil was being sold.

22. For example, in February 1985 the New Zealand government refused a U.S. request for a destroyer Buchanan to pay one of its ports a call. The U.S. retaliated by declaring the ANZUS treaty no longer binding so as not to set a precedent that could be followed by "allies that really count, the ones in NATO and, of course, Japan" (*Washington Post*, 26 February 1985).

23. The authors are grateful to David Baron for clarifying this issue.

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# INCOME DISTRIBUTION IN THE UNITED STATES

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*P*olitical action has affected postwar income distribution in the United States mainly through policy-induced variations in macroeconomic activity and government transfer benefits in proportion to total income. We present a small dynamic model of the connections among the partisan balance of power, macroeconomic fluctuations, transfer spending trends, and income distribution outcomes. The model is based on the premise that the parties have different distributional goals, and it is designed to identify how shifts in party control of the presidency and the strength of the parties in Congress have affected the distribution of after-tax, after-transfer income by influencing cyclical economic performance and the flow of resources to transfer programs. We therefore extend the "partisan theory" of macroeconomic policy to the domain of income distribution outcomes.

Economic bases of party affiliation must be sought in [the] area of income distribution.

—George J. Stigler

**C**onventional democratic political action can affect significantly the distribution of economic well-being, and politically induced changes in distribution affect in turn the level of living standards experienced by individuals and classes. For these reasons political life revolves to a great degree around short-run distributional struggles.

The main empirical indicator of the distribution of economic well-being is the distribution of (measured) income. At any given time and place the observed income distribution depends on the distribution of chance events (luck), the distribution of

skills and ability (individual endowments of human capital, innate and acquired), the distribution of motivation and effort (ambition), the degree to which structural and institutional arrangements permit the transmission from generation to generation of earlier distributions (the rigidity of the class system), the macroeconomic environment (business cycles and the degree of economic "slack"), and government social and economic policies (see Becker 1967; Bowles 1972; Jencks et al. 1972; and Mincer 1974). Government policy influences distributional outcomes in at least three ways.

First, government regulations (tariffs, quotas, legislated barriers to market entry and market competition, etc.) and targeted tax and credit policies may profoundly advantage those engaged in the favored activities. Aside from tax prefer-

ences geared to and exploited by high-income classes, however, such micro-economic policies generally do not affect inequality by systematically conferring benefits on some income classes at the expense of others. Redistributions from this quarter are largely arbitrary and not related to the preexisting size distribution of income. (For many examples, see Tullock 1983.)

Second, government macroeconomic policy (aggregate fiscal thrust and the aggregate supply of money and credit) affects distributional outcomes indirectly by influencing cyclical macroeconomic activity. Empirical work shows that lower unemployment and higher growth rates, in addition to raising aggregate national income, reduce both inequality and outright poverty. Empirical evidence also indicates that increased inflation, which often accompanies cyclical expansions, is largely neutral with respect to inequality of income distribution and erodes the wealth position of (the highest) income classes holding financial capital (Blank and Blinder 1986; Blinder and Esaki 1978; Hibbs 1987, chaps. 2, 3).

Finally, government directly affects income distribution through income- and employment-contingent taxes and transfers. Throughout the postwar period effective federal tax rates on personal incomes have been only mildly progressive (somewhat less so after the changes achieved by President Reagan than before); so the federal tax system has made, at most, a modest contribution to after-tax equality. The personal tax system as a whole is essentially proportional (see Pechman 1985).

Most of the government's direct influence on income inequality is exerted by federal transfer spending programs, notably social security (OSADHI), unemployment insurance (UI), Aid to Families with Dependent Children (AFDC), housing assistance, supplementary security income (SSI), food stamps, medicaid and

medicare. During the last 30 years the resources devoted to these programs have grown substantially, though at varying rates. In fact, in the United States as well as in other developed democracies, transfers account for nearly all of the growth of government expenditure in relation to aggregate income during the past quarter century. Spending on transfers to persons (inclusive of spending for in-kind transfer programs) grew from around 5% of total personal income in the early 1950s to just over 16% by the early 1980s. To be sure, several of the important transfer programs are not targeted on low-income households alone. But with the exception of UI, two-thirds or more of the benefits of the programs listed above flow to the bottom quintile of the income distribution, and all are important to the economic well-being of lower-income classes (Danziger, Haveman, and Plotnick 1981).

Politics and policy, then, systematically affect inequality of income distribution in the United States by influencing the flow of transfer spending and cyclical macroeconomic activity. We analyze how shifts in the balance of power between the parties have affected distributional outcomes during the postwar period in the United States. Specifically, we examine the response of the size distribution of after-tax, after-transfer family income to changes in unemployment and transfer spending generated by shifts in party control of the presidency and variations in the strength of the parties in Congress.

### **Party Constituencies, Voter Interests, and Party Goals**

The Democratic and Republican parties have more heterogeneous social bases and are less distant ideologically than the major parties in most advanced industrial democracies. Nonetheless, in U.S. national politics the Democratic party is indisputably the party of the "Left," with



## Income Distribution in the U.S.

differential appeal to lower-income classes; and the Republican party is just as clearly the party of the "Right," with relatively great attractiveness to upper-income classes. This is essentially as true today as it was during the late New Deal, at which time the displacement of traditional sectional politics by modern class politics was consolidated,<sup>1</sup> although, as Edsall argues, the Republicans in recent years may have become more effective in representing the interests of their upper-income constituencies (Edsall 1984).

The down-scale classes, which make up the core constituency of the Democratic party, hold human capital (their economic well-being depends primarily on earnings from labor) and tend to occupy unsheltered, lower-status jobs. Democratic supporters therefore have greater exposure to rising unemployment than Republican supporters and they normally bear a disproportionate share of the economic and broader social costs of cyclical contractions. Consequently, their relative position in the income distribution as well as their absolute level of economic well-being is typically improved by falling unemployment and tight labor markets. The Democratic constituency is also advantaged by a generous system of income- and employment-contingent transfer spending, even if such spending is financed by proportional tax levies.

Up-scale groups, which form the core constituency of the Republican party, hold the lion's share of financial capital in the household sector; and they tend to occupy higher-status, sheltered jobs. So Republican supporters absorb greater losses from extra inflation than Democratic supporters and they experience the greatest gains from disinflation. On average, the Republican constituency also loses (or benefits less than Democratic supporters) from growth-of-income transfers.

These differences in the interests of the parties' core constituencies are reflected in

the pattern of policies and outcomes observed under Democratic and Republican administrations. On the side of the macroeconomy, Democratic administrations have been more likely than Republican ones to pursue expansive policies yielding lower unemployment and extra growth but running the risk (frequently realized) of higher inflation. Republican administrations usually weight the problem of inflation more heavily. As a result they have more readily and more vigorously pursued disinflationary policies, and they have been more cautious about stimulating aggregate demand and employment.

Similarly, the parties have contrasting income distribution goals that are consistent with the locations of their core constituencies in the hierarchy of income classes. (For some opinion data on the views of party leaders and followers on income equality and redistribution see Verba and Orren 1985.) The general course of income security and redistribution spending policies during the postwar period is one of Democratic initiatives that successfully (though rather modestly) helped improve the relative position of the lower quintiles of the income distribution, followed by periods of Republican inaction or retrenchment, followed in turn by new Democratic efforts to improve equality, and so on (see Page 1983).

### **Income Distribution Measurement**

Empirical analysis of trends and fluctuations in distributional outcomes requires choice of an income distribution measure. Measurement of income inequality is a complicated matter and has been the subject of an enormous literature. (See Cowell 1977 for a good overview.) Fortunately, our purposes are well served by a very straightforward measure that follows naturally from what we

know about the incidence of government taxes and transfers and the distributional consequences of cyclical economic activity.

Income-contingent transfers, which are the main fiscal engine of government-induced redistribution, flow almost entirely to the bottom two quintiles of the pretax, pretransfer distribution. Computations by Musgrave and Musgrave (1984) and others show that the tax system is nearly proportional and that net benefits of all taxes and transfers are unambiguously positive (and progressively sloped) for the bottom two quintiles of the distribution, are quite flat or proportional over the next two quintiles, and turn progressively more negative over the top quintile. The principal redistribution achieved by the tax and transfer system is from the top 20% of the distribution to the bottom 40%.

The very same distributive effects accompany cyclical fluctuations of the macroeconomy. Empirical work shows that variations in the inflation rate are only weakly connected to fluctuations of the size distribution of income. And the associations that do exist suggest that inflation adversely affects the income share of the upper quintile relative to the shares of the bottom two quintiles. On the other hand, cyclical declines in unemployment unambiguously raise the share of the bottom two quintiles, almost exclusively at the expense of the top one. (See the sources cited previously.)

Hence, income distribution movements induced by the business cycle and the tax-transfer system consist largely of flows between the top 20% and the bottom 40% of income classes. A natural income distribution measure for our purposes, therefore, is the ratio of the posttax, post-transfer share of the top 20% of the family distribution to the posttax, post-transfer share of the bottom 40% of the family distribution.<sup>2</sup> The natural logarithm of this ratio (multiplied by 100),

which we use in the empirical analyses ahead, gives the percentage gap between the net (after taxes and transfers) income shares of the top fifth and bottom two-fifths of the family distribution. We shall frequently refer to this inequality measure as the *equality gap*.

During the period 1950–83 the average share of net income going to the top quintile was about 40% of the total family income, whereas the average net share of the bottom two quintiles was about 20%. The postwar mean of the 20-to-40 ratio is 1.97, with standard deviation .15; and the mean for the natural log of the ratio (times 100) is 67.6 (an average equality gap of 67.6%), with standard deviation 1.3. The 20-to-40 ratio of net income shares essentially contrasts the relative experience of the upper middle class and the rich to the lower middle class and the poor. It registers most of the distributional action in the underlying data on all quintile shares and at the same time corresponds nicely to the parties' core constituencies insofar as they are income-related. The "omitted quintiles," the middle 40% of the net income distribution (the broad middle classes), though tilted toward the Republicans at the upper range, are the critical battlegrounds of party competition for marginal votes.

### A Small Dynamic System for the Politics and Economics of Income Inequality

#### The Proximate Determinants of Inequality

It is easy to show that the political strength of the Democrats has a significant negative statistical association with income inequality over the postwar period. This much is clear from the first two columns of Table 1, which report simple regressions (for annual data, 1950–83) of the equality gap-variable just

## Income Distribution in the U.S.

**Table 1. Estimates for Income Inequality Equations, Annual 1950-83**

Variables	Regression Models				Means
	(1)	(2)	(3)	(4)	
Constant	6.42* (3.49)	16.3** (6.36)	27.7** (9.78)	24.1** (7.48)	
$\log I_{t-1}$ (equality gap)	.912** (.05)	.868** (.05)	.624** (.11)	.640** (.09)	68.2
$U_t$ (unemployment rate)			1.90** (.49)	1.85** (.37)	5.56
Trans <sub>t</sub> (share of transfers in personal income)			-1.03** (.33)	-1.01** (.27)	10.3
Dempres <sub>t-1</sub> (Democratic presidents = 1)	-2.09** (.83)		.438 (1.02)		.47
Demcong <sub>t-1</sub> (% share of Democrats in Congress)		-.135* (.08)	-.048 (.07)		58.3
Adjusted $R^2$	.904	.894	.931	.935	
Standard error of regression	2.36	2.47	1.99	1.94	
Method	OLS	OLS	OLS	OLS	

Note: Base model:

$$\log I_t = \alpha_0 + \alpha_1 \log I_{t-1} + \alpha_2 U_t + \alpha_3 \text{trans}_t + \alpha_4 \text{Dempres}_{t-1} + \alpha_5 \text{Demcong}_{t-1}.$$

Estimated standard errors are shown in parentheses.

\* $p < .05$ .

\*\* $p < .01$ .

described (denoted  $\log I$ ) on its own lagged value ( $\log I_{t-1}$ ) and a binary variable for Democratic presidential administrations (Dempres) and the average percentage share of Democrats in the Congress (Demcong),<sup>3</sup> both lagged one period. The third regression in Table 1, however, shows that these results are not structural; they merely establish statistical correlations. In the presence of unemployment ( $U$ ) and spending on transfers to persons as a percentage of gross personal income (trans), which are the two main channels by which government policy systematically affects the size distribution of income, the coefficients of Dempres and Demcong are indistinguishable from zero. Hence, the best model in Table 1 for the time path of the equality gap ( $\log I$ ) is the first-order dynamic equation in the last column

(Model 4), where fluctuations in inequality are driven by changes in unemployment and transfer spending:

$$\log I_t = \alpha_0 + \alpha_1 \log I_{t-1} + \alpha_2 U_t + \alpha_3 \text{trans}_t \quad (1)$$

The results for this equation indicate that each extra percentage point of unemployment initially raises inequality by 1.85 percentage points. Sustained indefinitely, an extra point of unemployment adds just over five points to the equality gap ( $1.85/[1 - .64] = 5.1$ ). Transfer spending also has powerful effects on income inequality. A rise of one percentage point in the transfer share of personal income closes the equality gap by about one percentage point during the first year; after all lags of adjustment the

same increase in transfer spending lowers inequality by 2.8% ( $-1.01/[1 - .64] = -2.8$ ).

Movements in unemployment<sup>4</sup> and transfer spending, with allowance for first-order dynamic adjustment, are, then, the principal proximate determinants of postwar fluctuations in the inequality of disposable personal incomes. It follows that political influence on the size distribution of income must operate through at least one of these channels. We develop first a political analysis of the unemployment channel.

### Modeling Unemployment and Inflation Time Paths

As we noted previously, the Democratic and Republican parties have different unemployment targets, denoted ahead by  $U^T$ , which are constrained by and tend to vary about a "normal" or benchmark unemployment rate, denoted  $U^N$ . Although the Democrats' constituency weights unemployment more heavily and inflation less heavily than the Republicans' constituency, few voters of any political orientation are happy with high rates of inflation (Hibbs 1987, chaps. 4, 6). Consequently, the unemployment targets of both parties become more conservative (less ambitious) as the inflation rate observed by the actors,  $P_{t-1}$ , rises. Conditional on the received inflation rate ( $P_{t-1}$ ), the unemployment target prevailing during Democratic presidential administrations typically is lower than the corresponding target during Republican administrations, which leads to the  $U^T$  equation

$$U_t^T = \beta_0 + U_t^N + \beta_1 \text{Dempres}_{t-1} + \beta_2 P_{t-1} \quad (2)$$

where, as before, Dempres is a binary variable equal to one during Democratic presidential administrations and zero during Republican administrations;  $U^N$ ,

which trends upward from 5% in the late 1950s to 6% in the late 1970s, is set equal to Robert J. Gordon's calculation of the natural unemployment rate (Gordon 1984b, Appendix B);  $P$  is the year-on-year percentage rate of change of the GNP deflator; and we anticipate that  $\beta_2 > 0$ ; and the party hypothesis requires  $\beta_1 < 0$ .

Because the macroeconomic policy machinery is largely directed by the administration rather than Congress, the party controlling the White House (Dempres), rather than the strength of the parties in Congress (Demcong), is the relevant variable for political analysis of unemployment outcomes. (This presumption is supported by empirical analysis not reported here.) The Dempres term appears with a one-period lag because the unemployment target reflected in current policies is based on the party in power during the previous period.<sup>5</sup> Hence by Equation 2 the unemployment target underlying policy during a typical Democratic administration is  $\beta_0 + U_t^N + \beta_1 + \beta_2 P_{t-1}$ , and the target during a typical Republican administration is  $\beta_0 + U_t^N + \beta_2 P_{t-1}$ . Given the behavioral lags in policy formulation, institutional lags in policy implementation and, most important, structural lags in the response of the economy to policy actions, economic goals cannot be realized immediately. Administrations are able to adjust the observed unemployment rate,  $U$ , to their preferred rate,  $U^T$ , only partially each period. The adjustment mechanism is

$$U_t - U_{t-1} = \phi(U_t^T - U_{t-1}) + \beta_3 \text{shock}_{t-1} + v_t \quad (3)$$

where  $0 < \phi < 1$ , and  $v$  is a well-behaved disturbance.

Equation 3 implies that policy-induced changes in unemployment from one year to the next are capable of closing only a fraction ( $\phi$ ) of the gap between the current target and the actual unemployment rate

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observed for the previous period. Fluctuations in unemployment due to shocks exogenous to the domestic political economy, notably the energy price hikes of 1973-74 and 1979-80, are represented by the variable *shock*, which is equal to the percentage point change in the price of imported oil less the inflation rate of the GNP deflator, weighted by the net share of energy imports (imports less exports) in GNP.

Substituting Equation 2 into Equation 3 and solving for  $U_t$  yields the following nonlinear equation for the time path of unemployment:

$$\begin{aligned}
 U_t = & \phi\beta_0 + (1 - \phi)U_{t-1} + \phi U^N_t \\
 & + \phi\beta_1 \text{Dempres}_{t-1} + \phi\beta_2 P_{t-1} \\
 & + \beta_3 \text{shock}_{t-1} + v_t
 \end{aligned}
 \quad (4)$$

Equation 4 specifies the way in which political and economic forces influence income inequality via the unemployment channel of Equation 1. Estimates for this equation, which appear in the first column of Table 2 (Regression Model 5), give strong support to the party cleavage approach to modeling unemployment outcomes. (The constant  $\beta_0$  in Equation 4 was statistically insignificant and nearly zero, so it is omitted from the estimation equation in Table 2.) The rate of adjustment of unemployment to party targets ( $\phi$ ) is on the order of 38% per year (.38), which means that adjustment is almost fully realized after four to five years.

Unemployment also was affected by exogenous supply shocks. The estimate of  $\beta_3$  indicates that the OPEC supply shocks initially raised unemployment, after a one-period lag, by .8 percentage points for each GNP-weighted percentage point increase in energy prices above the domestic inflation rate. The shock variable takes peak values of 2.3 in 1974, .5 in 1979, and 1.4 in 1980 (in other periods shock is negligible); and dynamic calculations imply that the energy price hikes raised unemployment directly by about

1.8% in 1975, .9% in 1976, .4% in 1980, and 1.3% in 1981. (After 1982 declining energy prices were a source of downward pressure on unemployment.) Once allowance is made for the rises in inflation caused by the oil price increases, calculations of the shock-induced effects on unemployment would be even greater because extra inflation tends to raise the unemployment targets of administrations of either party.

Political analysis of the unemployment dynamics implied by Equation 4 requires joint consideration of the parameter for, and time path of, party control of the presidency ( $\beta_1$ ,  $\text{Dempres}_{t-1}$ ) and the parameter for, and time path of, inflation ( $\beta_2$ ,  $P_{t-1}$ ).  $\beta_1$  estimates the magnitude of the cross-party difference in unemployment targets and hence the typical impact of sustained changes in party control of the presidency on unemployment performance, *assuming everything else is held equal*. Hence, for benchmark unemployment ( $U^N$ ), external supply shocks (shock), and lagged inflation ( $P_{t-1}$ ) held fixed the results in Table 2 imply that a shift from a Republican to a Democratic administration typically would yield a decline in unemployment of about .97 percentage points ( $\phi\beta_1$ ) after the first year and about 2.6 percentage points ( $\beta_1$ ) after all adjustment lags.

For the reasons noted earlier, however, inflation prompts the parties to raise their unemployment targets, which eventually results in higher unemployment rates. The estimates in Table 2 indicate that each extra point on the inflation rate leads to a rise in unemployment of about .11 percentage points ( $\phi\beta_2$ ) after one year and, if sustained long enough, to a rise of .30 percentage points of unemployment ( $\beta_2$ ) per extra point of inflation after all lags of adjustment to party goals. Moreover, the U.S. Phillips curve tells us that higher unemployment yields lower future inflation; a connection that Robert J. Gordon aptly described as "one of the most stable

empirical macroeconomic relationships of the postwar era" (Gordon 1984a, 42). These feedback relationships—from rising inflation to higher unemployment via the implementation of party targets, and from higher unemployment to lower future inflation via the Phillips curve—mean that although  $U^N$  and shock may realistically be viewed as exogenous and held fixed when evaluating the impact of shifts in party control of the White House, the inflation rate may not. So interpreting  $\beta_1$  in isolation (by holding inflation "fixed") may give an exaggerated idea of the interparty difference in unemployment goals and performance.

Accurate evaluation of the impact of the parties on income distribution through party differences in unemployment performance requires bringing the response of inflation to unemployment (the Phillips curve) explicitly into the analysis. The standard rule-of-thumb for the U.S. macroeconomy is that each extra percentage point of unemployment, sustained a year, produces a decline in the annual inflation rate, exogenous shocks aside, of about one-half a percentage point. (By *extra unemployment*, we mean deviations of the actual rate,  $U$ , from the "natural" or nonaccelerating inflation rate,  $U^N$ .) This standard rule implies the relation

$$P_t = \alpha_1 P_{t-1} + \alpha_2 (U_t - U^N_t) + \alpha_3 \text{shock}_t \quad (5)$$

with  $\alpha_1 = 1.0$ ,  $\alpha_2 = -.5$  and  $\alpha_3 > 0$ . The regression estimates reported in the second column of Table 2 (Regression Model 6) conform almost perfectly to this stylized Phillips curve.<sup>6</sup>

Joint analysis of Equations 4 and 5 will yield appropriate estimates of the magnitudes and duration of the effects of changes in party control of the White House on the time path of unemployment and, through the unemployment channel, the effects of partisan change on the time

path of income distribution. Before pursuing this further, we first complete the system by analyzing the transfer spending channel of political influence on inequality.

### Modeling the Flow of Transfers in Personal Income

We pointed out previously that cash and in-kind transfer programs have been the government's main fiscal instruments for influencing income inequality and poverty. So the parties' transfer spending targets ( $\text{trans}^T$ ) are intimately related to their income distribution goals ( $\log I^T$ ). Indeed, in contrast to unemployment and inflation, which influence income distribution (directly in the case of unemployment, indirectly in the case of inflation because extra inflation raises party unemployment targets) but are independent objects of policy, transfer spending goals are based primarily on the distributional effects of income security programs.<sup>7</sup> At each period party spending targets are also constrained by the existing fiscal landscape, as reflected in actual transfer spending during the previous period ( $\text{trans}_{t-1}$ ). As "incrementalist" theories of budgeting have correctly emphasized (the seminal contribution being Davis, Dempster, and Wildavsky 1966), spending goals are not formed *de novo* each period; any given stock of spending tends to get "locked in"—or at least to exhibit considerable inertia—which imparts an upward bias to spending in relation to gross income. Accordingly, the transfer spending target embodied in current policy is

$$\text{trans}_t^T = \delta_0 + \delta_1 \log I_t^T + \delta_2 \text{trans}_{t-1} \quad (6)$$

where  $\log I^T$  denotes the prevailing target for the equality gap variable introduced previously and, as before,  $\text{trans}$  (actual transfer spending) and  $\text{trans}^T$  (transfer

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spending targets) are expressed as percentage shares of aggregate personal income, inclusive of in-kind benefits.

By Equation 6, transfer spending goals, after allowing for the constraining influence of the received level of transfer expenditures ( $\delta_2 \text{trans}_{t-1}$ ) and a scaling constant ( $\delta_0$ ), are proportional to the prevailing inequality target. Other things equal, the higher the prevailing target for income inequality (which, as we indicate below, depends on the balance of political forces), the lower the prevailing target for transfer spending in relation to total personal income. Clearly, the theory requires that  $\delta_1$  be less than 0 in the transfer spending equation. The quantity  $\delta_2$ , which weights the constraining influence of existing transfer spending on the formation of policy targets, should lie in the interval  $0 < \delta_2 \leq 1$ . And, for  $\delta_2 < 1$ ,  $\delta_0$  should be greater than 0.

The Democrats, as we argued earlier, aim for a more egalitarian distribution of income than the Republicans; in other words, they generally push for a smaller equality gap,  $\log I$ . In principle, the magnitude of the inequality target underlying current transfer spending policy might depend on both the party controlling the White House ( $\text{Dempres} = 1$  denoting Democratic control) and the strength of the parties in Congress ( $\text{Demcong}$  being the average share of Democratic seats in the House and Senate). But neither party seriously entertains "pie-in-the-sky" income distribution goals; at every period the formation of inequality targets is constrained by the income distribution observed in the previous period,  $\log I_{t-1}$ . These ideas underlie the equation for the income distribution target reflected in policy:

$$\begin{aligned} \log I_t^T = & \pi_0 + \pi_1 \text{Dempres}_t \\ & + \pi_2 \text{Demcong}_t + \pi_3 \log I_{t-1} \end{aligned} \quad (7)$$

The party cleavage or partisan model implies that  $\pi_1, \pi_2 < 0$ , and that the coef-

ficient scaling the constraining impact of the observed income distribution should fall in the interval  $0 < \pi_3 \leq 1$ .  $\pi_0 > 0$  allows for the possibility that  $\log I_t^T > \log I_{t-1}$ .

U.S. institutional arrangements make it impossible to move actual transfer spending instantly into line with policy goals. Moreover, no matter what the current partisan balance of power and prevailing transfer spending target, transfers will respond automatically to recent changes in the rate of unemployment because of preexisting provisions of unemployment insurance and other employment- and income-contingent transfer programs. (In the longer run the responsiveness of transfers to the business cycle may of course be changed by political action.) For these reasons, we specify a first-order, partial adjustment-to-target equation for movements in actual spending that includes recent changes in unemployment rates:

$$\begin{aligned} \text{trans}_t - \text{trans}_{t-1} \\ = \Theta(\text{trans}_t^T - \text{trans}_{t-1}) + k_1 dU_t \\ + k_2 dU_{t-1} + e_t \end{aligned} \quad (8)$$

where  $\Theta$  represents the fraction of the gap between transfer spending targets and actual transfer spending in the previous period that is closed each year;  $dU$  the first backward difference of unemployment ( $U_t - U_{t-1}$ ); and  $e$  a well-behaved stochastic disturbance. The model implies  $k_1$  and  $k_2 > 0$  and that  $\Theta$  should take a value in the interval  $0 \leq \Theta \leq 1$ .

Substituting the inequality target ( $\log I^T$ ) given by Equation 7 into Equation 6 and substituting the resulting equation for transfer spending targets,  $\text{trans}^T$ , into Equation 8 yields

$$\begin{aligned} \text{trans}_t = & \Theta(\delta_0 + \delta_1 \pi_0) \\ & + (1 - \Theta + \Theta \delta_2) \text{trans}_{t-1} \\ & + \Theta \delta_1 \pi_1 \text{Dempres}_t \\ & + \Theta \delta_1 \pi_2 \text{Demcong}_t + \Theta \delta_1 \pi_3 \log I_{t-1} \\ & + k_1 dU_t + k_2 dU_{t-1} + e_t \end{aligned} \quad (9)$$

It is obvious from Equation 9 that only composite parameters may be estimated. The estimation equation that follows, naturally from Equation 9, however, preserves the signs of political effects anticipated by the underlying theory:

$$\begin{aligned} \text{trans}_t = & \pi_0^* + \Theta^* \text{trans}_{t-1} \\ & + \pi_1^* \text{Dempres}_t + \pi_2^* \text{Demcong}_t \\ & + \pi_3^* \log I_{t-1} + k_1 dU_t \\ & + k_2 dU_{t-1} + e_t \end{aligned} \quad (10)$$

where  $\pi_0^* = \Theta(\delta_0 + \delta_1\pi_0)$ ;  $\Theta^* = (1 - \Theta + \Theta\delta_2)$ ;  $\pi_1^* = \Theta\delta_1\pi_1$ ;  $\pi_2^* = \Theta\delta_1\pi_2$ ; and  $\pi_3^* = \Theta\delta_1\pi_3$ . Given the theoretically required signs of  $\Theta$ ,  $\delta_1$ ,  $\delta_2$ ,  $\pi_1$ ,  $\pi_2$ , and  $\pi_3$  in Equations 6–8, the anticipated signs of the composite parameters in Equation 10 are  $0 < \Theta^* \leq 1$ ,  $\pi_1^* \geq 0$ ,  $\pi_2^* \geq 0$  and  $\pi_3^* < 0$ . (The sign of  $\pi_0^*$ , though likely to be positive, is indeterminate.) Although individual parameters in target and adjustment equations cannot be estimated from observables, it is clear from Equation 10 that political pressure to raise transfer spending, net of recent movements in unemployment (which, as we shall see ahead, is an important qualification), is greatest when the partisan balance favors the Democrats. (For quantitative analyses of spending appropriations and spending trends that include partisan variables, see Browning 1985 and Kiewiet and McCubbins n.d.).

Estimates for two variants of the transfer spending equation appear in the last two columns of Table 2 (Regression Models 7 and 8). The regression results, as anticipated, show that the transfer spending share of personal income responds systematically to economic contractions and expansions. A transitory, one-year jump in the unemployment rate directly raises transfer spending over two years, with most of the effect coming during the first year (.45 as compared to .16). And cyclical expansions lower the transfer share by an equivalent magnitude per

point of declining unemployment. Steady rates of unemployment have no influence on the flow of transfers in relation to personal income, though any sustained shift in the level of unemployment has a prolonged effect on the stock, or level, of transfers in personal income via the lagged feedback term, .92  $\text{trans}_{t-1}$ .

It is also clear from the empirical results that the strength of the Democrats in Congress is the main direct source of politically induced increases in the share of transfers in gross personal income. Although the major income security programs were promoted by and established during Democratic presidential administrations, changes in the resources devoted to these programs, once they were in place, were driven by the congressional strength of the Democrats. Shifts in party control of the presidency had negligible direct influence.

It is important to recognize, however, that our conclusion that changes in party control of the White House typically have little *direct* influence on spending trends is conditional on unemployment fluctuations. Indeed, to the degree that unemployment rises under Republican administrations and falls under the Democratic administrations (Regression Model 5 of Table 2) at any given partisan balance of power in Congress one would tend to observe *positive* statistical correlations between Republican control of the White House and spending as a fraction of aggregate income, because recessions increase the pool of eligible transfer recipients and at the same time shrink total income and output. Consequently, policy-induced contractions yielding extra unemployment may be an important indirect channel by which transfer spending rises during Republican presidencies.

Finally, the estimates for the lagged equality gap term ( $\log I_{t-1}$ ) in regression Models 7 and 8 of Table 2 show that transfer spending tends to fall with increases in income inequality, after a one-



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**Table 2. Estimates for Unemployment ( $U$ ), Inflation ( $P$ ), and Transfer Spending (trans) Equations, Annual 1950-83**

Variables	Regression Models				Means
	$U$ (5)	$P$ (6)	trans (7)	trans (8)	
$\pi_0^*$			2.22 (1.60)	2.02 (1.52)	
$\phi$ ( $U_{t-1}, N^N$ )	.379** (.11)				5.53 ( $U^N$ )
( $U_t - U_t^N$ )		-.517** (.09)			.04
trans $_{t-1}$			.917** (.029)	.919** (.027)	
Dempres $_{t-1}$	-2.60** (.86)		.041 (.18)		.47
Demcong $_{t-1}$			.024* (.014)	.026* (.011)	58.3
log $I_{t-1}$ (equality gap)			-.038** (.015)	-.036** (.013)	68.2
$P_{t-1}$ (inflation rate)	.296** (.12)	.987** (.03)			4.0
shock $_t$ (in % of GNP)		1.64** (.31)			.11
shock $_{t-1}$	.791* (.35)				
$dU_t(U_{t-1} - U_{t-1})$			.450** (.060)	.448** (.058)	.17
$dU_{t-1}$			.170** (.061)	.163** (.051)	
Adjusted $R^2$	.757	.884	.992	.992	
Standard error of regression	.853	.871	.349	.344	
Method	nonlinear LS	AR1 rho = -.51** (.17)	OLS	OLS	

Note: Models:

$$U_t = (1 - \phi)U_{t-1} + \phi U_t^N + \phi\beta_1 \text{Dempres}_{t-1} + \phi\beta_2 P_{t-1} + \beta_3 \text{shock}_{t-1}.$$

$$P_t = \alpha_1 P_{t-1} + \alpha_2 (U_t - U_t^N) + \alpha_3 \text{shock}_t.$$

$$\text{trans}_t = \pi_0^* + \Theta^* \text{trans}_{t-1} + \pi_1^* \text{Dempres}_{t-1} + \pi_2^* \text{Demcong}_t + \pi_3^* \log I_{t-1} + k_1 dU_t + k_2 dU_{t-1}.$$

Estimated standard errors are shown in parentheses; inflation equation excludes Korean War period.

\*p < .05.

\*\*p < .01.

period lag. Although this result is consistent with the theoretical derivation of the model presented earlier, it may at first seem counterintuitive. Remember, though, that inequality targets are proportional (positively) to the actual income distribution observed the previous period. Hence, at a given balance of power between the parties, a downward shift in observed inequality leads to more ambitious (egalitarian) distribution goals, which in turn lead to higher transfer spending. In a sense the model implies that nothing succeeds like success: transfer efforts that successfully reduce the equality gap help sustain future transfer spending. The dynamic works just the opposite way for upward movements in inequality.

Moreover, the response of transfer spending to inequality outcomes augments the direct impact on spending trends of the Democrats' strength in Congress. Higher Democratic congressional strength tends to push up transfer spending because, other things equal, Democrats entertain a lower inequality target than Republicans. In turn, higher spending directly lowers income distribution inequality, which leads to more ambitious income distribution goals and fuels continued higher spending in subsequent periods. Here, as previously, it is apparent that we cannot accurately gauge the quantitative effects (and in some cases even the qualitative effects) of political and economic events on income inequality by looking at the equations of the model individually. They must be analyzed as a system.

### **Numerical Analysis of the System**

The equations developed in the last section represent a small dynamic system of the politics and economics of income distribution in the postwar United States.

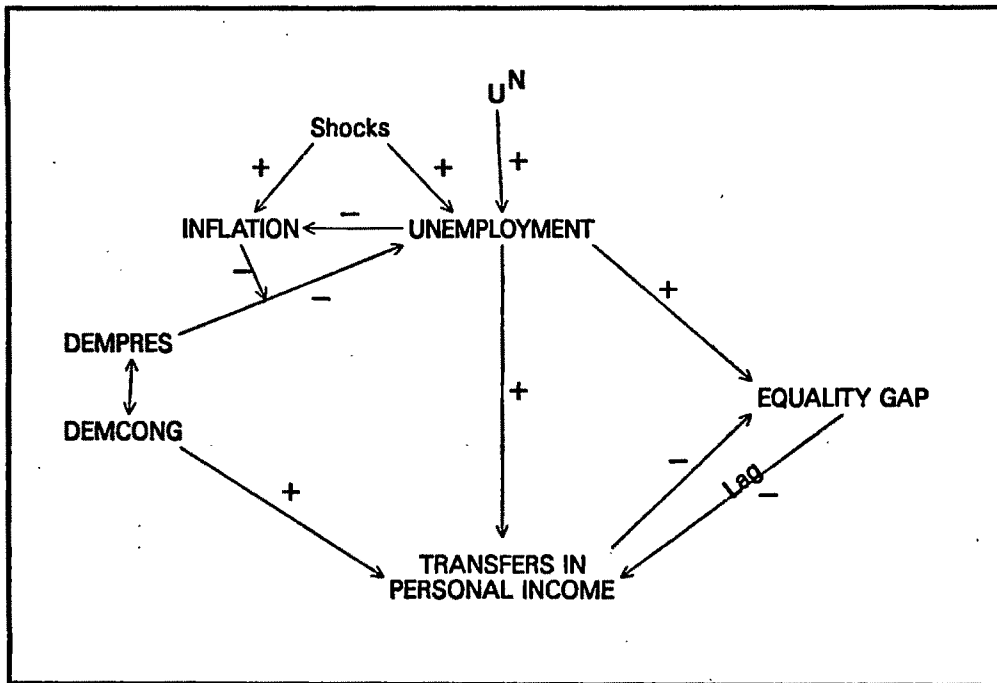
Unemployment, inflation, transfer spending, and inequality are determined endogenously, and they are driven politically by the party controlling the White House and the partisan balance in Congress. External economic shocks and changes in the natural rate of unemployment, which lie beyond the control of conventional policies, also affect the macroeconomy (directly) and spending and inequality (indirectly). Figure 1 illustrates the structure of the system and shows the estimated equations used to analyze its dynamics. The Appendix presents further statistical tests of the central causal assumptions of the model and of its robustness for partisan presidential periods.

We saw from regression Model 4 in Table 1 that both the rate of unemployment and the share of transfers in personal income have powerful effects on inequality of income distribution over time. These are the main channels by which party politics affects distributional outcomes. The discussion associated with Models 5 and 6 in Table 2 showed that estimation of political influence on inequality via the unemployment channel required building inflation explicitly (endogenously) into the system. And Models 5 and 7 in Table 2 implied that the parties indirectly affect income distribution through partisan-based movements in unemployment rates and directly affect inequality by influencing transfer spending flows. Clearly, estimation of the ultimate impact of political forces on income inequality requires that we take account of these various direct, indirect, and feedback lines of influence by analyzing the equations explicitly as a system. This is most conveniently accomplished by dynamic simulation of the estimated equations.

Since the natural rate of unemployment and external supply shocks are incidental to our purposes (though it was necessary to include these variables in the regres-

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**Figure 1. The Politics and Economics of Personal Income Inequality**



Note: These are the estimated equations of the model:

$$U_t = (1 - .38) U_{t-1} + .38 U_t^N - 2.6 \cdot .38 \text{Dempres}_{t-1} + .38 \cdot .30 P_{t-1} + .79 \text{shock}_{t-1}$$

$$P_t = .99 P_{t-1} - .52 (U_t - U_t^N) + 1.64 \text{shock}_t - .51 e_{t-1}$$

$$\text{trans}_t = 2.02 + .92 \text{trans}_{t-1} + .026 \text{Demcong}_t - .036 \log I_{t-1} + .45 dU_t + .16 dU_{t-1}$$

$$\log I_t = 24.1 + 64 \log I_{t-1} + 1.85 U_t - 1.0 \text{trans}_t$$

$U$ : unemployment rate, civilian labor force

$dU$ :  $U_t^{-1} U_{t-1}$

$P$ : year-on-year percentage rate of change of GNP deflator

$\text{trans}$ : transfers to persons as a percentage share of aggregate personal income (inclusive of in-kind benefits)

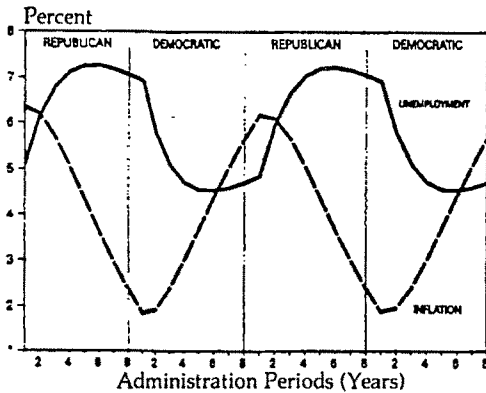
$\log I$ : natural log (times 100) of the ratio of the income share of the top quintile to the lowest two quintiles of the family distribution, inclusive of taxes, cash, and in-kind transfers and the imputed value of unreported income

$\text{shock}$ : annual inflation rate of energy prices less inflation rate of GNP deflator weighted by net share (imports over exports) of energy in GNP

$\text{Dempres}$ : 1 during Democratic presidential administrations, 0 otherwise

$\text{Demcong}$ : average percentage share of Democratic seats in the House and Senate

**Figure 2. Unemployment and Inflation under the Parties**



sions to secure unbiased estimates of parameters), they are not varied in the dynamic calculations. Shock is kept at zero, and  $U^N$  is held at 6%—Robert J. Gordon's estimate of the natural rate of unemployment for the mid-1970s and after. The simulation calculations therefore do not reflect the one-percentage-point rise in  $U^N$  from the 1950s to the 1970s. (Given recent declines in the labor force, computations of  $U^N$  for the last half of the 1980s, which lies beyond our estimation range, show a decline from the 6% natural rate prevailing earlier. In fact, recent estimates of the natural rate suggest that it fell from approximately 6% in 1980 to approximately 5.2% by 1984.) Because neither party has held the presidency for more than two terms in succession in the postwar period, we analyze the system for eight-year cycles of party control of the White House. The strength of the parties in Congress is evaluated at "high," "average," and "low" values corresponding to postwar experience. Initial conditions for endogenous variables are based on their average values for the early 1950s.

#### Macroeconomic Cycles under the Parties

Figure 2 shows the time path of un-

employment and inflation for two pairs of eight-year regimes of Republican and Democratic control of the presidency. The strength of the parties in Congress does not enter the story here because, as we pointed out earlier, macroeconomic policy lies primarily under the control of the executive branch. Since the equations underlying these results are stripped of random shocks, supply shocks, and macroeconomic influences beyond the control of conventional economic policy, the time paths graphed in the figure, unlike "real world" outcomes, are quite smooth. Yet they reveal the fundamental influence of differences in the parties' economic goals and priorities on postwar unemployment and inflation performance.

Two features of the results are immediately apparent. First, the unemployment and inflation cycles observed under the two-term presidential regimes are stable. Neither economic variable exhibits the sort of unbounded growth or decline that (in this case) would be a sure sign of a poorly specified model. Second, the peak interparty differences in unemployment performance occur several years before the end of each eight-year stretch of party control of the White House. Under the typical Democratic presidency, unemployment falls steadily (at a declining rate) over the first six years, bottoms out at a value just above 4.5%, and then begins to creep upward toward the end of the second presidential term. Unemployment rises steadily (again, at a declining rate) over the first six years of the typical two-term run of Republican presidents, reaches a peak value of about 7.25%, and then declines over the last few years of the second term.

The response of inflation to unemployment in the economic system combined with the sensitivity of party unemployment goals to inflation in the political system underlie these characteristic unemployment time paths. Falling unem-

## Income Distribution in the U.S.

ployment under Democratic presidencies yields rising inflation, which eventually prompts reevaluation and relaxation of unemployment goals. This usually occurs as the inflation rate approaches 5%. The reverse process tends to work during Republican regimes. As inflation falls to 3% in response to rising unemployment, the desire to create still more slack dissipates, policy changes course, and unemployment begins to decline somewhat. These inflation-induced shift points in the parties' unemployment goals and performance, implied by the model and illustrated in Figure 2, closely track Hibbs's independent estimates, obtained from opinion survey data, of the macroeconomic configurations underlying shifts in the electorate's relative concern about the problems of inflation and unemployment (Hibbs 1987, chap. 4).

Viewed from the end of one eight-year partisan regime to another, the results graphed in Figure 2 indicate that the difference between the unemployment performance of the typical Democratic and Republican administration is about two percentage points, again assuming a steady natural rate of 6% and no supply shocks. The corresponding difference for inflation performance is just over four percentage points. The difference in the parties' unemployment performances would dwindle to much smaller proportions (and for a completely accelerationist inflation equation would vanish completely), and the difference in inflation performance would become larger, if we analyzed the implications of the model for much longer stretches of party control of the White House. But neither party has held the White House for more than eight years in the postwar era, and so no political-economic model can yield reliable empirical estimates of the likely consequences of prolonged runs of party control of the presidency for economic performance. (On the issue of duration see also Alesina and Sachs n.d.; Alt 1985;

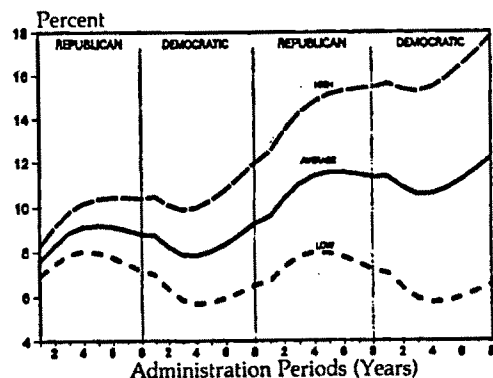
Chappel and Keech 1986a, 1986b; and Hibbs 1977, 1986, and 1987.)

Naturally the stylized results shown in Figure 2 for the macroeconomic consequences of shifts in control of the presidency (that is, shifts in control of the macroeconomic policy machinery) do not conform precisely to observed performance under any particular Democratic or Republican presidential administration. But they do give a realistic idea of the characteristic macroeconomic cycles, net of external shocks and stochastic variation, that accompany change in partisan control of the White House. And as we pointed out earlier, such party-based differences in economic performance have important consequences for transfer spending flows and income distribution outcomes.

### Trends in Transfer Spending under the Parties

Figure 3 graphs trends in transfer spending in percentage of aggregate personal income under the parties. The figure illustrates the time path of transfer spending under Democratic and Republican presidential administrations at different levels of the party balance of power in

Figure 3. Transfer Spending in Percentage of Personal Income under the Parties



Congress. Party strength in Congress is varied at "average," "high," and "low" values, corresponding to the postwar mean and deviations of plus- and minus-one standard deviation from the mean, respectively.<sup>8</sup> As before, results for two stretches of eight-year party regimes, or thirty-two years of stylized political-economic history, are shown. And, also as before, the simulations are fully dynamic and are based on all relevant equations in the recursive system (the unemployment and inflation equations, the endogenous effect of lagged inequality, and the transfer spending equation itself). The initial condition for lagged spending is based on the average transfer spending rate of the early 1950s.

The vertical distances between the calculated time paths in Figure 3 show the response of transfer spending over time to shifts in Democratic strength in Congress by the party holding the White House. An increase in the Democrats' share of seats in Congress yields upward pressure on spending, which in turn puts downward pressure on inequality. Any decrease in inequality prompts more ambitious equality targets than otherwise would have been the case and consequently higher spending than otherwise would have been the case in subsequent periods. In this way the impact of a shift in Democratic congressional strength is magnified in future periods. The process works in reverse for a sustained downward shift in Democratic congressional strength; the impact on spending is magnified in a negative direction over subsequent periods. But one should not take the magnitudes of the time magnifications shown in the figure literally, because the congressional balance of power has never been at "high" or "low" values for very long periods.

The results in Figure 3 also indicate that when the Democrats are at average or above average strength in Congress,

transfer spending in percentage of personal income tends to trend upward, no matter which party controls the White House. In fact, experiments not graphed here show that when unemployment is held fixed at the natural rate, which excludes the impact of shifts in party control of the presidency from the model, transfer spending begins to trend upward when the Democrats' congressional seat share exceeds 53%-54%. Democratic congressional strength in the vicinity of 53%-54% appears to be a critical political threshold for increasing transfer efforts. Since the Democratic seat share in Congress averaged about 58% over 1950-83, the broad upward trend of spending under the "average" and "high" scenarios shown in Figure 3 corresponds, as it should, to actual postwar developments (transfer spending in gross personal income rose from around 6% in the mid-1950s to just above 15% in the early 1980s).

Indeed, the only significant decline in transfer spending in relation to personal income during the postwar period occurred from 1950 to 1952, when the spending share fell by almost two percentage points. This decline was fueled by the unusually low Democratic congressional strength of the period (the Democrats were even weaker in 1953-54 when the Republicans gained numerical majorities in both the House and Senate) and the coincident decline in unemployment under Truman. In Figure 3 this scenario (the only situation in the model—and in the real world—in which the transfer spending share of personal income actually falls significantly) is illustrated by the time path of spending during Democratic presidential administrations when the Democrats are weak in Congress.

The slopes of the simulation time paths in Figure 3 give the typical growth rates of transfer spending under Republican and Democratic presidential administrations

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## Income Distribution in the U.S.

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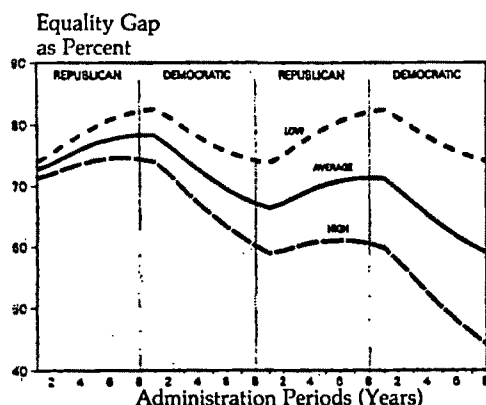
at each level of the congressional balance of power between the parties. Here we see that at any given Democratic strength in Congress, the average rate of growth of transfers in personal income over each eight-year presidential regime tends to be *higher* under Republican presidents than under Democratic presidents. (This pattern, which is consistent with postwar experience, turns up in Browning's 1985 single-equation regression analysis of the correlates of postwar welfare spending trends.) The model developed here implies that the main source of this pattern is the tendency of Republican administrations to create extra unemployment (see Figure 2 and the previous discussion), which automatically raises income- and employment-contingent transfer spending and at the same time slows the growth of aggregate income and output, thereby raising the share of spending in aggregate income.

Events during President Reagan's first three years provide a graphic example of the stylized results shown in the figure for the "low"-Democratic-congressional-strength-Republican-president scenario. The Reagan administration assumed power with greater commitment to reversing the postwar trend of social spending than any other Republican government in the modern era. The unusually low strength of the Democrats in Congress gave the President something of a golden opportunity. Yet the administration was also determined to bring inflation under control. The severe policy-induced disinflationary contraction of 1981-83, which lowered personal income and output and produced the highest unemployment rates since the end of the Great Depression, overwhelmed the administration's spending plans. The sharp economic downturn automatically raised income- and employment-contingent spending. As a result, the transfer spending share of personal income rose continuously from 1980 to 1983 (though, to

be sure, at a slower rate than would have occurred had the Democrats been stronger in Congress). The first Reagan administration did not succeed in bringing the transfer share of personal income down to the levels inherited from Carter until the last year of the president's term. (Events under Mrs. Thatcher's government in Britain illustrated the same pattern.)

The joint potency of shifts in the congressional balance of power in favor of the Democrats and rising unemployment under Republican presidential administrations is illustrated well by the events of 1973-75. Transfer spending in relation to personal income jumped more than three percentage points during these years (from 12.7% to 15.9%), the largest rise over such a short period in postwar history. (Two-thirds of the jump occurred over 1974-75.) One important reason for the spending surge was the big increase in the Democrats' muscle in Congress. Following the Watergate debacle the Democrat's share of congressional seats rose by 7.7 percentage points to 63.5%, way above their average strength under Republican presidents. All by itself, this increased pressure on spending. A second reason was the enormous rise in unemployment from 4.9% to 8.5%, though both our model and common sense suggest that only some of extra joblessness (perhaps half of the 3.5-point rise) can be attributed to the higher unemployment targets typical of Republican administrations. The rest of the increase in unemployment and therefore some of the increase in spending should be laid at the door of the first OPEC supply shock rather than partisan politics. The main message of the results shown in Figure 3, however, is that both direct and indirect channels of influence must be modeled explicitly in order to understand properly the dynamic process underlying statistical correlations among politics, economics, and policy outcomes.

**Figure 4. Income Distribution Outcomes under the Parties**



#### Income Distribution under the Parties

Our main purpose was to develop a plausible macro model, consistent with postwar empirical data, of the politics and economics of income distribution in the United States. Figure 4 graphs the impact of shifts in party control of the presidency and changes in the party balance of power in Congress on inequality of income distribution over time, as transmitted through movements in unemployment and transfer spending (shown in Figures 2 and 3). The format of the figure is the same as that used earlier, and the results are again based on a fully dynamic simulation of the estimated equations of the system.

As in Figure 3, the vertical distances between the calculated time paths in Figure 4 show the estimated response of the equality gap to changes in the partisan balance of power in Congress. We learned earlier that the congressional strength of the parties influences income distribution through the flow of funds to transfer programs in relation to the growth of aggregate personal income. And we saw that the impact of any sustained change in the partisan balance in Congress on transfer

spending tends to be magnified over time. This is why the response of inequality to shifts in the Democratic share of seats in Congress is also magnified over time. Assuming that the parties alternate in control of the White House, the calculations in Figure 4 indicate that a change of one standard deviation in the strength of the Democrats in Congress (a change, for example, of about 12 seats in the House and 5 in the Senate), sustained for 32 years, would yield changes in the equality gap that grow from around 1 percentage point in the first period to nearly 15 percentage points in the last. As in the case of transfer spending, the scale of the time magnification should not be taken literally, because neither party has experienced unusually high or low congressional strength for such a long period. Yet the calculations from the model estimates do illustrate an important dynamic tendency in the U.S. political economy.

Secular trends in income distribution outcomes shown in Figure 4 also resemble those in transfer spending discussed previously, but now we see the distributional consequences of movements in unemployment and transfer spending induced directly and indirectly by partisan change. If the Democrats were perpetually at sub-par strength in Congress, dynamic calculations of the system illustrated by the "low" simulation time path suggest that income inequality would tend to be flat over the long run, assuming that the parties oscillated in control of the presidency. In the "low" scenario the increase in the equality gap occurring over a typical two-term Republican presidency is just offset by an equivalent decline in the equality gap over a typical two-term Democratic administration. Luckily for those in the bottom quintiles of the income distribution, postwar history provides no prolonged stretch of Democratic weakness in Congress.

The first three years of President Reagan's administration (the only Reagan



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years for which complete data for our income distribution variable are currently available), illustrate empirically the impact of limited duration Democratic weakness in Congress under a Republican president committed to achieving substantial disinflation. The Republicans' above-average strength in Congress (particularly during Reagan's first two years) made it possible for the administration to reduce income maintenance spending well below what it would have been in a comparable economic environment with a stronger Democratic congressional presence (see Hibbs 1987, chap. 9). In conjunction with the policy-induced rise in unemployment, which was designed to attack (successfully) inflation, the assault on discretionary transfer spending helped produce a five-percentage-point rise in the equality gap (from 60.5% in 1980 to 65.8% in 1983).

Figure 4 indicates that when the Democrats are at average or above-average congressional strength, inequality tends to fall over the long run. The rise of the equality gap during Republican presidential administrations is more than countered by a larger decline during Democratic administrations. Events under President Johnson, whose party enjoyed above-average congressional strength for most of time LBJ occupied the White House, illustrate empirically the course of income distribution when the political balance runs strongly in the Democrats' favor. The Johnson administration gave high priority to achieving full employment, and the unemployment rate fell by more than two percentage points during LBJ's tenure. (Some of the decline was surely due to the escalation of the Vietnam War, but here we make no attempt to net out the war's effects.) Despite the cyclical expansion, transfer spending in relation to personal income nonetheless rose because of Democratic strength in Congress. In combination with falling unemployment, the expansion of transfer

spending produced the biggest sustained reduction of inequality in postwar U.S. history. From 1963 to 1968 the equality gap variable declined by more than 15 percentage points.

The trends from Republican to Democratic presidential administrations in the "average" congressional strength scenario track the postwar experience as a whole most closely, as they should. In the absence of supply shocks and with the natural unemployment rate fixed at 6%, the equality gap tends to rise by 4-5 percentage points under the typical two-term Republican presidential regime and to fall by around 12 points over eight years of Democratic control of the White House. If the parties oscillate in control of the presidency, as they have done over the postwar period, inequality tends to fall over the long run, which, taking in-kind benefits into account, has been the postwar empirical pattern. Despite the large increase during President Reagan's first three years, the equality gap stood about 12 percentage points lower in 1983 than it was in 1952 (65.8% versus 77.5%). And in 1980 it was 17 percentage points lower than in 1952. Twelve, or even 17 percentage points in our equality gap variable hardly amount to revolutionary movements in inequality of income distribution. Yet for those in the bottom 40% of income classes, changes of these magnitudes represented significant shifts in economic well-being.

### **A Concluding Word about the Future Research Agenda**

In the model presented, changes in the balance of political power between the parties are treated as exogenous to macroeconomic, spending, and distributional outcomes. In order to "close the system" and analyze more completely cyclical, as well as equilibrium, properties of the U.S.

Table A-1. Time-Series Causality Tests

Endogenous Variable	Test Equation		
	(1) Pure Autoregressive	(2) Autoregressive Plus Exogenous Economic	(3) Autoregressive Plus Economic and Political
Unemployment	100 (1.34)	77.8	55.0
Inflation	100 (1.78)	83.8	53.6
Transfer spending	100 (.32)		25.4
Inequality	100 (6.20)		63.5

Note: Models:  $Y_t = f(Y_{t-1}, Y_{t-2}, Y_{t-3}, \text{economic variables, political variables})$ , annual 1952-83. The residual variance from each test regression is expressed in percentage of the residual variance of the appropriate autoregressive benchmark. Actual residual variances from the purely autoregressive equations are shown in parentheses.

political economy, the partisan balance of power must be made endogenous.

We know from the literally hundreds of papers on the topic that a government's macroeconomic performance exerts important influence on electoral outcomes. Experience suggests that the Democrats have been more likely than the Republicans to get into difficulty with the voters by pursuing overly ambitious unemployment goals creating extra inflation. The Republicans, on the other hand, appear more frequently to have suffered electoral setbacks because of their enthusiasm for disinflationary bouts of economic slack. In an era when elections have increasingly turned on economic performance, such policy "overshooting" may help explain why neither party has managed to hold the U.S. presidency for more than two consecutive terms since the Second World War.

Yet, surprisingly, relatively little is known about how fiscal outcomes, or, more important, how shifts in income distribution, affect the party balance of power over time. In fact, the only paper we know of that analyzes the interdepen-

dence of distribution and political support for U.S. parties in a dynamic setting is Sprague's unpublished study, which appeared more than a dozen years ago (1975). We believe this important and neglected topic should receive high priority in future work on politics and economics in the United States.

### Appendix: Some Regression Diagnostics

Tables A-1 and A-2 present regression diagnostic tests of the central causal assumptions of the political-economic model for income distribution presented in the main text and of the robustness of the fits generated by each equation in the model for partisan presidential periods.

Table A-1 reports results from standard time-series regressions testing whether right-side variables in the model equations "Granger-cause" the left-side variables. (See Geweke 1984 for a comprehensive review of causality testing in time-series models.) The entries in this table are

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**Table A-2. Equation Prediction Errors for Partisan Presidential Periods:  
Mean Residuals and Associated Significance Levels**

Period	Unemployment	Inflation	Transfer Spending	Inequality
Republican				
Reagan	.68 (.34)	.066 (.89)	-.007 (.96)	.29 (.81)
Nixon-Ford	-.43 <sup>a</sup> (.04)	-.26 (.45)	.074 (.52)	-.37 (.46)
Eisenhower	.0039 (.99)	.15 (.46)	-.091 (.43)	-.096 (.55)
Democratic				
Carter	.075 (.83)	.67 (.43)	-.10 (.52)	1.38 (.13)
Kennedy-Johnson	.25 (.23)	.0013 (.99)	.035 (.73)	-.87 (.22)
Truman	-.38 (.55)		.095 (.83)	1.43 (.51)
Means of the variables	5.6	4.0	10.3	67.6

Note: Residuals are from the final model equations shown with Figure 1. Associated significance levels are shown in parentheses.

<sup>a</sup>Significant departure from mean residual of zero.

based on residual variances obtained from the following sets of test regressions for each endogenous variable in the model: (1) benchmark, purely autoregressive test equations specified with three year lags; (2) test regressions in which the exogenous economic variables (energy price shocks and the natural unemployment rate) that are postulated by the theoretical setup to appear in a particular equation are added to the benchmark autoregressions; and (3) test regressions in which all variables postulated by the theoretical setup to appear in a particular equation are added to the benchmark autoregressions.

For ease of interpretation, the residual variance from each test regression is expressed in percentage of the residual variance of the appropriate benchmark autoregression, and so this percentage equals 100 in the first column of Table A-1. (Actual residual variances from the

benchmark autoregressions are given in parentheses.) Other than the autoregressive terms, the dating of right-side economic and political variables and the functional forms of the equations yielding the results in Table A-1 are based on the theoretical specifications presented in the main text. Parallel time-series test regressions in which a trend term was added to each equation yield the same pattern shown in Table A-1 and are available by request. (Trend terms are often included in Granger causality test regressions to ensure conditional stationarity.)

With the direction of time-*t* relations as part of the maintained hypotheses, the results in Table A-1 show that the economic and political determinants of the dependent variables in the model easily satisfy the Granger standard of causality. Of particular importance are the results showing that the unbiased residual vari-

ances in the unemployment and transfer spending equations (unemployment and transfers being the main proximate sources of movements in inequality) decline substantially with the inclusion of political and endogenous economic variables. (The political variables and endogenous economic variables also pass Granger causality tests when added separately to autoregressive test equations.)

Table A-2 reports residual means and associated significance levels, computed by partisan presidential period, for each structural equation of the political-economic model of income distribution. If the global results in Tables 1 and 2, which, in turn, underlie the simulation analyses illustrated in Figures 2-4, are robust for partisan periods, the residual means should be insignificantly different from zero. By conventional significance levels the only failure of the model to generate robust fits for partisan periods appears in the unemployment equation during the Nixon-Ford years. The residual mean of  $-.4$  is significant at the .04 level, implying that the equation overpredicts unemployment for this Republican period.

## Notes

This is revised composite of papers delivered at the Midwest Political Science Association annual meeting, Chicago, 1985; the Growth of Government in Developed Economies conference, Onasbruck, West Germany, 1986; and the Western Political Science Association annual meeting, Eugene, Oregon, 1986. The work is a joint effort, in which Hibbs had primary responsibility for model specification, statistical analysis, and writing, and Dennis had primary responsibility for research on, and compilation of, the income distribution data series (see Dennis 1986). The authors are grateful to Ulf Christofferson, Henry Chappel, and Hans Jørgen Nielsen for many useful comments on an earlier draft. Hibbs also thanks the Institute of Political Studies, University of Copenhagen; the Danish Social Science Research Council; and the Department of Economics at Göteborg University, Sweden for supporting his contribution. All data series and computations used for the article are available from Hibbs by request.

1. The relation of partisanship to income appears to have been strengthened somewhat from the Eisenhower to the Reagan presidency. See the SRC/CPS data reported by Wattenberg 1986, tbl. 9.3, p. 145.

2. The income data are net of taxes, cash, and in-kind transfers and the estimated magnitude of unreported income. The income concept therefore equals disposable income adjusted for in-kind benefits and income underreporting. Because we include in-kind benefits, which have made an increasingly important contribution to the economic well-being of low-income groups, our data show less inequality (as they should) than cash income data alone. Also, by focusing on post-tax-transfer data (rather than, say, comparing differences between pre- and post-tax-and-transfer distributions) the analysis is not distorted by actions in the market designed to offset the potential redistributive effects of tax-transfer policies. Over time families are a more comparable unit than all households and so we use data on family income shares. Note, however, that we make no attempt to adjust the data to a life cycle conception of income inequality. For further information about our income data consult Dennis's data appendix (1986).

3. Demcong is the percentage share of Democrats in the House plus the percentage share in the Senate divided by two. This measure of the strength of the Democrats in Congress was superior to plausible alternatives (for example, the Democratic shares in the House and Senate measured separately) in all equations we estimated.

4. We might just as well have used real output movements in place of unemployment throughout because unemployment and output are intimately connected through the macroeconomic relation known as Okun's law.

5. Analysis of quarterly models in Hibbs 1986 and 1987 shows that the fiscal and monetary policies observed under the parties are consistent with the differences in party targets implied by the partisan model. To conserve space, only the unemployment results necessary for our analysis of political influences on distribution are developed here.

6. This equation was also run with a constant, which was indistinguishable from zero. For a more elaborate setup, which yields nominal-real trade-off estimates broadly consistent with the results for the simpler equation used here, see Gordon and King 1982.

7. Lindbeck (1983) believes that similar forces underlay increases in social spending in Europe. He argues that the rapid rise of expenditure "is largely a result of the increased use of government budget policies for attempted redistributions of income and welfare" (p. 288).

8. Party strength in Congress and party control of the presidency are positively correlated. The values of party congressional strength used in the simula-

## Income Distribution in the U.S.

tions therefore depend on the party of the president. During Democratic presidencies the low, average, and high values for Democratic congressional strength (which are based on the mean and standard deviation of sample period data) are 56.9%, 61.4%, and 65.9%. During Republican presidencies the corresponding values are 50.4%, 55.6%, and 60.8%.

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# RULERS AND THE RULED: PATTERNED INEQUALITY AND THE ONSET OF MASS POLITICAL VIOLENCE

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*The theory of patterned inequality between rulers and ruled provides a valuable analytic approach to the relationship between inequality and political violence. Under conditions of a bifurcated pattern of inequality, the probability of political violence is likely to be greater than under a more generalized inequality typically measured by the Gini index. A strong systematic relationship between patterned inequality in Latin American landholdings and deaths from political violence was discovered using the exponential distribution as a model for the lower portion of the land distribution and the log-exponential for the upper. This degree of association was far stronger than that found between the Gini index of land inequality in Latin America and deaths from political violence. Evidence supporting the theory was also found in an analysis of Middle Eastern landholdings.*

I set out to explore a conundrum that has emerged in the systematic literature on inequality and political violence. This is the fairly typical finding of a weak, barely significant relationship between inequality and political violence. Sometimes the relationship is above the level of significance (generally  $p < .05$ : Muller 1985; Nagel 1976; Parvin 1973; Russett 1964; Sigelman and Simpson 1977; Tanter and Midlarsky 1967), sometimes below it (Hardy 1979; Nagel 1974; Weede 1981, 1987); but rarely is there a robust relationship discovered between the two variables. Equally rarely does the relationship plunge into the depths of the black hole of nonsignificance. Thus the findings on the whole hover around the most commonly accepted significance level while continuing the controversy as exemplified by the ex-

change between Muller (1986) and Weede (1986).

As will be seen, a principal outcome of this study is the development of a measure of land inequality that will demonstrate a far stronger relationship with political violence than the Gini index. At the same time, serious doubt will be cast on the validity of recent findings claiming the relative unimportance of land inequality as a progenitor of political violence (Muller and Seligson 1987).

## Theoretical Foundations

Theories of the relationship between inequality and political violence have been far more decisive in their assertions. From Aristotle to Marx and Tocqueville, the nexus between inequality and political

violence has been claimed to be strong and direct, with little or no quibbling as to the ultimate outcome—in most cases revolution.

Here, at the outset I will assume a position somewhat different from that of previous efforts. First because of the anomalous or at least divergent findings, I will assume that the relationship between inequality and political violence is context-specific. By that I mean that different regions of the world may yield different sorts of relationships between these variables because of large-scale historical and geographical differences. The term *context* also will have an additional meaning pertaining to the *pattern* of inequalities. I hasten to add that the choice of context specificity does not automatically imply that the relationship between inequality and political violence is not of universal validity, although it may not be. It merely suggests that different world regions could have different empirical *expressions* of inequality, which in the end yield political violence.

The theoretical structure upon which this study is based also will depart somewhat from previous efforts particularly in the choice of intellectual tradition. It emerges from a concern for social and, in particular, distributive justice that is really as old as Aristotle, but which has modern exponents.

A modern theorist of justice approaches this problem from the need for a requirement for justice that will at the same time be stabilizing. This is the requirement by Rawls (1971, 302) that each gain to the most advantaged in society be accompanied by a gain to the least advantaged. This "difference principle," as articulated by Rawls, is stated to ensure that whatever inequalities are the result of differences in talent or inheritance are not perpetuated to the point of generating really severe societal inequalities. In this expanding economic scenario there can occur a minimal sort of identification

between the wealthy and worst-off, for both are gaining from the newly created largesse.

However, this positive form of the difference principle in which all gain as the result of economic expansion should be complemented by the negative form in which all lose as the result of economic contraction.<sup>1</sup> In the event of a depression or some other social or natural calamity, the declines of both rich and poor should be evident. It is this identification between rich and poor simultaneously losing that may have prevented mass political violence from occurring in many instances. The image of rich investors leaping from tall buildings in the 1929 Wall Street Crash may have done much to establish the common sense of identity between haves and have-nots that would forestall any serious successful efforts at massive political change that could result in mass political violence. In contrast, in two recent instances of mass political violence in Nicaragua and in Iran such mutual identification was absent. Immediately after the earthquake of 1972, the Somozas of Nicaragua were perceived as gaining in wealth from the Red Cross aid while many, mostly the poor, suffered. In Iran, the severe decline in economic activity, especially in the building trades in Tehran, differentially affected rulers and ruled, for the shah and his family were hardly at all affected while unemployment increased dramatically and the poor were still further impoverished (Nejad 1986).

Another aspect of the modern scholarly tradition is the emphasis on relative deprivation that includes, among others, Davies 1962, Feierabend and Feierabend 1966, Gurr 1968, 1970, and Huntington 1968 in its ranks. Although the present effort is not firmly rooted in that tradition, the concluding section will develop relationships with it.<sup>2</sup>

Identification between rulers and ruled is the intellectual anchor of this study.



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## Mass Political Violence

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This identification does not have to be uniform or complete, but means only that in a Rawlsian sense, if one societal sector is increasing in wealth or at least holding its own while another poorer sector is declining, then identification between ruler and ruled is virtually nonexistent. If the absence of identification along with continued emiseration of the population relative to the ruling sector continues for long, then large-scale political violence becomes more likely. In the societies to be examined below, landed elites generally have constituted the ruling oligarchies, which, if not directly in control of governments, at least have exercised considerable influence from a short distance. Land distributions in such societies can become potent indicators of bifurcation between rulers and ruled if the appropriate models are applied to extract the bifurcations.

As will become apparent in the empirical analysis, I am dealing here with patterned instead of generalized inequalities as measured by the Gini index. By *patterned inequality* is meant a systematic departure of the pattern of holdings of one societal sector relative to the pattern of holdings of another. Here I will be measuring the pattern of landholdings although, in principle, any societal good can be treated in this fashion. Intrinsic to this analysis is the assumption that human beings are pattern recognizers par excellence. It may even be our ability to recognize the pattern of an environment and react to it successfully that accounts for our evolutionary success as a species (Pagels 1982, 110). The peasant is in a unique position to observe and assess the environment for the peasant spends much time in the fields and is keenly aware of his or her holdings relative to others'. It is not that the peasant is expected to accept or reject a mathematical model at some probability level, as I shall do shortly, but that the constant exposure to the agricultural environment along with family and village histories passed down from one

generation to the next, make of the peasant, even if illiterate, an acute observer and interpreter of the environment.<sup>3</sup>

### Empirical Foundations

As indicated at the outset, the empirical results on inequality and political violence certainly are not uniform; they suggest mixed findings on the relationship between inequality and political violence. Moreover, most of these studies (with the obvious exception of Russett 1964, Tanter and Midlarsky 1967, and Nagel 1976) have emphasized income inequality as opposed to land inequality. Several recent exceptions emphasize population growth and an attendant land inequality as progenitors of the French, Russian, Chinese (Midlarsky 1982), English (Goldstone 1983), and Salvadoran (Midlarsky and Roberts 1985) revolutions.

In the last of these, Nicaragua is contrasted with El Salvador in the coalition-based insurgency against the Somozas versus the largely rural-class-based insurrection in El Salvador. With increased population growth on the same amount of arable land, an exponential distribution of landholdings was predicted in situations of land scarcity relative to population size. That was found to be the case in El Salvador but not in Nicaragua, suggesting an absence of land scarcity and inequality to the extent found in El Salvador. In turn, a broad-based coalition of business, banking, urban groups, and peasants participated in the Nicaraguan revolution, not terribly dissimilar to that which developed earlier in Cuba (Dominguez 1978) under conditions of similar personalist dictatorship. A general conclusion of these analyses was that rural-class-based revolutions are more likely where the land is scarce and the exponential distribution is an adequate descriptor as in El Salvador. Where the land is less scarce as in Nicaragua or Cuba, other types of revolu-

tionary behaviors can arise and broad-based revolutionary coalitions are more likely.

### A Sequential Analysis

These probabilistic analyses were based on random subdivisions of land, leading ultimately in the limiting case, to the exponential distribution. Land scarcity and the random subdivision process (based principally on a large birth rate and the absence of primogeniture) yield the exponential distribution. In this stochastic process there are no strongly favored or least favored actors. All families and landholdings (mostly peasant) are treated equally in their susceptibilities to subdivision over time as the result of their need to accommodate the new family members maturing into adulthood.

Suppose however, that there is a programmed inequality. Instead of each of the early landholdings being approximately equal in size and the subdivision process over time inducing inequality as in the earlier treatments, the inequality is inherent in the distribution at the outset. There are processes that exhibit such severe inequalities. Any sequestering of a scarce resource that also requires a surrounding "cushion" or margin of safety is a case in point. Those who come later to the process must defer to the earlier arrivals who already have established their control with the accompanying margin of safety to ensure continued control.

The history of the British in India expanding to the northwest frontier is a case in point. Later the French in Asia could colonize only on the fringes of the large British holdings. Still later the Germans, especially in Africa, could colonize only on the fringes of the British and French holdings. In successive stages each of the arrivals determines the smaller proportionate share of the later arrivals in contrast to the equiprobability of initial access assumed in ordinary stochastic models.

Land distribution is another variable likely to be modeled successfully by this sequential process. As in the sequence of colonial acquisitions, the first arrivals to a sparsely settled area will have first choice, or at least will be in the best position to oust the indigenous population. Proximal land with required resources such as water also will likely be taken by the first arrivals. Later arrivals will be able to take only some (smaller) proportion of the remaining area. Land distribution in Latin America as territory settled by the Iberian arrivals after the fifteenth century is one case in point, as we shall see shortly.

The assumption of a progressive sequential inequality leads to the Pareto distribution or its mathematical equivalent, the log-exponential (Gumbel 1958, 151; Johnson and Kotz 1970, 1:240) as the theoretical distribution for the upper tail of a land distribution.<sup>4</sup> As in the specification of the exponential model for the lower portion of the distribution, the Pareto or log-exponential for the upper is also based on the assumption of land scarcity although certainly not as severe as that found in the lower portion. Indeed, the derivation of a sophisticated mathematical model of income distribution (Badger 1980) relied on the introduction of the scarcity assumption in the final steps of the derivation in order to achieve an excellent fit to the upper tail.

### Theoretical Expectations

The analysis proceeds now according to the following expectations: Where there exists scarcity of agricultural land for the peasantry, the exponential is expected to be the theoretical model for the lower portion of the land distribution. This should be so not only because of the prior finding of the applicability of this distribution to the Chinese and Salvadoran cases at the time of the outbreak of their revolutions but because of the reasoning invoked earlier. The peasantry situated on fixed

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plots of land would reproduce over time, thus leading in the limit to the exponential distribution of land holdings at the lower end. At the upper end of that distribution, however, the process should be Pareto-log-exponentially distributed. The tendency for primogeniture to prevail at this societal level or simply the financial ability to send younger sons to the city or abroad would leave most of these holdings largely intact. If there is subdivision, then it would likely occur at a much slower rate than for the peasantry with ordinarily a much larger birth rate (often necessary for working the land) and the financial inability to help male offspring other than by the process of land subdivision and inheritance.

These are the expectations for the distributions of land at the upper and lower portions of the curve. They are the limiting equilibrium statistical distributions which evolve after a relatively long time period. The feeling that the large landowner as a human being is essentially no different from the peasant can be exacerbated with extreme envy and ultimately even lead to violence if the land distributions are vastly different for peasant and large landowner and if the differences are in fact worsening as the result of differential rates of subdivision. The "limited good" theory of the peasant environment leading to extreme envy has been developed by Foster (1967) and further amplified into a theory of the "tunnel effect" by Hirschman (1981), wherein societies will tolerate extreme inequalities only for a certain time period after they are instituted, after which revolutionary behavior is to be expected if benefits do not accrue to the worst-off in society. A dichotomized agricultural universe would also more likely lead to a radicalized peasant response than a more graduated environment with inherently more options available.<sup>5</sup> As we shall soon see, El Salvador, with the history of the most intense recent conflict in Latin America has a classic

bifurcated distribution between rulers and ruled.

### Empirical Tests

Ideally, of course, one would want to have a cross-national test across as many country units as possible. Yet the preceding arguments constrain the boundaries of the country universe. First, the distribution of land must be relevant to at least some significant proportion of the population, else the theoretical connection with political violence is virtually meaningless. The numbers of persons affected by land distributions must be large enough to have at least some influence on social turmoil. This requirement effectively excludes the United States, Canada, and the European countries as well as Japan, Australia, and New Zealand, for all of these cluster at the low end of the data on percentage labor force in agriculture (Taylor and Jodice 1983, 1:208-10).

Second, the sequential acquisition process must have had some chance of operating, mostly in colonial contexts within recent history, in order to generate a bifurcated distribution. This too would exclude the European countries and, interestingly, several African countries, largely governed by the French in colonial times. Cameroon, Chad, Congo and Gabon, are examples of countries which simply do not have a distribution at the upper end that could conform to the log-exponential (Food and Agriculture Organization 1981, 36), likely because of the absence of large-scale European settlement.

Another reason for excluding Africa in addition to the heterogeneity of historical experience, language, religion and ethnicity, is the differential experience with regard to anticolonial activity. Whereas most of the African countries experienced a peaceful transition to independence, others such as Algeria, Kenya, Angola,

and Mozambique experienced violent revolutions against the colonial authorities (or their immediate legacies as in Nigeria and its bloody civil war or in Zaire), thus introducing a large domestic violence component mostly independent of recent land distributions. The majority of data available for African countries are found in the results of the 1970 *World Census of Agriculture* (Food and Agriculture Organization 1981, 33-38), which, for the most part, do not reflect the colonial experience.

The Middle East—as a more homogeneous region historically, linguistically, and even in race, religion, and ethnicity—does qualify; but much of the data available are for the early 1970s, which would be inappropriate for an examination of political violence between 1948 and 1977. Some time lag is required for the development of discontent by the peasantry. As we shall see, five countries in the Middle East do have data available from an earlier period and will be analyzed separately.

A test of the theory, therefore, requires a set of countries that can at least potentially meet the preceding requirements. Latin American countries, being for the most part agrarian and having been settled by Iberian colonists, would be suitable, as the land distributions of these countries would satisfy the requirement of sequential acquisition.<sup>6</sup> The fair degree of cultural homogeneity (i.e., relative to other regions of the world) also makes it an appropriate choice. Incidence of mass political violence is frequent enough to provide considerable numbers of instances of such violence and more importantly, variation on the dependent variable, deaths due to political violence, 1948-77 (Taylor and Jodice 1983, 2:48-51).<sup>7</sup> Land distribution data for Latin American countries during almost the entire post-World-War-II period are found in the *Statistical Abstract of Latin America* (Wilkie and Perkal 1986).

In one sense at least, the following should be viewed as a cross-national test of the theory and not as an examination of the details of Latin American politics. For I am concerned only with the validity of the proposed relationship in *any* context that would prove to be suitable, Latin America having emerged as the result of the presence of sequential acquisition in recent history and other properties of this region. Any other area of the world with a similar historical experience would be equally acceptable.

The case of El Salvador is instructive, for it is the country that has experienced the most intense class-related conflict in recent years and at the same time demonstrates one of the most clearly bifurcated distributions of any Latin American country. Table 1 gives the most recent detailed land distribution for El Salvador. The predicted values for the lower<sup>8</sup> portion of the distribution are given by the exponential distribution

$$p_i = Ae^{-kx_i} \quad k > 0, \quad 0 \leq x < \infty, \quad (1)$$

where  $p_i$  is the proportion of holdings held by the  $i$ th category  $x_i$ ;  $k$  is a constant, generally estimated from the empirical distribution; and  $A$  is a normalization constant designed to make the sum of the  $p_i$  equal to one. The log-exponential (Pareto) model for the upper portion is given by

$$p_i^1 = A_1 e^{-k_1 \log_e x_i} \quad k_1 > 0, \quad 1 \leq x < \infty, \quad (2)$$

where as before  $k_1$  and  $A_1$  (normalization) are constants.

The theoretical values in Table 1 are calculated by first linearizing the probability distributions as a consequence of taking logarithms of both sides of equations 1 and 2. This leads to

$$\log_e p_i = \log_e A - kx_i$$

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**Table 1. Tests of the Exponential and Log-Exponential Models of Landholdings in El Salvador**

Size of Holdings (Hectares) <sup>a</sup>	Observed Number of Holdings	Observed Proportion of Holdings (× 100)	Predicted Proportion of Holdings (× 100)
Lower Portion (Exponential) <sup>b</sup>			
.00-.99	107,054	55.38	52.54
1.00-1.99	48,501	25.09	25.71
2.00-2.99	22,038	11.40	12.58
3.00-3.99	8,527	4.40	6.16
4.00-4.99	7,178	3.71	3.01
$\chi^2 = .945, df = 3, p < .90$			
Upper Portion (Log-Exponential) <sup>c</sup>			
50.00- 99.99	2,214	51.16	54.80
100.00- 199.99	1,121	25.90	26.80
200.00- 499.99	713	16.47	11.18
500.00- 999.99	189	4.37	5.09
1,000.00-2,499.99	91	2.10	2.12
$\chi^2 = 2.877, df = 3, p < .50$			

<sup>a</sup>Category ranges are the same as those presented in the source (Wilkie and Perkal 1986, 36). Data are for 1960 as the last year such differentiated data are available. Values of  $a_i$  were estimated by the midpoints of each category of the size of holding.

<sup>b</sup> $k = .7147, A = .7511$ .

<sup>c</sup> $k_1 = 1.0319, A_1 = 47.1698$ .

for the exponential and

$$\log_e p^1_i = \log_e A_1 - k_1 \log_e x_i$$

for the log-exponential. The values of  $k$  and  $k_1$  are estimated by regressing  $\log_e p_i$  and  $\log_e p^1_i$  on the independent variables and the regression coefficients serve as least squares estimates. The values of  $A$  and  $A_1$  are chosen to normalize the equations so that the sum of the proportions equals unity, or

$$A = \frac{1}{\sum_{i=1}^n e^{-kx_i}}$$

and

$$A_1 = \frac{1}{\sum_{i=1}^n e^{-k_1 \log_e x_i}}$$

Predicted and observed distributions are compared by means of the chi-square goodness-of-fit statistic. A higher probability value for the chi-square statistic implies a better fit between observation and prediction while a lower value implies a poorer fit. In order to adopt a conservative policy on acceptance of either model (equations 1 or 2), a model will be rejected at  $p < .20$  (instead of the usual  $p < .05$ ), which indicates of course that any probability levels higher than .20 imply acceptance of the model. Acceptance or rejection will be modified in one instance to allow for the introduction of approximations to continua that are probably better reflective of social reality.

Both chi-square values for the upper and lower distributions in El Salvador are well above the chosen probability level and in fact indicate an excellent fit between observation and prediction. This of

**Table 2. Results of the Exponential and Log-Exponential Analyses of Latin American Land Distributions**

Country	Upper (Log-Exponential)			Lower (Exponential)		
	$\chi^2$ <sup>a</sup>	df	p <	$\chi^2$	df	p <
Argentina (1960)	4.433	3	.30			
Bolivia (1950) <sup>b</sup>	4.381	4	.50	3.584	2	.20
Brazil (1960)	3.815	3	.30			
Chile (1950) <sup>c</sup>	3.126	3	.50	1.310	3	.80
Colombia (1960)	2.862	3	.50	4.086	4	.50
Costa Rica (1960)	4.167	2	.20			
Cuba (1952)	.120	3	.99			
Dominican Republic (1960) <sup>d</sup>	1.717	3	.70			
Ecuador (1954)	1.205	3	.80			
El Salvador (1960)	2.877	3	.50	.945	3	.90
Guatemala (1950)	2.737	2	.30			
Haiti (1970) <sup>e</sup>	1.672	3	.70			
Honduras (1960)	1.021	3	.80			
Mexico (1960)	2.696	5	.80			
Nicaragua (1960)	3.712	3	.30			
Panama (1960)	.302	2	.90			
Paraguay (1960)	6.230	6	.50			
Peru (1961) <sup>d</sup>	1.186	3	.80	3.057	2	.30
Uruguay (1960)	5.680	3	.20			
Venezuela (1960)	1.287	3	.80			

Source: Wilkie and Perkal 1986, except where noted.

<sup>a</sup>Where the expected value was below 1.5, adjacent categories were combined for the chi-square test until that figure was obtained, as suggested in Gibbons 1971 (p. 72).

<sup>b</sup>Heath, Erasmus, and Buechler 1969 (p. 35).

<sup>c</sup>Log-exponential lower.

<sup>d</sup>Food and Agriculture Organization 1966-70.

<sup>e</sup>Exponential upper.

course is but one illustration; however, it does suggest that I perform a general systematic analysis of the relationship between patterned inequality as defined by the two segments of land distribution, and mass political violence in Latin America.

Note that the observed distributions are aggregations of landholdings of similar sizes, in the ranges specified, from all parts of a single country. Thus, it is probable that a single hacienda or other large holding will be surrounded by much smaller holdings. It is possible to imagine the growing decrement of identification between the large and small landowner, if it ever existed in large measure, as the

smallholdings subdivide over time while the hacienda or large holding in the same vicinity remains mostly intact.<sup>9</sup> These data then aggregate the number of such instances over the entire country, yielding the two distributions shown in Table 1.

Table 2 lists the results of the chi-square goodness-of-fit tests between the theoretical distributions and the observed values, first for the upper portion of the distributions and then for the lower. The year for each distribution was chosen based on the availability of the most detailed information for that distribution. Where the amount of information given was equal for two years, the earlier one

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was chosen to allow for the development and mobilization (generally slow) of anti-regime organizations (Tilly 1978), if that indeed were to be the case. Only a few countries are listed for the lower portion because so many can be rejected simply by inspection; they are not monotonically decreasing as demanded by the exponential distribution but rise to a given peak and then fall. Most of the cases, however, do conform to the log-exponential for the upper portion as suggested by the theory of sequential acquisition.

Five distinct patterns emerge from the table: instances (1) where both models can be rejected, (2) where only the exponential is applicable to the upper portion, (3) where only the log-exponential is applicable to the upper, (4) where the log-exponential is applicable to the upper and there is a near applicability (to be defined) of the exponential to the lower portion and (5) where both are applicable respectively to their theoretically suggested domains.

Five categories of increasing violence potential are now suggested for the examination of the proposed relationship. They arise from the five groupings that emerged from Table 2. First, both models can be rejected—a category that is to be associated with the least amount of violence—for neither model is applicable. Next we find the exponential—a less severe inequality distribution than the log form (see the Appendix)—associated with the upper portion but not the lower, followed by the log-exponential for the upper, but again no goodness of fit of any distribution for the lower portion. The next to highest category of violence potential would have the log-exponential fit the upper portion and a *near* fit of the exponential to the lower portion.

This near fit can be accomplished in one of two ways. There can be doubt as to whether the exponential or some other distribution, such as the log-exponential, fits the data. This would eliminate the

bifurcation suggested by theory, as well as possible identifications among the peasantry because of the increased degree of inequality within that societal sector. The second way a land distribution can be included in this category is by obtaining  $p < .20$  for the exponential distribution in the lower portion in addition to the precise fit of the log-exponential to the upper—a near overall fit but not precisely meeting the adopted criterion. Finally, the precise fit of the two models to their respective portions of the distribution is to be included in the highest-potential-violence category. Effectively, an incremental procedure is used to accommodate the variation in fit of these models to the data in order to begin to approximate a continuum of applicability.

Deaths from political violence (Taylor and Jodice 1983, vol. 2) is treated in a similar fashion. The range for the 20 Latin American countries (as identified in Wilkie and Perkal 1986 [pp. 34–35]) is segmented into quintiles with four countries in each. This particular variable is chosen because it can reflect the experience of instability from various sources. There is no necessity for the agrarian structure described here to eventuate in any one type of instability. There can occur a variety of either anomic or organized, violent activities simply based on the availability of a large number of persons with severe disaffection from the prevailing authority structure. An additional element in favor of the deaths from political violence variable is the fact that reporting of deaths tends to be more accurate than other measures of instability (Weede 1981).

Table 3 lists the cross tabulation of deaths by the five patterns that emerged from Table 2. The value of  $\tau$ - $b$  is significant at  $p < .005$  as can be seen in the table. In addition, the value of gamma that is conditional on a distribution of this type without tied ranks is fairly robust at .70. These values should be compared

**Table 3. Association between Patterned Inequality and Political Violence for the 20 Latin American Countries**

Political Violence	Patterned Inequality				
	1 None	2 Upper Exp	3 Upper Log-Exp	4 Near Both	5 Both
5			Argentina Nicaragua		Colombia El Salvador
4			Cuba Dominican Republic Venezuela	Bolivia	
3			Guatemala Mexico	Chile	Peru
2		Haiti	Brazil Ecuador Paraguay		
1	Costa Rica Uruguay		Honduras Panama		

Note:  $\tau_b = .51$ ,  $p < .005$ ,  $\Gamma = .70$ .

with that between the Gini index of land inequality itself (Taylor and Hudson 1972, 267-68, in order to maximize the number of cases) and the same violence variable with tau-*b* equal to .36 ( $N = 15$ , significant at  $p < .05$ ) and a value of gamma equal to .43. When compared with the results of the analysis of Table 3, there has been a tenfold increase in significance level for the patterned inequality variable and its relationship to political violence over and above the Gini index.

It is further instructive to consider the relationship between patterned inequality and the Gini index. A value of tau-*b* equal to .27 ( $N = 15$ , not significant at  $p < .05$ ) was calculated with an associated value of gamma equal to .40. These statistics suggest a low level of association between the two measures of inequality. They further suggest that generalized land inequality, at least as measured by the Gini index in the Latin American context, either is more heavily influenced by random factors or is tapping a somewhat different dimension than is the patterned inequality variable.

We should note here that the finding of

a significant relationship at  $p < .05$  between the Gini index and political violence is thoroughly consistent with earlier findings of either a barely significant relationship, just nonsignificant, or just about at the significance level as was found here (actually  $p = .048$ ). This suggests that this finding conforms to the majority of studies undertaken thus far, and perhaps even implies some degree of generalizability of these findings beyond the strict confines of Latin America. What distinguishes the present analysis from these earlier ones using the Gini index is the substantially improved degree of association between patterned inequality and political violence.

There is an additional aspect of Table 3 to be noted. This is the commonality of experience of all countries in the fourth to fifth quintiles for patterned inequality. All of them—Chile, Peru, Bolivia, Colombia, and El Salvador—experienced either intense political violence or redistributive governments.

In the case of Chile, a Marxist-redistributive government—that of Salvador



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Allende—received a plurality of votes in 1970 (and even earlier in 1964) leading to a government whose policies were sufficiently antithetical to the preferences of the military to lead to the coup of 1973 and the subsequent repression. In Peru, the 1968 coup by the military completed the work begun by the 1962 effort and a major land redistribution was undertaken. Bolivia experienced the successful revolution of 1952 with its associated violence and land redistribution. Colombia and El Salvador, with both models entirely applicable (cell no. 5-5 in Table 3), of course, have experienced intense violence, the former in *La Violencia* stretching approximately from 1948 until its gradual decline in the late 1950s and denouement in 1964 (Dix 1967; Hamby 1986; Oquist 1980). It is of interest that the land redistribution of 1961, which was not insubstantial (Wilkie and Perkal 1986, 39), apparently was instrumental in ending *La Violencia* several years later. The El Salvador violence of course has been extremely intense and is still ongoing with no end in sight. At the other end of the diagonal of Table 3, we have Uruguay and Costa Rica which in most respects have been among the least prone to violence of any of the Latin American countries and at the same time both theoretical distributions do not apply.<sup>10</sup>

Variation in political violence but none in pattern of holdings in the middle column suggests the invariance properties of the Pareto-log-exponential distribution. It is known that the Pareto-Lévy-type distribution is only one of two types of distribution (the other being the Gaussian) that are invariant upon the addition of other random variables (Mandelbrot 1960, 1961). Thus it would be expected that once formed, only serious redistribution would be capable of breaking the pattern of the Pareto-log-exponential and we find this to be the case in the conformity of so many of these countries to this model in the upper portion of the dis-

tribution.

The results for Latin America are encouraging. Are there other regions of the world that also exhibit this association between patterned inequality and political violence? There does exist another region of the world with a similar kind of homogeneity (relative to other regions of course)—the Middle East. There also can exist here a sequential parceling of land for the upper portion of the distribution, as in a massive land reshuffling for both rulers and ruled, as frequently occurs upon the advent of a new political order (Wesson 1967, 63–65). The frequency of such change of course has been endemic to the Middle East. Unfortunately, early data are not available for all countries of the Middle East and as a consequence we cannot perform a systematic test as was done for Latin America. Nor can we really combine the two regions, for, as we shall see, there is a somewhat different pattern of landholding for the Middle East, which does not readily integrate with that for Latin America. Nevertheless, it is instructive to examine the five countries for which data are available at an early enough time to predict to the revolutions of the 1950s and 1960s.

Egypt and Lebanon have both portions of their distributions conform to the expected theoretical models and would fit in the fifth column of Table 3. The pattern for Egypt is shown in Table 4 with excellent degrees of fit between observation and prediction. Iraq's pattern of landholdings is the same as that of Bolivia and Chile (a fit of the log-exponential to the upper and a near fit of the exponential to the lower), while that for Iran and Turkey is the expected fit of the exponential model to the lower portion of the distribution but no fit of either model to the upper. (This is the major departure from the Latin American pattern, suggesting perhaps that a fairly recent historical settlement is required for a regional uniformity of fit of the log-exponential to the

upper portion.) The findings are shown in Table 5.

Results pertaining to redistributive political violence generally support those found for Latin America. As in the cases of Bolivia, Colombia, and El Salvador—the three Latin American instances most closely conforming to the theory—Egypt, Iraq, and Lebanon either experienced redistributive revolutions or have been experiencing intense political violence.

The Egyptian revolution of 1952 led by Naguib and Nasser resulted in an almost immediate land redistribution (extended later in 1961) as did the 1958 revolution of Kassem in Iraq. Lebanon, of course, has been experiencing the most intense sort of political violence, with a highly complex bifurcation approximating roughly the Christian-Muslim division.

The two remaining cases, Turkey and Iran, are of interest because of their

**Table 4. Tests of the Exponential and Log-Exponential Models of Landholdings in Egypt**

Size of Holdings (Hectares) <sup>a</sup>	Observed Number of Holdings	Observed Proportion of Holdings (× 100)	Predicted Proportion of Holdings (× 100)
<b>Lower Portion (Exponential)<sup>b</sup></b>			
.0- .4	434,219	31.44	36.55
.4- .8	385,901	27.94	25.82
.8- 1.3	286,804	20.76	17.46
1.3- 1.7	174,595	12.64	11.81
1.7- 2.1	99,722	7.22	8.34
$\chi^2 = 1.721, df = 3, p < .70$			
<b>Upper Portion (Log-Exponential)<sup>c</sup></b>			
2.1- 4.2	170,019	66.17	64.62
4.2- 8.4	56,705	22.07	24.90
8.4-21.0	23,811	9.27	7.76
21.0-42.0	6,424	2.50	2.72
$\chi^2 = .670, df = 2, p < .80$			

<sup>a</sup>The data source is the Food and Agriculture Organization (1970, 212-13) and the year is 1961. Although listed under the name of the United Arab Republic, the data refer only to Egypt.

<sup>b</sup> $k = .8689, A = .4349,$

<sup>c</sup> $k_1 = 1.3758, A_1 = 3.1327,$

**Table 5. Results of the Exponential and Log-Exponential Analyses of Middle Eastern Land Distributions**

Country	Upper (Log-Exponential)			Lower (Exponential)		
	$\chi^2$	df	p <	$\chi^2$	df	p <
Egypt (1961)	.670	2	.80	1.721	3	.70
Iran (1960)	7.984	2	.02	2.905	4	.70
Iraq (1958)	.751	2	.70	7.248	4	.20
Lebanon (1961)	1.069	2	.70	3.457	4	.50
Turkey (1963)	11.513	2	.01	2.330	4	.70

Source: Food and Agriculture Organization 1966-70.

departures from the other patterns. These countries in the recent past did not—as their land distributions would suggest—experience serious redistribution.<sup>11</sup> And the Ataturk revolution in Turkey after World War I was not essentially redistributive but more cultural and political, as Turkey sought to Westernize rapidly. Khomeini's revolution of course was urban-based initially (Nejad 1986) and then increasingly relied on the Shiite clergy as its revolutionary vanguard to the exclusion of most redistributive questions aside from the shah's wealth and that of his supporters.

The Egyptian case deserves additional mention because of its intrinsic interest in light of the current theory. Note first that the excellent fit of the models to both the upper and lower portions of the land distribution (1961) occurred *after* the redistribution of 1952. This suggests that the implied state of patterned inequality prior to the redistribution of 1952 must have been horrendous.<sup>12</sup> It further implies that despite the earlier redistribution, the very rapid population growth—one of the world's fastest—along with encroachments of the desert onto the Nile Valley, diminishing the amount of arable land, was sufficient either to maintain or to create anew a state of patterned inequality. Now perhaps we can understand the readiness of the United States to support the Egyptian government, the second highest aid recipient, in the amount of several billion dollars per year. The state of patterned inequality worsening under rapid population growth and increased land scarcity could lead to a new, more extreme redistributive revolution, with a likely increased Soviet involvement in the country and region.

### The Gini Index and Related Findings

Although the use of patterned inequality has shown strong improvement over

the Gini index in a systematic analysis, it will be useful to compare individual country scores on the two measures. In so doing we also will be able to reflect on the validity of recent findings claiming the relative unimportance of land inequality in the explanation of political violence (Muller and Seligson 1987). As one would expect, in extreme cases there is agreement between the two methods. In the Muller and Seligson study Haiti received a Gini index of inequality of .47; and in the present study Haiti had only the exponential distribution—an indicator of less inequality—fit the upper part of the land distribution. Given this relatively low level of inequality found in both studies, it is likely that the recent Haitian violence is far more a product of a population mostly unified against the political thuggery left over from the personalist Duvalier regime(s) than of any class-related bifurcation in land holdings. Beyond this agreement at the margins, the two approaches diverge strongly.

The most important of these divergences is that the Gini index simply does not reveal important differences between cases and perhaps even distorts them. Some of these are egregious. Costa Rica and Uruguay were found by Muller and Seligson (1987, 445–46) to have a slightly higher Gini index of land concentration (both at .82) than does El Salvador (.81); yet the first two countries fall into the lowest quintile of both patterned inequality and political violence in this study, and El Salvador the highest (Table 3). Guatemala was found by Muller and Seligson to have exactly the same value as Costa Rica, yet has an intermediate position in patterned inequality between Costa Rica and El Salvador in this study. Mexico and Venezuela were reported to have the highest Gini values (.93 and .91, respectively) of all Latin American countries (along with Peru at .91) but have intermediate positions here and generally moderate levels of political violence. Nicaragua, also intermediate here in pat-



terned inequality, has virtually the same value (.80) as El Salvador (.81) despite the basic differences in revolutionary process experienced by the two countries.

Similarly, in the Middle East data we find serious anomalies. Iran and Iraq have experienced very different revolutionary processes, as would be suggested by the different patterns of landholdings identified in Table 5, yet have almost the same Gini value in Muller and Seligson (.63 and .61, respectively). Israel (Food and Agriculture Organization 1981, 37) and Lebanon here also have very different patterns of landholdings (Israel's is not even monotonically decreasing in the lower range) and histories of domestic political violence yet have almost identical Gini values (.75 and .77, respectively).

While the Gini index can isolate the fundamentally different instances as in the case of Haiti, if there are substantial inequalities but of different patterns and forms, then the Gini index is virtually useless as an instrument of differentiation among them. Worse yet, in certain instances, it may actually distort the findings by reversing the rank order of inequality most relevant to politics (Costa Rica and Uruguay vs. El Salvador). If one seeks to disclose relationships between inequality and political violence, the more sensitive procedure and one with stronger systematic variation with the dependent variable is to be preferred.

Although in a strict sense, the findings here pertain only to Latin America with some extension to the Middle East, they clearly have strong implications for any systematic study that uses the Gini index as a measure of inequality. The findings here are especially relevant for studies that claim to have found no direct relationship between the Gini index of land inequality and a dependent variable such as political violence, or at most an indirect relationship (Muller and Seligson 1987); for clearly now a more sensitive measure is available to distinguish among

seemingly undifferentiated cases. In the final analysis, a major advantage of the patterned inequality measure is that it is theoretically based, in contrast to the Gini index, which is simply a mathematical measure (the area between one curve and another), without theoretical content.

### Conclusion

The analysis has shown that the patterned inequalities implied by the two models for land distribution bear a strong association with political violence in Latin America, with supportive evidence emerging from the Middle East. Patterned inequalities of this type constitute better predictors of political violence than more generalized but less context-specific measures like the Gini index. This measure is subject to random influences throughout the range of holdings in contrast to the more focused analysis here on the patterns that obtain at the two ends of the land distribution.

At the same time, we have seen that political violence is not the inevitable outcome of patterned inequalities. Redistributive measures can be undertaken even after a military coup with relatively few deaths associated with it, as in Peru. The current extreme economic difficulties in Egypt can be alleviated, at least temporarily, by the massive infusion of monies from abroad. The effectiveness of such policies still must be tempered with caution; for despite the redistribution in Peru, there is an ongoing rural guerrilla insurrection suggested in part by the patterned inequality of Table 3, wherein Peru is found in the highest inequality category, perhaps not sufficiently equalized by the later redistributive policies.

One caveat: it must be remembered that the mostly agrarian status of many of these countries has been changing over time. Although the findings of this study apply to the post-World-War-II period

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**Table A-1. Illustrations of Exponential and Log-Exponential  
(Pareto) Distributions**

Resource Units (Exponential)	Population	Proportion of Population	Resource Units (Log-Exponential)
2	10,000	.5020	1
4	5,000	.2509	2
6	2,500	.1255	4
8	1,250	.0627	8
10	625	.0314	16
12	312.5	.0157	32
14	156.25	.0078	64
16	78.125	.0039	128

and provide an explanatory basis for the occurrence of political violence in that time period, the results concerning land distributions alone may be less applicable to the future if urbanization increases rapidly in the developing world. On the other hand, the contemporary relevance of El Salvador, Peru, Egypt, and Lebanon is manifest in the realm of political violence with perhaps portent for the future in those societies that will continue to have a significant agrarian component.

Finally, the theory and analysis here have put forward a specific referent for those who may feel themselves deprived relative to some other social entity. This is the distribution of holdings that differs from one societal sector to another. The significance of the different distributions is not only that they suggest different patterns of inequality as indicated in this study but that they imply a degree of *predictability* to the impoverishment of the peasantry—the inevitable subdivision of holdings—relative to another land distribution that is predicted to change at a much slower rate if at all. If the preceding mode of investigation could be generalized to other scarce utiles, then it might serve as a basis for future extended research on the sources of political violence.

### **Appendix: Comparison of Exponential and Log-Exponential Distributions**

Intuitively, it appears obvious that the log-exponential (Pareto) distribution represents a greater degree of inequality than the exponential itself. Perhaps an example will fix ideas more exactly. Table A-1 presents eight categories of resource holdings (land, financial, or industrial). To represent the scarcity condition, the total number of resource units is approximately equal for both distributions (78,438 vs. 80,000—it is difficult to achieve exact equality) but sufficiently close for our illustrative purposes. The proportions of the population also are held constant; all that is allowed to vary is the exponential versus log-exponential distributions of these holdings. (I will now refer to this distribution as log-exponential to emphasize both its connection and contrast with the exponential distribution of scarce resources. Clearly it is also understood to be a Pareto distribution.) Even the overall means are approximately equal with  $\bar{x} = 3.9362$  for the exponential and  $\bar{x} = 4.0106$  for the log-exponential.

Columns 1 and 3 together yield an exponential distribution, while columns 3

and 4 yield a log-exponential. Comparison of each of the categories indicates the extent of inequality. First, the richest 6% of the population hold 50% of the resources in the log-exponential, with a mean of 34.13; while in the exponential distribution, the top 6% hold 17% of the resources, with a mean of 11.47, approximately one-third the mean value of the log-exponential. In the log-exponential, the poorer 75% of the population own 25% of the resources with a mean of 1.33; while in the exponential they own 51% with twice the mean at 2.66. The rich are richer and the poor are poorer in this distribution of extreme inequality. And this is in addition to the blatant inequality already inherent in the exponential distribution itself. Thus, in the analyses of this study, when the lower (peasant) portions of the holdings subdivide, leading to the exponential distribution at this level, the resultant inequality is built upon and added to the substantial inequality already existing between the upper and lower portions of the log-exponential distribution.

## Notes

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1. This point is developed at greater length in Midlarsky 1983 (pp. 195-99).

2. This study departs from the relative deprivation tradition not in the denial of its basic postulates, such as *frustration leads to aggression*, but in emphasis. Instead of the frustration-aggression nexus, the present analysis emphasizes identification between rulers and ruled as a basis for political stability that would be seriously disrupted under conditions of bifurcated landholdings and associated patterned inequalities. This form of inequality can be interpreted as a structural condition that can be added to a self-generative model of political violence (Lichbach and Gurr 1981) for a more complete explanation of this phenomenon.

3. As Finley (1982, 214) remarked in connection with Mycenaean peasants,

Illiterate peoples, even rather primitive ones, are capable of considerable feats of memory in the ordinary routines of living. They transmit their mythologies and genealogies, they sort out rather complicated kinship patterns, they know exactly where their hunting and agricultural lands are located, and what the precise status of obligations is at any moment in potlatch or bride-wealth arrangements—all without any records whatsoever. To them it would be accepted as a matter of course that Eumaeus, without consulting documents, could say to a stranger: "My master is so rich that not twenty men together have so much wealth. I will enumerate it for you: twelve herds of cattle on the mainland, twelve of sheep, twelve of swine, and so on."

4. Midlarsky (n.d.) includes a geometric derivation of this distribution applied to colonial holdings. The Pareto-log-exponential distribution, of course, can be derived using other approaches, such as a Markov chain that incorporates a stability condition of decreasing income (Champernowne 1953) or a multiplicatively increasing mean (West 1985, 89). However one arrives at this distribution, it is markedly different from the exponential that is the essence of the theory of bifurcation offered here.

5. Recent evidence has emerged from another cultural context that suggests a political radicalization, especially in the egalitarian direction, of those who perceive a dichotomous social universe (Zaborowski 1986, 117).

6. Two Latin American countries, Argentina and Uruguay, are situated just above the European countries in percentage labor force in agriculture (Taylor and Jodice 1983, 1:209-10). It was decided not to exclude them from this study (1) for the sake of completeness of the country universe and (2) more importantly because in contrast to the European countries and Japan the sequential acquisition process occurred in the Latin American countries more recently, thus distinguishing these countries from those just below them in percentage labor. The political residues of such an agrarian bifurcation could still affect contemporary social turmoil, even as the country moves away from dependence on the agrarian sector. It has been estimated that at least 70 years are required to establish a stable party system (Converse 1969), a figure that is likely equal to that required to reverse the remains of earlier political conflict.

7. In order to update the dependent variable, more recent (until 1982) data on deaths from domestic violence were obtained from the Inter-university Consortium on Political and Social Research. However, very little difference was observed between these data and the published data (until 1977) even for countries, such as El Salvador and

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## Mass Political Violence

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Nicaragua, that are known to have experienced intense violence in this period. LaFeber (1983, 239) for example estimates the dead in Nicaragua during this period to be on the order of tens of thousands, which would place Nicaragua in the first quintile of deaths. As a result, both Nicaragua and El Salvador were placed in the upper quintile with no other changes effected in the 1977 data.

8. The point at which the exponential was chosen to end, and the log-exponential to begin, was selected by inspection. As a result of this, the degrees of freedom will vary from case to case, in addition to the differences in reporting of landholdings by various countries. In many instances, the point was fairly clear, as shown in Table 1, with, in most instances, the middle of the distribution omitted because of mixing of the two models over time. Where there was doubt as to the accuracy of the point selected, at least one point below and one above were also selected for additional calculations to ensure that the reported findings represent the best possible fit between model and data.

9. Although the land distribution data are not available for Latin America prior to World War II—so we cannot examine long-term effects of this type for this region—one can see such a pattern in Irish landholdings (*Agricultural Statistics* 1913 [for 1912]; *Returns* 1854 [for 1853]). Whereas the upper distribution remains largely the same between 1852 (contained in the 1853 report) and 1912 (e.g., 20,436 holdings between 100 and 200 acres in 1852 and 22,728 in 1912), the lower has changed radically wherein there were 35,058 holdings with less than 1 acre in 1852 but 87,451 in 1912. Concomitantly, there was a decline in the number of somewhat larger holdings of 1–5 and 5–15 acres between 1852 and 1912, which almost exactly equaled the rise in the number of holdings with less than 1 acre, thus directly indicating the subdivision process. Of course, the absence of identification between the Irish Catholics and Protestant “ascendancy” situated on the larger, relatively unchanging holdings was evident and likely contributed strongly to the eventual break between Great Britain and Ireland in 1922.

10. Although the Tupamaros were active in Uruguay in the late 1960s and 1970s, they were urban guerrillas, without significant connections with the countryside. Even with their guerrilla activity, the total deaths from political violence in Uruguay still place it in the lowest quintile for all Latin American countries (Taylor and Jodice 1983, 2:50).

11. In the early 1960s, the shah effected the so-called white revolution, which included, as a major emphasis, land reform. However, as has been widely observed (see for example Hooglund 1982 or Goodell 1981), this reform was inadequate because of the sizes of the new holdings and financial procedures that ended in the impoverishment of much of the peasantry and their relocation to the city.

12. The earlier distribution (circa 1950 [Tuma 1965, 148]) indeed suggests a degree of inequality that far exceeds that reported here, and appears to approximate the log-exponential throughout the entire range of holdings. The absence of a finer breakdown for the lower categories (especially 1–5 feddan) prevented a systematic test of these data along the lines of the material in Table 4.

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# POLITICAL INTOLERANCE AND POLITICAL REPRESSION DURING THE McCARTHY RED SCARE

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*I test several hypotheses concerning the origins of political repression in the states of the United States. The hypotheses are drawn from the elitist theory of democracy, which asserts that repression of unpopular political minorities stems from the intolerance of the mass public, the generally more tolerant elites not supporting such repression. Focusing on the repressive legislation adopted by the states during the McCarthy era, I examine the relationships between elite and mass opinion and repressive public policy. Generally it seems that elites, not masses, were responsible for the repression of the era. These findings suggest that the elitist theory of democracy is in need of substantial theoretical reconsideration, as well as further empirical investigation.*

Over three decades of research on citizen willingness to "put up with" political differences has led to the conclusion that the U.S. public is remarkably intolerant. Though the particular political minority that is salient enough to attract the wrath of the public may oscillate over time between the Left and the Right (e.g., Sullivan, Piereson, and Marcus 1982), generally, to be much outside the centrist mainstream of U.S. politics is to incur a considerable risk of being the object of mass political intolerance.

At the same time, however, U.S. public policy is commonly regarded as being relatively tolerant of political minorities. Most citizens believe that all citizens are offered tremendous opportunities for the expression of their political preferences (e.g., McClosky and Brill 1983, 78). The First Amendment to the U.S. Constitution is commonly regarded as one of the most uncompromising assertions of the right to freedom of speech to be found in

the world ("Congress shall make no law. . ."). Policy, if not public opinion, appears to protect and encourage political diversity and competition.

The seeming inconsistency between opinion and policy has not gone unnoticed by scholars. Some argue that the masses are not nearly so intolerant as they seem, in part due to biases in the questions used to measure intolerance (e.g., Femia 1975) and in part because the greater educational opportunity of the last few decades has created more widespread acceptance of political diversity (e.g., Davis 1975; Nunn, Crockett, and Williams 1978). Most, however, are willing to accept at face value the relative intolerance of the mass public and the relative tolerance of public policy but to seek reconciliation of the seeming contradiction by turning to the processes linking opinion to policy. Public policy is tolerant in the United States because the processes through which citizen preferences are linked to government action do not faith-

fully translate intolerant opinion inputs into repressive policy outputs. Just as in so many other substantive policy areas, public policy concerning the rights of political minorities fails to reflect the intolerant attitudes of the mass public.

Instead, the elitist theory of democracy asserts, policy is protective of political minorities because it reflects the preferences of elites, preferences that tend to be more tolerant than those of the mass public. For a variety of reasons, those who exert influence over the policy-making process in the United States are more willing to restrain the coercive power of the state in its dealings with political opposition groups. Thus there is a linkage between policy and opinion, but it is to *tolerant elite opinion*, not to *intolerant mass opinion*. Mass opinion is ordinarily not of great significance; public policy reflects elite opinion and is consequently tolerant of political diversity. The democratic character of the regime is enhanced through the political apathy and immobility of the masses, according to the elitist theory of democracy.<sup>1</sup>

The elitist theory nonetheless asserts that outbreaks of political repression—when they occur—are attributable to the mass public. While the preferences of ordinary citizens typically have little influence over public policy—in part, perhaps, because citizens have no real preferences on most civil liberties issues—there are instances in which the intolerance of the mass public becomes mobilized. Under conditions of perceived threat to the status quo, for example, members of the mass public may become politically active. In the context of the general propensity toward intolerance among the mass public, mobilization typically results in demands for political repression. Thus, the elitist theory of democracy hypothesizes that political repression flows from demands from an activated mass public.

The theory of “pluralistic intolerance”—recently proposed by Sullivan, Piereson,

and Marcus (1979, 1982) and Krouse and Marcus (1984)—provides a nice explanation of the process through which mass intolerance is mobilized (see also Sullivan et al. 1985). The theory asserts that one of the primary causes of political repression is the *focusing* of mass intolerance on a specific unpopular political minority. To the extent that intolerance becomes focused, it is capable of being mobilized. Mobilization results in demands for political repression, demands to which policymakers accede. The authors claim support for their theory from recent U.S. history:

During the 1950s, the United States was undoubtedly a society characterized by considerable consensus in target group selection. The Communist Party and its suspected sympathizers were subjected to significant repression, and there seemed to be a great deal of support for such actions among large segments of the political leadership as well as the mass public. . . . The political fragmentation and the proliferation of extremist groups in American politics since the 1950s has undoubtedly resulted in a greater degree of diversity in target group selection. If this is the case, such a situation is less likely to result in repressive action, even if the mass public is roughly as intolerant as *individuals* as they were in the 1950s (Sullivan, Piereson, and Marcus 1982, 85, emphasis in original).

Thus both the elitist theory of democracy and the theory of pluralistic intolerance are founded upon assumptions about the linkage between opinion and policy.

Despite the wide acceptance of the elitist theory of democracy, there has been very little empirical investigation of this critical linkage between opinion and policy.<sup>2</sup> Consequently, this research is designed as an empirical test of the policy implications of the widespread intolerance that seems to characterize the political culture of the United States. Using data on elite and mass opinion and on public policy in the states, the linkage hypothesis is tested. My focus is on the era of the McCarthy Red Scare, due to its

## Intolerance and Repression

political and theoretical importance. Thus I assess whether there are any significant policy implications that flow from elite and mass intolerance.

### **Public Policy Repression**

#### **Conceptualization**

A major impediment to drawing conclusions about the linkage between political intolerance and the degree of repression in U.S. public policy is that rigorous conceptualizations and reproducible operationalizations of policy repression do not exist. Conceptually, I define *repressive public policy* as statutory restriction on *oppositionist political activity* (by which I mean activities through which citizens, individually or in groups, compete for political power [cf. Dahl 1971]) upon some, but not all, competitors for political power.<sup>3</sup> For example, policy outlawing a political party would be considered repressive, just as would policy that requires the members of some political parties to register with the government while not placing similar requirements on members of other political parties. Though there are some significant limitations to this definition, there is utility to considering the absence of political repression (political freedom) as including unimpaired opportunities for all full citizens

1. to formulate their preferences
2. to signify their preferences to their fellow citizens and the government by individual and collective action
3. to have their preferences weighted equally in the conduct of the government, that is, weighted with no discrimination because of the content or source of the preference (Dahl 1971, 1-2).

That is the working definition to be used in this research.

#### **Operationalizing Political Repression—the 1950s**

There have been a few systematic attempts at measuring political repression as a policy output of government. Bilson (1982), for instance, examined the degree of freedom available in 184 polities, using as a measure of freedom the ratings of the repressiveness developed by Freedom House. Dahl provides system scores on one of his main dimensions of *polyarchy* (opportunities for political opposition) for 114 countries as they stood in about 1969 (Dahl 1971, 232). In their various research reports Page and Shapiro (e.g., 1983) measure civil rights and civil liberties opinions and policies in terms of the adoption of specific sorts of public policy. Typically, however, the endogenous concept in most studies of state policy outputs is some sort of expenditure variable. (See Thompson 1981 for a critique of this practice.) These earlier efforts can inform the construction of a measure of political repression in the policy outputs of the American states.

The measure of policy repression that serves as the dependent variable in this analysis is an index indicating the degree of political repression directed against the Communist party and its members during the late 1940s and 1950s. A host of actions against Communists was taken by the states, including disqualifying them from public employment (including from teaching positions in public schools); denying them access to the ballot as candidates, and prohibiting them from serving in public office even if legally elected; requiring Communists to register with the government; and outright bans on the Party. Forced registration was a means toward achieving these ends.

Of the fifty states, twenty-eight took none of these actions against Communists.<sup>4</sup> Two states—Arkansas and Texas—banned Communists from the ballot and from public employment, as well as banning the Party itself and requiring that

Communists register with the government. Another five states adopted all three measures against the Communists, but did not require that they register with the government. Pennsylvania, Tennessee, and Washington did not formally bar Communists from public employment but did outlaw the party and forbade its members from participating in politics. The remaining twelve states took some, but not all, actions against the Communists. From these data, a simple index of political repression has been calculated. The index includes taking no action, banning Communists from public employment, banning Communists from running candidates and holding public office, and completely banning Communists and the Communist Party. A "bonus" score of .5 was given to those states requiring that Communists register with the government.<sup>5</sup> Table 1 shows the scores of the individual states on this measure.

This measure can rightly be considered to be a valid indicator of political repression by the states.<sup>6</sup> In asserting this I do not gainsay that the state has the right—indeed, the obligation—to provide for its internal security. Consequently, statutes that prohibit such actions as insurrection do not necessarily constitute political repression. For instance, Texas made it unlawful to "commit, attempt to commit, or aid in the commission of any act intended to overthrow" the Texas government (Art. 6689-3A, Sec. 5). This section proscribes action, not thought or speech, and is therefore not an appropriate measure of political repression. However, the next subsection of the statute made it illegal to "advocate, abet, advise, or teach by any means any person to commit" a revolutionary act. Indeed, even conspiracy to advocate is prohibited (Art. 6889-3A, Sec. 5 [3]). This is indeed a constraint on the speech of political minorities and therefore is treated as repressive. As the action prohibited moves beyond a specific, criminal behavior, the line be-

tween repressive and nonrepressive legislation becomes less clear. Gellhorn (1952) commented,

Traditionally the criminal law has dealt with the malefactor, the one who himself committed an offense. Departing from this tradition is the recent tendency to ascribe criminal potentialities to a body of persons (usually, though not invariably, the Communists) and to lay restraints upon any individual who can be linked with the group. This, of course, greatly widens the concept of subversive activities, because it results, in truth, in forgetting about activities altogether. It substitutes associations as the objects of the law's impact. Any attempt to define subversion as used in modern statutes must therefore refer to the mere possibility of activity as well as to present lawlessness (p. 360).

There can be little doubt as to the effectiveness of this anti-Communist legislation. Not only were the Communist Party U.S.A. and other Communist parties essentially eradicated, but so too were a wide variety of non-Communists. It has been estimated that of the work force of 65 million, 13 million were affected by loyalty and security programs during the McCarthy era (Brown 1958). Brown calculates that over 11 thousand individuals were fired as a result of government and private loyalty programs. More than 100 people were convicted under the federal Smith Act, and 135 people were cited for contempt by the House Un-American Activities Committee. Nearly one-half of the social science professors teaching in universities at the time expressed medium or high apprehension about possible adverse repercussions to them as a result of their political beliefs and activities (Lazarsfeld and Thielens 1958). Case studies of local and state politics vividly portray the effects of anti-Communist legislation on progressives of various sorts (e.g., Carleton 1985). The "silent generation" that emerged from McCarthyism is testimony enough to the widespread effects—direct and indirect—of the political repression of the era (see also Goldstein 1978, 369-96).

## Intolerance and Repression

**Table 1. Political Repression of Communists by American State Governments**

State	Banned from Public Employment	Banned from Politics	Banned Outright	Scale Score
Arkansas	Yes	Yes	Yes	3.5
Texas	Yes	Yes	Yes	3.5
Arizona	Yes	Yes	Yes	3.0
Indiana	Yes	Yes	Yes	3.0
Massachusetts	Yes	Yes	Yes	3.0
Nebraska	Yes	Yes	Yes	3.0
Oklahoma	Yes	Yes	Yes	3.0
Pennsylvania	No	Yes	Yes	3.0
Tennessee	No	Yes	Yes	3.0
Washington	No	Yes	Yes	3.0
Alabama	Yes	Yes	No	2.5
Louisiana	Yes	Yes	No	2.5
Michigan	Yes	Yes	No	2.5
Wyoming	Yes	Yes	No	2.5
Florida	Yes	Yes	No	2.0
Georgia	Yes	Yes	No	2.0
Illinois	Yes	Yes	No	2.0
California	Yes	No	No	1.0
New York	Yes	No	No	1.0
Delaware	No	No	No	.5
Mississippi	No	No	No	.5
New Mexico	No	No	No	.5
Alaska	No	No	No	.0
Colorado	No	No	No	.0
Connecticut	No	No	No	.0
Hawaii	No	No	No	.0
Iowa	No	No	No	.0
Idaho	No	No	No	.0
Kentucky	No	No	No	.0
Kansas	No	No	No	.0
Maryland	No	No	No	.0
Maine	No	No	No	.0
Minnesota	No	No	No	.0
Missouri	No	No	No	.0
Montana	No	No	No	.0
North Carolina	No	No	No	.0
North Dakota	No	No	No	.0
New Hampshire	No	No	No	.0
New Jersey	No	No	No	.0
Nevada	No	No	No	.0
Ohio	No	No	No	.0
Oregon	No	No	No	.0
Rhode Island	No	No	No	.0
South Carolina	No	No	No	.0
South Dakota	No	No	No	.0
Utah	No	No	No	.0
Vermont	No	No	No	.0
Virginia	No	No	No	.0
West Virginia	No	No	No	.0
Wisconsin	No	No	No	.0

Note: The scale score is a Guttman score. A "bonus" of .5 was added to the scale score if the state also required that Communists register with the government. See note 4 for details of the assignments of scores to each state.

Nor was the repression of the era a function of the degree of objective threat to the security of the state. Political repression was just as likely to occur in states with virtually no Communists as it was to occur in states with large numbers of Communists.<sup>7</sup> The repression of Communists bore no relationship to the degree of threat posed by local Communists.

It might seem that the repression of Communists, though it is clearly repression within the context of the definition proffered above, is not necessarily "anti-democratic" because the objects of the repression are themselves "antidemocrats." To repress Communists is to preserve democracy, it might be argued. Several retorts to this position can be formulated. First, for democracies to preserve democracy through nondemocratic means is illogical because democracy refers to a set of means, as well as ends (e.g., Dahl 1956, 1961, 1971; Key 1961; Schumpeter 1950). The means argument can also be judged in terms of the necessity of the means. At least in retrospect (but probably otherwise as well), it is difficult to make the argument that the degree of threat to the polity from Communists in the 1940s and 1950s in any way paralleled the degree of political repression (e.g., Goldstein 1978). Second, the assumption that Communists and other objects of political repression are "antidemocratic" must be considered as an empirical question itself in need of systematic investigation. As a first consideration, it is necessary to specify which Communists are being considered, inasmuch as the diversity among those adopting—or being assigned—the label is tremendous. Merely to postulate that Communists are antidemocratic is inadequate. Third, the repression of Communists no doubt has a chilling effect on those who, while not Communists, oppose the political status quo. In recognizing the coercive power of the state and its willingness to direct that power against

those who dissent, the effect of repressive public policy extends far beyond the target group.

## Public Opinion Intolerance

### Conceptualization

"Political tolerance" refers to the willingness of citizens to support the extension of rights of citizenship to all members of the polity, that is, to allow political freedoms to those who are politically different. Thus, "tolerance implies a willingness to 'put up with' those things that one rejects. Politically, it implies a willingness to permit the expression of those ideas or interests that one opposes. A tolerant regime, then, like a tolerant individual, is one that allows a wide berth to those ideas that challenge its way of life" (Sullivan, Piereson, and Marcus 1979, 784). Thus, political tolerance includes support for institutional guarantees of the right to oppose the existing regime, including the rights to vote, to participate in political parties, to organize politically and to attempt political persuasion. Though there may be some disagreement about the operationalization of the concept, its conceptual definition is relatively noncontroversial (see Gibson and Bingham 1982).

### Operationalization

The simple linkage hypothesis is that where the mass public is more intolerant, state public policy is more repressive. Though the hypothesis is simple, deriving measures of mass intolerance is by no means uncomplicated. Indeed, the study of state politics continually confronts the difficulty of deriving measures of state public opinion. Though there are five general alternatives—ranging from simulations to individual state surveys—the only viable option for estimating state-level opinion intolerance during the

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## Intolerance and Repression

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McCarthy era is to aggregate national surveys by state.

The source of the opinion data is the Stouffer survey, conducted in 1954. This survey is widely regarded as the classic study that initiated inquiry into the political tolerance of elites and masses (even though earlier evidence exists, e.g., Hyman and Sheatsley 1953). Two independent surveys were actually conducted for Stouffer: one by the National Opinion Research Center (NORC) and the other by the American Institute for Public Opinion (AIPO-Gallup). This design was adopted for the explicit purpose of demonstrating the accuracy and reliability of public opinion surveys based on random samples. Each agency surveyed a sample of the mass public and of the political elites.<sup>8</sup>

Stouffer created a six-point scale to indicate political intolerance (see Stouffer 1955, 262–69). The index is a Guttman scale based on the responses to fifteen items concerning support for the civil liberties of Communists, socialists, and atheists (see Appendix for details). The items meet conventional standards of scalability and are widely used today as indicators of political tolerance (e.g., Davis 1975; Nunn, Crockett, and Williams 1978; McCutcheon 1985; and the General Social Survey, conducted annually by NORC).

The process of aggregating these tolerance scores by state is difficult because the states of residence of the respondents in the Stouffer surveys were never entered in any known version of the data set. Through an indirect process, using the identity of the interviewer and the check-in sheets used to record the locations (city and state) of the interviews conducted by each interviewer, state of residence could be ascertained for the NORC half of the Stouffer data set. The respondents were aggregated by state of residence to create summary indicators of the level of intolerance in each of the states. The Appendix

reports the means, standard deviations, and numbers of cases and primary sampling units for this tolerance scale for the states represented in the NORC portion of the Stouffer survey. Evidence that this aggregation process produces reasonably valid state-level estimates of political intolerance is also presented.

Aggregating the elite interviews to the state level is in one sense more perilous and in another sense less perilous. With a considerably small number of subjects (758 in Stouffer's NORC sample), the means become more unstable. On the other hand, the aggregation is not done for the purpose of estimating some sort of elite population parameter. The elites selected were in no sense a random sample of state elites, so it makes little sense to try to make inferences from the sample to some larger elite population. Instead, the elite samples represent only themselves. The Appendix reports the state means, standard deviations, and numbers of cases.

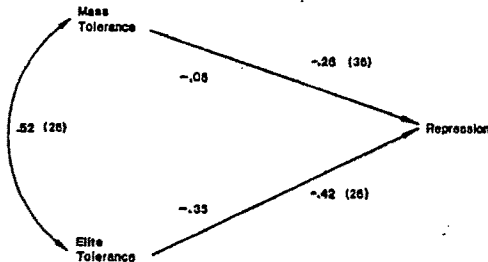
There is a moderate relationship between elite and mass opinion in the state ( $r = .52$ ). To the extent that we would expect elite and mass opinion in the states to covary, this correlation serves to validate the aggregate measures of opinion. The substantive implications of this correlation are considered below.

### The Simple Relationship between Opinion and Policy

Figure 1 reports the relationships between mass and elite political intolerance and the adoption of repressive public policies by the states. There is a modest bivariate relationship during the McCarthy era between mass opinion and repressive public policy. In states in which the mass public was more intolerant, there tended to be greater political repression, thus seeming to support the elitist theory. However, the relationship is



Figure 1. Relationships between Opinion and Policy



Note: Boldfaced entries are bivariate correlation coefficients, with pairwise missing data deletion. The nonboldfaced entries are standardized regression coefficients from a weighted least squares analysis using listwise missing data deletion. The numbers of cases are shown in parentheses.

somewhat stronger between elite opinion and repression. From a weighted least squares analysis incorporating both elite opinion and mass opinion, it is clear that it is elite preferences that most influence public policy. The *beta* for mass opinion is  $-.06$ ; for elite opinion, it is  $-.35$  (significant beyond  $.01$ ).<sup>9</sup> Thus political repression occurred in states with relatively intolerant elites. Beyond the intolerance of elites, the preferences of the mass public seemed to matter little.

Table 2 reports a cross-tabulation of policy outputs with elite and mass opin-

ion. The opinion variables have been dichotomized at their respective means. Though the number of cases shown in this table is small—demanding caution in interpreting the percentages—the data reveal striking support for the conclusion that elite opinion, not mass opinion, determines public policy. In eight of the ten states in which elites were relatively less tolerant, repressive legislation was adopted. In only six of the sixteen states in which elites were relatively more tolerant was repressive legislation passed. Variation in mass opinion makes little difference for public policy.<sup>10</sup>

It is a little surprising that elite opinion has such a significant impact on policy repression. After all, elites tend to be relatively more tolerant than the masses. Indeed, this finding is the empirical linchpin of the elitist theory of democracy.<sup>11</sup> This leads one to wonder just how much intolerance there was among the elites in the Stouffer data.

The survey data in fact reveal ample evidence of elite intolerance. For instance, fully *two-thirds* of the elites were willing to strip admitted Communists of their U.S. citizenship (Stouffer 1955, 43). Indeed, one reading of the Stouffer data is that elites and masses differed principally on the degree of proof of Communist party membership necessary before repression was thought legitimate. Much of the

Table 2. The Influence of Elite and Mass Opinion on the Repression of Communists (percentages)

Action	Elite Opinion Less Tolerant		Elite Opinion More Tolerant	
	Mass Opinion Less Tolerant	Mass Opinion More Tolerant	Mass Opinion Less Tolerant	Mass Opinion More Tolerant
Adopted repressive legislation	71	100	33	39
Did not adopt repressive legislation	29	0	67	62
Total	100	100	100	101*
Number of cases	7	3	3	13

\*Does not total 100 because of rounding error.

## Intolerance and Repression

mass public was willing to accept a very low level of proof of party membership (e.g., innuendo), while many elites required a legal determination of Communist affiliation. Once convinced of the charge, however, elites were very nearly as intolerant of Communists as members of the mass public. Just as McClosky and Brill (1983) have more recently shown significant intolerance within their elite samples, there is enough intolerance among these state elites to make them the driving force in the repression of Communists. Thus it is plausible that elite intolerance was largely responsible for the repressive policies of the era.

At the same time, there is little evidence that the communism issue was of burning concern to the U.S. public. For instance, Stouffer reported that "the number of people who said [in response to an open-ended question] that they were worried either about the threat of Communists in the United States or about civil liberties was, even by the most generous interpretation of occasionally ambiguous responses, *less than 1%*" (Stouffer 1955, 59, emphasis in original). Only one-third of the subjects reported having talked about communism in the United States in the week prior to the interview, despite the fact that the Army-McCarthy hearings were in progress during a portion of the survey period. Stouffer asserted, "For most people neither the internal Communist threat nor the threat to civil liberties was a matter of universal burning concern. Such findings are important. They should be of interest to a future historian who might otherwise be tempted, from isolated and dramatic events in the news, to portray too vividly the emotional climate of America in 1954" (Stouffer 1955, 72).

The issue of communism in the United States was of much greater concern to the elites. Nearly two-thirds of them reported having talked about communism in the United States during the week prior to the interview. When asked how closely they

followed news about Communists, fully 44% of the mass sample responded "hardly at all," while only 13% of the elite sample was as unconcerned (Stouffer 1955, 84). Just as elites typically exhibit greater knowledge and concern about public issues, they were far more attentive to the issue of domestic Communists.

Thus it is difficult to imagine that the repression of the 1950s was inspired by demands for repressive public policy from a mobilized mass public. Indeed, the most intense political intolerance was concentrated within that segment of the mass public *least* likely to have an impact on public policy (see also Gibson 1987). There can be no doubt that the mass public was highly intolerant in its attitudes during the 1950s. Absent issue salience, however, it is difficult to imagine that the U.S. people had mobilized sufficiently to have created the repression of the era.<sup>12</sup>

The actual effect of mass opinion may be masked a bit in these data, however. Perhaps it is useful to treat mass intolerance as essentially a constant across the states during the McCarthy era. Because the mass public was generally willing to support political repression of Communists, elites were basically free to shape public policy. In states in which the elites were relatively tolerant, tolerant policy prevailed. Where elites were relatively less tolerant, repression resulted. In neither case did mass opinion *cause* public policy. Instead, policy was framed by the elites. Nonetheless, the willingness of the mass public to accept repressive policies was no doubt important. Thus, the policy-making process need not be seen as a "demand-input" process with all its untenable assumptions but rather can be seen as one in which the preferences of the mass public—perhaps even the political culture of the state—set the broad parameters of public policy. In this sense, then, mass political intolerance "matters" for public policy.

We must also note that even if the

broad mass public has little influence upon public policy, specialized segments of the public may still be important. For instance, there is some correlation ( $r = .31$ ) between the number of American Legion members in the state and political repression.<sup>13</sup> Since the American Legion had long been in the forefront of the crusade against communism (see, e.g., American Legion 1937), it is likely that greater numbers of members in the state translated into more effective lobbying power. Thus particular segments of the mass public can indeed be mobilized for repressive purposes.

I should also reemphasize the strong correlation between elite opinion and mass opinion. This correlation may imply that elites are responsive to mass opinion or that they mold mass opinion or that elite opinion is shaped by the same sort of factors as shape mass opinion. Though it is not possible to disentangle the causal process statistically, there is some evidence that both elite and mass opinion reflect the more fundamental political culture of the state. The correlation between a measure of Elazar's state-level political culture and mass intolerance is  $-.68$ ; for elite opinion the correlation is  $-.66$ . In states with more traditionalistic political cultures *both* mass and elites tend to be more intolerant. Moreover, there is some direct relationship between political culture and political repression ( $r = .31$ ). Perhaps elite and mass preferences generally reflect basic cultural values concerning the breadth of legitimate political participation and contestation. In the moralistic political culture everyone should participate; only professionals should be active in the individualistic culture; and only the appropriate elite in traditionalistic political cultures (Elazar 1972, 101-2). Perhaps the political culture of the state legitimizes broad propensities toward intolerance, propensities that become mobilized during political crises.

One might also look at the data in

Figure 1 from a very different perspective. Rather than mass opinion causing public policy, perhaps mass opinion *is caused by* policy (cf. Page, Shapiro, and Dempsey 1987). To turn the elitist theory on its head, it is quite possible that the U.S. mass public is intolerant precisely because they have been persuaded and reinforced by the intolerance of U.S. public policy. Through the intolerance of public policy, citizens learn that it is acceptable, if not desirable, to repress one's political enemies. Though I do not gainsay that there are significant norms in U.S. society supportive of political tolerance (see Sniderman 1975), in practice citizens have been taught by federal and state legislation that Communists should *not* be tolerated. It is not surprising that many citizens have learned the lesson well.<sup>14</sup>

This argument is somewhat at variance with those who argue that greater exposure to the dominant cultural norms in the United States contributes to greater political tolerance. If the norms are tolerant, then greater exposure should create tolerance. But greater awareness of *repressive* norms—as expressed in public policies—should be associated with greater *intolerance*. Thus the result of political activism, high self-esteem, and other qualities that make us assimilate social norms will vary according to the nature of the norms (see Sullivan et al. 1985).

The norms of U.S. politics are at once tolerant and intolerant. Certainly, no one can doubt that support for civil liberties is a widely shared value. The key question, however, is "civil liberties for whom?" The U.S. political culture has long distinguished between "true Americans" and others and has always been willing to deny civil liberties to those who are "un-American." Foreign "isms" have repeatedly become the bogeymen in ideological conflict in the United States. Thus, citizens learn that civil liberties are indeed important to protect, but only for those

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## Intolerance and Repression

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who have a "legitimate" right to the liberty.

Thus the initial evidence is that political repression during the McCarthy era was most likely initiated by elites even if the mass public in most states would have acquiesced. These findings are not compatible with the elitist views that mass intolerance threatens democracy and that elites are the carriers of the democratic creed.

### The Political Culture of Intolerance and Repression

These findings may very well be limited to the specific historical era of McCarthyism. Due to the unavailability of historical data on elite and mass opinion it is difficult to judge whether earlier outbreaks of political repression can also be attributed to elite intolerance. Building on the discussion of political culture above, however, it is possible to give this issue further consideration.

Following World War I roughly one-half of the U.S. states adopted criminal syndicalism statutes.<sup>15</sup> For example, the statute adopted by California shortly after World War I defined the crime as "any doctrine or precept advocating, teaching or aiding and abetting the commission of crime, sabotage (which word is hereby defined as meaning willful and malicious physical damage or injury to physical property), or unlawful acts of force and violence or unlawful methods of terrorism as a means of accomplishing a change in industrial ownership or control, or effecting any political change" (Calif. Statutes, 1919. Ch. 188, Sec. 1, p. 281). Though no opinion data exist for the 1920s, it is possible to examine the relationship between state-level political culture and political repression during this earlier era.

The correlation between state political culture and the adoption of criminal syndicalism statutes is .40 ( $N = 50$ ), indicat-

ing once again that more traditionalistic states were more likely to engage in political repression. That this correlation is slightly stronger than the coefficient observed for the 1950s might speak to the breakdown of homogeneous state cultures as the population became more mobile in the twentieth century. In any event, we see in this correlation evidence that the more detailed findings of the McCarthy era may not be atypical.<sup>16</sup>

### Discussion

What conclusions about the elitist theory of democracy and the theory of pluralistic intolerance does this analysis support? First, I have discovered no evidence that political repression in the U.S. states stems from demands from ordinary citizens to curtail the rights and activities of unpopular political minorities. This finding differs from what is predicted by the elitist theory of democracy. Second, I find some evidence of elite complicity in the repression of the McCarthy era, a finding that is also incompatible with the elitist theory. Generally, then, this research casts doubt on the elitist theory of democracy.

Nor are these findings necessarily compatible with the theory of pluralistic intolerance advocated by Sullivan, Piereson, and Marcus. Though political intolerance in the 1950s was widespread and highly focused, there seems to have been little direct effect of mass opinion on public policy. Like the elitist theory of democracy, the theory of pluralistic intolerance places too much emphasis on mass opinion as a determinant of public policy.

The "demand-input" linkage process implicitly posited by these theories is probably their critical flaw. Early public opinion research that found high levels of mass political intolerance too quickly assumed that mass intolerance translated directly into public policy. The assumption was easy to make since little was

known of the processes linking opinions with policy. As linkage research has accumulated, however, the simple hypothesis relating opinion to policy has become increasingly untenable. The justification for studying mass political tolerance therefore cannot be found in the hypothesis that survey responses direct public policy.

At the same time, however, public opinion may not be completely irrelevant. Tolerance opinion strongly reflects the political cultures of the states, and, at least in the 1950s, political culture was significantly related to levels of political repression. Opinion is important in the policy process because it delimits the range of acceptable policy alternatives. It may well be that mass opinion is manipulated and shaped by elites; nonetheless, those who would propose repressive policies in California face a very different set of political constraints than those who propose repressive policies in Arkansas. This is not to say that repression is impossible—indeed, California has a long history of significant levels of political repression—but rather that the task of gaining acceptance for repression is different under differing cultural contexts.

For over three decades now, political scientists have systematically studied public policy and public opinion. Significant advances have been made in understanding many sorts of state policy outputs, and we have developed a wealth of information about political tolerance. To date, however, little attention has been given to repression as a policy output, and even less attention has been devoted to behavioral and policy implications of tolerance attitudes. The failure to investigate the linkage between opinion and policy is all the more significant because one of the most widely accepted theories in political science—the elitist theory of democracy—was developed on the basis of an assumed linkage between opinion and policy. I hope that this research,

though only a crude beginning, will serve as an early step in continuing research into these most important problems of democracy.

### **Appendix: Measurement and Aggregation Error in the State-Level Estimates of Mass Political Intolerance**

#### **Measurement**

The measure of political tolerance employed here is an index originally constructed by Stouffer. He used fifteen items to construct the scale. Eleven of the items dealt with communists; two with atheists (those who are against all churches and religion); and two with socialists (those favoring government ownership of all railroads and all big industries). Stouffer reported a coefficient of reproducibility of .96 for the scale, a very high level of reliability. He also reported that reproducibility was approximately the same at all educational levels.

I decided to use Stouffer's scale even though it includes items on atheists and socialists (1) in order to maintain comparability to Stouffer's research, (2) because an identical scale was created from a survey in 1973 that is very useful for assessment of aggregation error, and (3) because the scale is so reliable. Stouffer had a strong view of what his scale was measuring. He asserted, "But again let it be pointed out, this scale does not measure . . . tolerance *in general*. It deals only with attitudes toward certain types of nonconformists or deviants. It does not deal with attitudes toward extreme right-wing agitators, toward people who attack minority groups, toward faddists or cultists, in general, nor, of course, toward a wide variety of criminals. For purposes of this study, the tolerance of nonconformity or suspected nonconformity is *solely* within the broad context of the

## Intolerance and Repression

Communist threat" (Stouffer 1955, 54, emphasis in original).

The Stouffer measures of tolerance have recently been criticized (e.g., Sullivan, Piereson, and Marcus 1982). Perhaps the most fundamental aspect of this criticism is the assertion that the Stouffer items measure tolerance only for a specific group and thus are not generalizable. Because Stouffer was concerned only about intolerance of Communists, his findings may be time-bound; as the objects of mass displeasure evolve, the Communist-based approach to tolerance becomes less relevant and useful. This difficulty does not affect my analysis of policy and opinion from the 1950s, however, because Communists were probably a major disliked group for nearly all citizens in the survey. For instance, only 256 out of 4,933 of the mass respondents were willing to assert that someone believing in communism could still be a loyal U.S. citizen. Even if Communists were not the least-liked group for all U.S. citizens, they were certainly located in the "disliked-enough-not-to-tolerate" range for nearly everyone. Thus the Stouffer measure of tolerance is a valid and reliable indicator.

### **Aggregation Error**

Table A-1 reports the state-level means, standard deviations, and numbers of cases for the aggregation of elite and mass opinion. Not all states are included in Table A-1 because survey respondents were not located in every state. Since the Stouffer survey was not designed to be aggregated by state, it is necessary to try to determine whether there is any obvious bias in the state-level estimates. A few empirical tests can be conducted that, while not assuaging all doubts about the aggregation process, may make us somewhat more comfortable about using the state means.

The Stouffer survey was replicated in

1973 by Nunn, Crockett, and Williams (1978). Their survey was very nearly an exact replication of the Stouffer survey. In terms of the indicators of tolerance, it was an exact replication. Nunn, Crockett, and Williams were even extremely careful to reproduce Stouffer's scaling methodology in creating a summary index of intolerance (pp. 179-91). Thus it is possible to aggregate the same scale variable by state and derive a measure of political tolerance for the early 1970s.

With completely independent samples (including independent sampling frames), one would not expect that there would be much of a correlation between the Stouffer and the Nunn, Crockett, and Williams state-level estimates. Chance fluctuations in the distributions of primary sampling units (PSUs) per state would tend to attenuate the correlation between the state-level estimates. (The average number of PSUs in Stouffer's NORC survey is 2.3; for the Nunn, Crockett, and Williams survey it is 7.8.) Yet the correlation between the estimates from the two surveys is a remarkable .63 ( $N = 29$ ). If I were to exclude the 1973 estimate for Connecticut, an estimate that shows that state to be quite intolerant, then the correlation increases to .77 ( $N = 28$ ). It is difficult to imagine an explanation for this correlation other than that it is due to a common correlation with the true score for the state.

I have also investigated the relationship between state sample size and number of primary sampling units and aggregation error. I first assumed that differences between the  $t_1$  and  $t_2$  estimates of state opinion were due to aggregation error. The residuals resulting from regressing  $t_2$  opinion on  $t_1$  opinion represent this error; if squared, the residuals represent the total amount of error. The correlations between the squared residuals and  $t_1$  sample size and number of PSUs are  $-.30$  and  $-.27$ . The correlations between the residuals and  $t_2$  sample size and number of

PSUs are  $-.29$  and  $-.29$ . These correlations indicate that aggregation error is larger in states in which the number of subjects and number of PSUs is smaller—a not unexpected finding. However, since the relationships are modest, they do not undermine the basic aggregation procedure.

Another bit of evidence supporting the

aggregation process comes from the correlations of tolerance and political culture. The correlation between Elazar's measure of political culture and average state tolerance in the 1950s is  $-.68$ . This correlation enhances my confidence in the utility of the state-level estimates.

Another, very different tack that can be taken is to estimate the error associated

**Table A-1. State Mean Tolerance Scores, Mass Public, and Elites,  
NORC Stouffer Survey, 1954**

State	Mass Public				Elites		
	Mean	Standard Deviation	Number of Cases	Number of PSUs	Mean	Standard Deviation	Number of Cases
California	4.47	1.50	174	4	5.09	1.43	65
Missouri	4.44	1.20	18	2	5.45	.69	11
New Jersey	4.41	1.43	61	1	4.90	1.28	60
Washington	4.33	1.44	52	2	5.14	.66	14
Iowa	4.26	1.42	23	1	—	—	—
Wisconsin	4.24	1.56	41	2	5.44	.87	25
Massachusetts	4.22	1.47	81	2	4.51	1.21	41
New York	4.21	1.40	273	6	5.06	1.08	81
Oregon	4.20	1.47	15	1	—	—	—
Colorado	4.13	1.46	23	1	5.29	1.33	14
Connecticut	4.12	1.17	17	1	5.17	.83	12
Nebraska	4.06	1.24	16	1	4.40	1.35	10
Minnesota	3.92	1.43	64	3	5.33	.96	27
Ohio	3.83	1.57	103	4	5.02	1.04	54
Illinois	3.81	1.55	86	2	4.97	1.39	39
Nevada	3.77	1.61	31	1	—	—	—
North Dakota	3.76	1.46	41	1	5.17	1.27	12
Pennsylvania	3.75	1.41	179	6	4.77	1.29	43
Michigan	3.75	1.34	163	4	4.92	1.26	38
Kansas	3.64	1.26	59	2	—	—	—
Florida	3.61	1.43	84	2	4.46	1.47	24
New Hampshire	3.58	1.71	19	1	5.36	1.03	11
Maryland	3.45	1.46	51	2	—	—	—
Idaho	3.45	1.65	22	1	5.15	1.07	13
Oklahoma	3.43	1.44	67	3	5.31	.85	13
Virginia	3.40	1.68	15	1	—	—	—
Indiana	3.36	1.32	129	5	4.61	1.40	36
Alabama	3.32	1.27	37	2	4.30	1.46	27
Texas	3.28	1.05	156	5	4.30	1.49	40
Louisiana	3.27	1.34	26	1	4.33	1.67	12
North Carolina	3.17	1.17	65	3	3.60	1.90	10
Tennessee	2.98	1.62	44	2	—	—	—
Georgia	2.86	1.39	50	3	—	—	—
Kentucky	2.86	1.25	22	1	4.77	1.39	26
West Virginia	2.34	.90	29	2	—	—	—
Arkansas	1.79	1.27	19	1	—	—	—
Average	3.65	1.40	65	2.3	4.88	1.22	29

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with the aggregation process. For each survey, I aggregated the proportion of the respondents having twelve or more years of formal education. These percentages can be compared to census estimates of the level of education in the state. The comparison is not perfect due to two considerations. First, the census data are themselves population estimates drawn from survey samples. Second, the census reports the percentage of residents over the age of twenty-five with twelve or more years of education. I assume that those with twelve or more years of education have a high school degree, although this might not be true for every single respondent. Moreover, it is not possible to isolate those respondents twenty-five years and older in the Stouffer survey. Nonetheless, the correlation for the 1950s data between the survey and census estimates of education is a substantial .72 ( $N = 36$ ). While this correlation does not speak directly to the utility of the state-level estimates of tolerance, it does suggest that aggregation from the survey to the state is not completely inappropriate.

The correlation between elite opinion in the 1950s and elite opinion in the 1970s is .25 (.28 with a minimum-number-of-respondents requirement). That the correlation is not higher is a bit worrisome, although it is not difficult to imagine that there is greater flux in elite opinion over the two decades separating the two surveys than there is in mass opinion. Moreover, there were some slight differences in the composition of the elite samples drawn in 1954 and 1973.

As a means of assessing the validity of the aggregation of elite opinion, it is possible to compare elite tolerance with other elite attitudes. Erikson, Wright, and McIver (1987) have developed a separate measure of the degree of liberalism of state elites. The measure summarizes the ideological positions of the state's congressional candidates, state legislators, political party elites, and national

convention delegates. As an overall index of the liberalism-conservatism of state elites, they take the average score of the Democrats and the Republicans. Thus each state receives a score indicating the degree of liberalism-conservatism of state elites. Though most of the indicators are drawn from the 1970s, the authors believe this to be a more stable attribute of state elites. According to their index, the most conservative elites are found in Mississippi; the most liberal elites are found in Massachusetts.

The correlation of state elite conservatism and political tolerance is  $-.46$  ( $N = 26$ ) for the Stouffer elites and  $-.22$  ( $N = 29$ ) for the Nunn, Crockett, and Williams elites. Though liberalism-conservatism is conceptually distinct from political tolerance, some solace can be taken in this correlation. The aggregation process seems not to have introduced unexpected or obviously biased estimates of state-level elite opinion.

## Notes

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1. The elitist theory of democracy is actually an amalgam of the work of a variety of theorists, including Berelson, Lazarsfeld, and McPhee (1954); Kornhauser (1959); Lipset (1960); and Key (1961). The most useful analysis of the similarities and differences among the theories can be found in Bachrach 1967. Some elite theorists emphasize the dominance and control of public policy by elites,



while other theorists emphasize the antidemocratic tendencies of the mass public. The single view most compatible with the hypotheses tested in this article is Kornhauser's (1959). The hypotheses are also to be found in Dye and Zeigler 1987 (see also Dye 1976). Earlier empirical work on the tolerance of elites and masses includes Berelson, Lazarsfeld, and McPhee 1954; Lipset 1960; Prothro and Grigg 1960; and McClosky 1964. A more recent analysis of some of the propositions of elitist theory can be found in Gibson and Bingham 1984.

2. Linkage research is fairly common in other areas of substantive policy (e.g., Erikson 1976; Weissberg 1978), but the only rigorous investigation of civil liberties is that of Page and Shapiro (1983). They assessed the relationship between change in opinion and change in policy, and found that in eight of nine policy changes in the area of civil liberties there was opinion-policy congruence. They also found that state policies were more likely to be congruent with opinion than national policies, although the relationship did not hold in the multivariate analysis. Though their analysis was conducted at the national level, their findings seem to suggest that political repression results from demands from the mass public.

3. This is similar to Goldstein's definition, "Political repression consists of government action which grossly discriminates against persons or organizations viewed as presenting a fundamental challenge to existing power relationships or key governmental policies, because of their perceived political beliefs" (1978, xvi).

4. The source for these data is a 1965 study requested by a subcommittee of the Committee on the Judiciary in the U.S. Senate. See also Library of Congress, Legislative Reference Service, 1965; Gellhorn 1952; and Prendergast 1950. Care must be taken in using the Legislative Reference Service data because there are a variety of errors in the published report. Corrected data, based on an examination of all of the relevant state statutes, are available from the author.

The scores shown in Table 1 reflect actions taken by the state governments between 1945 and 1965. The decision to limit the policy measures to this period is based on the desire to have some temporal proximity between the opinion and policy data. This decision has implications for the scores of three states. Kansas and Wisconsin both barred Communists from political participation in legislation adopted in 1941. This legislation is excluded from Table 1. Arkansas is shown as having banned Communists from public employment, from politics, and outright. Only the outright ban was adopted in the 1945-65 period. Because a complete ban necessarily excludes Communists from public employment and from political participation, the score for Arkansas is shown as 3.5.

5. These three items scale in the Guttman sense.

That is, nearly all of the states outlawing the Communist party also denied it access to the ballot and public employment. Nearly all of the states that denied Communists access to the ballot as candidates also made them ineligible for public employment. The registration variable does not, however, exhibit this pattern of cumulativeness. Registration seems to have been a means of enforcing a policy goal such as banning membership in the Party. Because registration can raise Fifth Amendment self-incrimination issues, some states chose not to require it. Statutes requiring registration are treated for measurement purposes as representing a greater degree of commitment to political repression, and for that reason the "bonus" points were added to the basic repression score.

6. Validity means not only that measures of similar concepts converge; measures of dissimilar concepts must also diverge (Campbell and Fiske 1959). Thus it is useful to examine the relationship between the repression measures and measures of other sorts of policy outputs. Klingman and Lambers (1984) have developed a measure of the "general policy liberalism" of the states. General policy liberalism is a predisposition in state public policies toward extensive use of the public sector and is thought to be a relatively stable attribute. I would expect that political repression is not simply another form of liberalism, and indeed it is not. The correlation between general policy liberalism and political repression during the 1950s is only  $-.18$ . Moreover, the relationship between repression and a measure of New Deal social welfare liberalism policy (see Holbrook-Provow and Poe 1987; Rosenstone 1983) is only  $-.22$ . Repression occurred in states with histories of liberalism just about as frequently as it did in states typically adopting conservative policies. Thus the measure of repression is not simply a form of political liberalism, a finding that contributes to the apparent validity of the measure.

7. This conclusion is based on figures compiled by Harvey Klehr on the size of the Communist Party U.S.A. during the 1930s (Klehr 1984, tbl. 19.1 and personal communication with the author, 21 May 1986). The data are from the Party's own internal record. Klehr believes the data to be reasonably reliable, and others seem to agree (see, e.g., Glazer 1961, 208, n. 3; and Shannon 1959, 91). There is also a strong relationship between Party membership and votes for Communist candidates for public offices in the 1936 elections (as compiled by the American Legion 1937, 44), as well as a strong relationship with FBI estimates of Party membership in the states in 1951 (U.S. Senate, Committee on the Judiciary 1956, 34).

8. Stouffer defined *elites* as those who hold certain positions of influence and potential influence in local politics. The elite sample was drawn from those holding the following positions: community chest chairmen; school board presidents; library

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committee chairmen; Republican county chairmen; Democratic county chairmen; American Legion commanders; bar association presidents; chamber of commerce presidents; PTA presidents; women's club presidents; DAR regents; newspaper publishers; and labor union leaders.

9. Weighted least squares was used because I could not assume that the variances of the observations were equal. Following Hanushek and Jackson (1977, 151–52), I weighted the observations by the square root of the numbers of respondents within the state. The *r*-square from this analysis is .14. The regression equation with unstandardized coefficients is:  $Y = 7.31 - .14 (\text{mass opinion}) - 1.11 (\text{elite opinion})$ .

10. The data in Table 2 suggest that where the state elites are relatively less tolerant, increases in mass tolerance are associated with an increase in political repression. Caution must be exercised in interpreting the percentages, however, due to the small number of cases available. The data reveal that in five of the seven states with a relatively less tolerant mass public, repressive legislation was adopted, while in all three of the states with a relatively more tolerant mass public repressive legislation was adopted. In the context of the numbers of cases, I did not treat this difference as substantively significant.

11. It might be argued that elite opinion serves only to neutralize intolerant mass opinion. This suggests an interactive relationship between elite and mass opinion. Tests of this hypothesis reveal no such interaction. The impact of elite opinion on public policy is not contingent upon the level of tolerance of the mass public in the state.

12. Though it is a bit risky to do so, it is possible to break the policy variable into time periods according to the date on which the legislation was adopted. A total of sixteen states adopted repressive legislation prior to 1954; ten states adopted repressive legislation in 1954 or later. The correlations between pre-1954 repression and mass and elite tolerance, respectively, are  $-.05$ , and  $-.35$ . Where elites were more intolerant, policy was more repressive. Mass intolerance seems to have had little impact on policy.

The correlations change rather substantially for the post-1954 policy measure. There is a reasonably strong correlation between mass intolerance and repression ( $r = -.32$ ) but little correlation with elite intolerance ( $r = -.13$ ). If one were willing to draw conclusions based on what are surely relatively unstable correlations, based on limited numbers of observations, one might conclude that early efforts to restrict the political freedom of Communists were directed largely by elites, while later efforts were more likely to involve the mass public. The initiative for political repression therefore was with the elites, though the mass public sustained the repression once it was under way.

At the same time, however, the slight correlation between pre-1954 policy and mass intolerance suggests that mass opinion was not shaped by public policy. Where policy was more repressive, opinion was not more intolerant. The close temporal proximity here should give us pause in overinterpreting this correlation, however.

13. Note that Stouffer found that the leaders of the American Legion were the most intolerant of all leadership groups surveyed (Stouffer 1955, 52). Indeed, the commanders interviewed were only slightly less intolerant than the mass public.

14. At the same time, it should be noted that U.S. citizens became substantially more tolerant of Communists by the 1970s (e.g., Davis 1975; Nunn, Crockett, and Williams 1978). This too might reflect changes in public policy, as well as elite leadership of opinion. As the U.S. Supreme Court invalidated some of the most repressive state and federal legislation of the McCarthy era, and as U.S. political leaders (including Richard Nixon) sought improved foreign relations with Communist nations, it became less appropriate to support the repression of Communists. These comments illustrate, however, the difficulty of sorting out the interrelationships of opinion and policy and also reveal that many efforts to do so border on nonfalsifiability.

15. Between 1917 and 1920, twenty-four states adopted criminal syndicalism statutes. There is some ambiguity in published compilations about the number of states with such laws. Dowell (1969) lists twenty states with such legislation, not counting the three states that adopted but then repealed syndicalism laws. Dowell apparently overlooked Rhode Island, at least according to the compilations of Chafee (1967) and Gellhorn (1952). On the other hand, neither Chafee nor Gellhorn listed Colorado or Indiana as having such statutes (though Chafee did list the states that had repealed their legislation). This latter problem is in part a function of determining whether specific statutes should be classified as banning criminal syndicalism. By 1937, three states had repealed their statutes (although one of these—Arizona—apparently did so inadvertently during recodification). As of 1981, seven of these states still had the statutes on their books, and one additional state—Mississippi—had passed such legislation (Jenson 1982, 167–75). For purposes of this analysis, Dowell's twenty-three states and Rhode Island are classified as having criminal syndicalism laws as of 1920.

16. It should also be noted that political culture is fairly stably related to mass political intolerance. Estimates of state opinion were derived from Roper data on an item about loyalty oaths asked in a 1937 survey. Opinion in more traditionalistic states was more supportive of mandatory loyalty oaths ( $r = -.44$ ,  $N = 47$ ). Similarly, the correlation between political culture and the state aggregates from the Stouffer replication in 1973 (see the Appendix) is

-.58 ( $N = 35$ ). These coefficients are nothing more than suggestive, but they do suggest that political intolerance is a relatively enduring attribute of state political culture.

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## CHURCHES AS POLITICAL COMMUNITIES

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**M**ost studies of contextual influences on political attitudes and behavior have treated geographical areas as the operative social environment. As early research on social influence processes noted, the conditions that promote consensus among inhabitants of a common environment are likely to be present in formal organizations that encourage face-to-face interaction. Churches possess many of the characteristics that should maximize behavioral contagion and are thus fertile ground for the dissemination of common political outlooks. This expectation is tested by assessing the link between theological and political conservatism in 21 Protestant congregations. The theological climate in the churches is found to contribute strongly to the members' political conservatism over and above the personal commitment of respondents to traditional Christian values and a variety of social and attitudinal variables. As churches constitute the single most widespread form of voluntary organizational affiliation in the United States, their potential political impact appears to be considerable.

**E**ver so slowly, political scientists have come to appreciate the influence of social environment on individual political behavior. The "contextual" approach, identified by Durkheim (1951) and first applied to electoral data by Tingsten (1937), asserts the "dependence of individual political behavior on variation in properties in the individual's environment" (Sprague 1982, 99). While scholars have not abandoned the individual as the unit of analysis, there is growing acceptance for the proposition that the salience of politically relevant characteristics may be reinforced or undermined by the larger environment that surrounds the individual (Huckfeldt 1986). The operation of contextual influences has been documented for several different types of political behavior—participation, vote choice, candidate evaluation, attitude formation—in local, na-

tional and cross-national samples. The environmental properties found to exert behavioral influence have included racial and ethnic composition, social class distribution, and the level of partisan competition.

For the most part, these studies have operationalized social context in terms of residential areas such as neighborhoods (census blocks or tracts), counties, communities, and regions. The choice of areal units has been dictated partly by the assumption that residential context is politically significant and partly by the ready availability of social and political data for geographic areas. As scholars have recognized, the residential context may not be the most important social environment that operates on the political attitudes and behavior of individuals. Practitioners have similarly suggested that "social precincts" defined by the

work place, secondary associations, and personal friendship networks may have replaced the traditional geographical precincts as the locus of attitude formation for some individuals (Mahe 1987). Consistent with this insight, systematic studies that hone in on intimate social environments—the family or friendship networks—report very strong behavioral influences associated with these more immediate contexts (Eulau and Rothenberg 1986; Finifter 1974; McCloskey and Dahlgren 1959; MacKuen and Brown 1987).

Our research shifts the analysis to yet a new level of social context—the church attended by the individual. As voluntary associations of individuals bound by strong affective ties and regular social interaction, churches constitute genuine communities that are well suited to the transmission and maintenance of group norms (White 1968). Churches also form by far the most extensive network of voluntary organizations in the United States, reaching perhaps two-thirds of the adult population (Stark 1987). This combination of characteristics favorable to social influence warrants the attention of political scientists because of the recent renewal of political activism in the churches. As it did in earlier periods of U.S. history, the religious factor now helps to define the national political agenda and to structure mass preferences for candidates, parties, and issue positions (Wald 1987). With the exception of otherworldly faiths that counsel withdrawal from corrupt secular institutions, church attendance is positively correlated with electoral participation. Religious activists have entered the political arena as the shock troops for several controversial political movements and candidacies. Yet despite the apparent growth in religiously based political conflict, scholars have paid little systematic attention to the environment where religious influence is most likely to be brought to bear on citizens—the local church.

Research on religion and politics has adopted the essentially individualistic approach to political behavior so long dominant in U.S. voting studies. This limitation is due largely to a reliance on surveys that pluck respondents randomly from a community or nation and thus provide too few cases to allow analysis of the influence of specific congregations (but see Legee and Gremillion 1986). The type of data available in national or community-wide surveys forces scholars to assess church influence with denominational preference items or measures of individual religious belief systems.<sup>1</sup> If the linkage between religion and politics is forged in the context of the congregation or parish, then measures derived from personal religious values or denominational identity will underestimate the political significance of religion generally and the specific impact of the immediate religious community—the congregation or parish. With data obtained from a sample of churches, this study provides a unique opportunity to assess the contribution of proximate religious environments to individual political behavior.

### Churches as Contexts

Contextual research has identified a variety of influence mechanisms that promote consensus among members sharing an environment. At a minimum, a contextual effect would seem to require (1) the communication of political messages and (2) opportunities for members to observe the reactions of fellow members to these messages and to bring their own behavior into conformity with them. Like similar organizations that provided the basis for early theoretical work on contextual influence (Blau 1960a, 1960b), churches are environments with the capacity to facilitate both processes.

There are ample opportunities for church authorities to communicate political messages through direct channels

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## Political Communities

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such as sermons, pastoral messages, adult education classes, poster displays, and church publications. Implicit political messages may also be transmitted through aspects of what Parenti (1967) aptly described as the latent "political culture" associated with religious traditions—attitudes toward authority, knowledge, the requirements for salvation, and so on. Because of the high esteem in which churches are held and the voluntary nature of membership, any political messages transmitted by church authorities are likely at least to receive a respectful hearing, to enjoy substantial credibility, and potentially to alter opinions not initially in conformity with the prevailing views. In addition to the messages transmitted by religious leaders, political cues may also be disseminated among parishioners through conversation and other modes of social intercourse. The church provides many avenues for the reinforcement of such messages by bringing the membership together on a regular basis for formal worship and promoting informal social interaction among congregants. Similarly, the congregation has the capacity to punish deviations from collective norms by withdrawing approval from members who stray from acceptable attitudes or exhibit proscribed forms of behavior. This potent array of social influence mechanisms equips congregations with the capacity to promote uniformity in sociopolitical outlook.

The manner in which churches utilize these mechanisms was demonstrated in two of the congregations we observed during the data collection phase of the study. In a "mainline" Protestant church, the sanctuary was festooned with posters promoting solidarity with Central American victims of rightist oppression, the minister's sermon lauded the resistance of women to tyranny throughout history, the explanation of harvest symbols stressed the need to combat poverty and hunger, and the choice of hymns included

folk songs from the civil rights era. During the announcement period of the service, several congregants encouraged their fellow members to attend demonstrations, meetings, and workshops in support of the causes that had received ministerial endorsement. A cursory inspection of the bumper stickers on vehicles parked around the church suggested that a considerable proportion of church members had indeed been active in such liberal organizations. A fundamentalist church visited one week later presented somewhat more indirect political tutelage. Though the sermon took as its text a verse from Exodus, a biblical book pregnant with political implications (Walzer 1985), the minister used it to encourage contributions for building a fence around church property. The fence was necessary, he said, to protect against beer parties and other rowdy activities taking place nearby. The church bulletin included a request for volunteers to help staff the local "right-to-life" office. Without formally endorsing either abstention from liquor or antiabortion activities, the church nonetheless conveyed clear social messages. The conservative line propounded on these issues was consistent with the church's past sponsorship of visits by conservative evangelists like the Rev. Jerry Falwell and the pastor's previous campaign for public office on a platform of moral conservatism. Each of the divergent churches offered a distinctive "climate of opinion" that linked Christianity to a particular political agenda.

Rather than isolated instances of political activism, these examples illustrate the growing willingness of churches in the United States to communicate political messages to their members. While tax-exempt status is theoretically dependent upon refraining from overt endorsement of candidates for public office, even that restriction can be evaded quite easily. No such restriction hampers the

church that takes public stands on issues or policy debates. During the 1960s, the more liberal churches, especially those from mainline Protestant denominations, took advantage of the leeway accorded churches to press for causes such as racial justice, opposition to the Vietnam war, and campaigns for economic reform (Quinley 1974); they have continued to carry the torch for various liberal causes on the national agenda. The conservative Protestant clergy, who were slower to accept a political role for the church, now seem bent upon emulating their activist colleagues, though on behalf of different issues and positions. Once regarded as politically quiescent or at least as uncritically accepting of U.S. public policy, the Roman Catholic clergy have recently weighed in with pronouncements regarding the contemporary controversies of abortion, arms control, Central American policy, and the U.S. economy. In the black community, members of the clergy maintain the strong political leadership role that was evident during the civil rights movement (Walton 1985, 47-49).

Of course, churches are not primarily intended to function as political movements but as spiritual agencies. Perhaps the most effective limit upon their political capacity is the resistance of members to "excessive" political entanglement. The strong U.S. tradition of separation between church and state may lead some parishioners to resist attempts to foist a political line on churches, whether such attempts emanate from the clergy, denominational bodies, or self-appointed spokespersons lacking official standing. During a period of retrenchment that followed the activism of the 1960s, the backlash against such politicization resulted in ferocious internal conflict, forced some liberal clergy from their pulpits, and may even have contributed to declining membership in some of the most politically active denominations (Hadden 1969). Even if church members accept the

legitimacy of social action, churches rarely circumscribe the lives of adherents. Thus church members are exposed to other agencies that may compete with the church as sources of political information and influence.

We examine the contextual dimension to the often-asserted affinity between conservatism in religion and politics. Despite the example of liberation theology in South America, the precedent of Christian socialism, and the social gospel tradition in U.S. religious life, many students of politics reflexively treat religion as a force for order, social control, and conservative positions on public policy. The assumption also persists despite a preponderance of disconfirming evidence in the scholarly literature (Wuthnow 1973) and repeated findings that it is the content of religious values rather than intensity of commitment that seems to influence political outlooks (Benson and Williams 1982; Gorsuch and Aleshire 1974; Welch and Leege, n.d.). Despite these caveats, there are some good reasons to hypothesize a connection between conservative orientations in religion and politics. Scholars have identified asceticism, otherworldliness, transvaluation, and respect for authority as characteristics of Christianity that comport comfortably with resistance to social and political change. To the extent that church attendance and involvement are positively correlated with social class, that too should promote political conservatism among churchgoers. The social values of Christianity have generally, if not universally, been interpreted to promote respect for tradition and conventional behavior. Though speaking of anti-abortion sentiment, Himmelstein (1986, 9) made a point generally applicable to traditionalist Christianity when he noted a common theme of "opposition to too much individual autonomy, too much freedom from constraints imposed by traditional roles and norms (especially those of the family), and too much emphasis on



individual self-determination and self-fulfillment." So long as one does not elevate these affinities into iron laws and overlook the conditions that may breed exceptions, there is a sound basis to hypothesize a tie between traditionalist religion and conservative politics. The connection may have been underestimated in past empirical research by the decision to operationalize religion as a personal belief system or denominational self-identification.

The task before us is to examine that linkage with a focus upon the forces responsible for it. Is it the individual's theology or the theology of the individual's congregation that most effectively links the domains of religion and politics? We are interested in whether such linkage operates solely at the individual level, as conventional electoral analysis would have it, or depends to some degree upon the climate of opinion within congregations. Our observations have led us to expect that congregations should indeed constitute political communities.

### Data and Methods

The data for the study were obtained from questionnaires administered over the course of a calendar year to volunteer members of 21 Protestant churches in the Gainesville, Florida metropolitan area.<sup>2</sup> With a concern to maximize diversity among the churches sampled, we categorized churches by their perceived theological orientation and then contacted ministers or education directors from several churches within each category. Respondents were promised anonymity for their questionnaires and confidentiality for their churches. With a very high degree of cooperation, we were permitted to administer the lengthy questionnaire to members of adult education classes (the source of two-thirds of our respondents) and to persons attending various worship services on Sunday or Wednesday. This

procedure produced data from a grand total of 673 respondents in 23 churches. For this analysis, we have dropped two churches whose 16 respondents were recruited directly by the minister, leaving 657 respondents from 21 different churches. Because of time pressure, the length of the questionnaire, misunderstanding, suspicion about the motives of the authors in soliciting personal information, and the general problem of securing compliance to instructions with self-administered surveys, many questionnaires contained at least some unanswered or incorrectly answered questions. There appeared to be no bias in the distribution of such questionnaires by church, so we followed the conservative strategy of including only respondents with complete information on all the items to be used for analysis.<sup>3</sup> This will reduce the effective *N* to between 502 and 629 cases, depending upon the number of variables included in the analysis.

The 21 churches provide the operative social contexts for this study.<sup>4</sup> The use of such units offers some potential advantages over studies that employ the residential neighborhood as a surrogate for social environment. The church is a distinct and bounded context, recognized as such by members who have chosen voluntarily to affiliate and who may leave if they find the congregation no longer to their liking. Because of the selection criteria, the churches in the study are strikingly diverse in their social composition, theological position, and political tendencies. By virtue of their presence on church premises when the questionnaire was administered, we know that respondents have face-to-face contact with members of the group and exposure to communication from church authorities; indeed, responses to some of the items on the interview schedule indicate the presence of strong affective commitments and extensive involvement in the life of the church. These tendencies, which should magnify

the likelihood of behavioral contagion, cannot always be assumed to operate in the neighborhood setting.

By conducting on-site administration of questionnaires, we have isolated a group of respondents who are highly vulnerable to group norms. As noted, the sample consists of the subset of church members who were present at services or participants in adult education classes. We know from previous research that intensity of involvement with the church is positively correlated with the likelihood of conformity to group norms (Stark 1984). The prospects for contextual influence are less promising for weakly attached members who seldom attend services or participate in adult education classes—and who were undoubtedly underrepresented in our survey. Of course, the peripheral members are also likely to be underrepresented in mail surveys from church rosters, the major alternative data collection strategy available to us.

The other limits of our research strategy stem from the manner of selecting congregations. As all the churches were located within a single county in a region with a distinctive political culture, we cannot generalize the findings to other settings. Neither can we compare the impact of church-level influences with competing geographical contexts. By confining the survey to Protestant congregations, we gave up the opportunity to explore contextual influences associated with Catholic or Jewish congregations. Moreover, we cannot claim a random sample of churchgoers in the community as a whole. These drawbacks notwithstanding, the research design offers a rare opportunity to assess how one important social environment may structure opinions on political issues.

### Dependent Variables

Two measures of political conservatism will be used in the analysis. The first, a composite scale of preferences on public

issues with a moral dimension, taps support for “traditional” values in public policy. Though developed independently, it bears a strong relationship to the “moral traditionalism” battery included on the interview schedule for the 1986 National Election Study (Conover and Feldman 1986). The “moral conservatism” scale is based on items about abortion, cohabitation, homosexuality, traditional sex roles, censorship, birth control, the family, drug use, and racial intermarriage.<sup>5</sup> The 10 Likert-style items in the battery engendered a common reaction as indicated by an internal reliability coefficient (standardized Cronbach’s alpha) of .8. These issues have historically been the source of the strongest religious differences in mass political attitudes (Hoge and Zulueta 1985). We have also employed a general measure of ideological self-definition common in the literature. Respondents were asked to pick from a list of eight phrases (including *don’t know*) the one label that best described their political beliefs. The resulting measure of self-described ideology was compressed to a five-point continuum with response options of strongly conservative or liberal, conservative or liberal, and middle-of-the-road. To encourage respondents to cue their answers to political rather than religious matters, this question followed six others that inquired about past and present partisan identification, voter registration, and candidate preferences in recent national elections.

While the two measures differ in several ways, the major distinction is that the moral conservatism scale has a stronger surface relevance to church doctrine than does the ideological label. When traditionalist ministers express political opinions from the pulpit, they stress propriety and rectitude in personal conduct (Koller and Retzer 1980). To the historical injunction against the “finger sins” of drinking, smoking, and gambling, ministers have added renewed emphasis to the impor-

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tance of maintaining monogamous relations within marriage and to reserving legitimacy for the traditional family—themes expressed by the items on the moral conservatism scale. Churchgoers have much greater difficulty connecting church doctrine with the question of government size and spending that undergirds the left-right political spectrum. Hence we should expect to find religious conservatism more strongly related to the moral conservatism scale than to the choice of an ideological designation.

### Theological Conservatism

The measurement of religious conservatism is fraught with conceptual and methodological hazards (Kellstedt and Smidt 1985). In the absence of a single widely accepted measure, we decided to rely upon a slightly revised version of Gallup's three-part scale (Hunter 1983). The scale taps three elements in Christian thought that have been given special emphasis by Protestant traditionalists: evangelism; an intense, personal relationship with Christ; and an inerrantist approach to Scripture. We employed these specific items to assess Protestant orthodoxy:

1. Have you ever tried to encourage someone to believe in Jesus Christ or to accept Jesus Christ as his or her savior?
2. Would you say that you have been "born again" or have had a turning point in your life when you committed yourself to Christ?
3. Which of the following statements comes *closest* to your feelings about the Bible?
  - a. The Bible is the actual word of God and it should be taken literally, word for word.
  - b. The Bible is the inspired word of God but not everything in it should be taken literally, word for word.
  - c. The Bible is an ancient book of

history, legends, and moral precepts recorded by men.<sup>6</sup>

Respondents who answered the first two items affirmatively and endorsed the literalist view of the Bible were assigned the maximum score of three on the scale. The middle position (with a value of two) was occupied by respondents who also responded positively to the first two questions but selected option b on the question about the Bible. Persons who endorsed any other combination of views, including negative responses to the first two items and support for the secularist approach to the Bible, were accorded the lowest score of one for religious conservatism. The entire sample yielded a roughly even division with 31% in the lowest category, 30% in the medium position, and the remaining 39% assigned the maximum score. Religious conservatism scores were calculated individually for each respondent and averaged within each church to obtain a measure of the congregational religious orientation. In the subsequent discussion, we will refer to individual scale scores as personal theology and use "congregational theology" to designate the congregational mean.

### Results of Analysis

While it is not adequate to specify the nature of contextual influence, a simple analysis of variance with the contextual units as levels of analysis can at least suggest the possibility of church-related influence. When we performed such analysis for both moral conservatism and self-described ideology (not shown), the 21 churches in our sample differed widely. Using each church as a dummy variable, between-group differences accounted for almost 58% of the variation in moral conservatism and 35% of the variation in self-described ideology. To give some sense of the magnitude of such effects, the

**Table 1. Regression of Moral Conservatism and Conservative Identification on Congregational Theology**

Designation	Congregational Theology	Constant	Adjusted R <sup>2</sup>	Number of Cases
Churches				
Moral conservatism	9.0**	-.5	.74	21
Conservative identity	.6*	2.1**	.32	21
Individuals				
Moral conservatism	9.0**	-.2	.41	616
Conservative identity	.7*	2.0**	.17	629

Note: Entries are unstandardized regression coefficients. The first two rows report analyses conducted with the church as the unit of analysis; in the last two rows, the individual respondent is the unit of analysis.

\*Significant at .01.

\*\*Significant at .001.

grand mean for moral conservatism was 18.5 with a possible range from 0 to 36. Church means ranged as low as 5.9 and as high as 30.1. On the conservative self-designation item, which was confined to values from 1 to 5 with a mean of 3.4, we observed individual church scores from 2.1 to 4.5. Something associated with the churches, if only compositional differences between them, accounts for substantial interchurch variations in receptivity to political conservatism.

To assess the impact of context more directly, we will report a series of OLS regression analyses conducted at different levels of analysis and with different sets of variables. To preview the analysis that will be presented, the first set of equations (Table 1) looks simply at the contribution of congregational theology—the mean of personal theology scores in churches—to moral conservatism and conservative self-identification. The analysis is conducted separately for churches and individuals, giving a preliminary reading on the significance of congregational opinion in forming political outlooks of church members. The analysis is then replicated in Table 2 but with personal theology substituted as a predictor variable and church-level dummy variables included as proxies for contextual influence. In the final set of

analyses reported in Table 3, the joint impact of congregational and personal theology is assessed after those two variables are purged of collinearity through a statistical transformation; that analysis also includes a set of demographic and partisan variables linked by previous research to both political and religious conservatism. After the discovery of strong church-level influences on the attitudes of individual congregants, we will assess the perennial rival hypothesis offered by critics of contextual analysis, the self-selection model.

The case for contextual influence was first examined by an OLS regression equation conducted at the level of the church rather than the individual. For this analysis, the dependent variables were the average scores of church members on the two political conservatism measures. The independent variable was the mean church score on the theology index:

$$\begin{aligned} \text{conservatism} &= b_0 \\ &+ b_1 (\text{congregational theology}) \end{aligned}$$

The results of the procedure are listed in the first two rows of Table 1.

We found that mean church theology was a strong predictor of moral conservatism at the congregational level—explaining just under three-fourths of the varia-

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**Table 2. Regression of Moral Conservatism and Conservative Identification on Personal Theology**

Designation	Personal Theology	Constant	Adjusted R <sup>2</sup>	Number of Cases
Churches				
Moral conservatism	5.9**	6.3**	.33	600
Conservative identity	.5*	2.4**	.15	614
Individuals				
Moral conservatism	1.5**	15.6**	.57	600
Conservative identity	.2*	3.1**	.34	614

Note: See the note to Table 1. The equations reported in the last two rows included dummy variables representing the individual congregations.

\*Significant at .01.

\*\*Significant at .001.

tion in that measure—and a weaker but nonetheless impressive influence on self-designation as a conservative (multiple  $r$ -square = .32). For both dependent variables, increases in church-level theological conservatism produced the predicted conservatizing influence. The same pattern held when analysis shifted back to the individual level but the independent variable, theological conservatism, was based on aggregated data. For the analysis reported in the third and fourth rows of Table 1, we spread the church-level scores on theological conservatism (congregational theology) to each of the parishioners in a church and then used that variable to predict individual differences in political conservatism. The church-level measure contributed strongly to both forms of conservatism, producing for each one-unit increase a corresponding increment of 9 on the moral conservatism scale and .7 on the 5-point ideological spectrum. Both coefficients are strikingly stable and the equations are powerful (multiple  $r$ -square = .41 for moral conservatism and .17 for self-described ideology).

But a case can also be made for the significance of theology at the individual level rather than through contextual influence. To get a first reading about the con-

tribution of individual-level theological differences to sociopolitical perspectives, we analyzed the personal theologies of the individuals who composed the sample:

$$\begin{aligned} \text{conservatism} = & b_0 \\ & + b_1 (\text{personal theology}) \end{aligned}$$

In the equations reported in the first two rows of Table 2, personal theology also proved a strong and significant predictor of individuals' political conservatism. With an  $N$  of 600, each unit increase in the measure of theological conservatism accounted for an almost six-point increment in moral conservatism and magnified the ideology score by almost half a point; both coefficients were highly significant. The variance explained for moral conservatism and ideological self-designation was, respectively, 33% and 15%, an impressive degree of predictability for any single independent variable in survey research. This hardly disproves contextual influence because the addition of dummy variables representing individual congregations appreciably boosted the explanatory power of the equation to 57% for moral conservatism and 34% for conservative identification. The model tested in the third and fourth rows of Table 2 took the form

$$\begin{aligned} \text{conservatism} &= b_0 \\ &+ b_1 (\text{personal theology}) \\ &+ b_2 \dots b_{21} \\ &(\text{church dummy variables}) \end{aligned}$$

With both personal and congregational theology (the latter simply the mean of personal theology scores within a church) emerging as potent predictors of political conservatism in separate equations, the skeptical analyst will naturally want a direct comparison of effects, if only to insure that the impact of church-level variables is not simply an artifact of individual outlooks. Comparing individual and contextual impact is no easy matter because of the multicollinearity caused by the strong relationship between individual and group theology. This is not a problem in conventional contextual analysis because individuals make a negligible contribution to the overall behavior of the geographical unit where they reside. But with group means derived from samples that averaged 31 and ranged in size from only 10 to 60 per church, the correlation between personal and mean church theology rises to an unacceptably high level ( $r = .8$ ). The same problem occurs with the multiplicative term computed to gauge the interaction of personal with church theology ( $r = .9$  between this term and its two components). The inclusion of such highly correlated terms as individual theology scores, group means, and the product of their interaction massively destabilizes the coefficients for all three measures, making impossible a direct comparison of their effects.

Boyd and Iversen (1979, 60-76) have suggested an elegant solution to the problem of correlations between individual measures and aggregate values derived from them (see Tate 1984 for qualifications). The centering procedure first requires obtaining within-group slopes, means, and intercepts and then constructing new independent variables from them.

The variables constructed following this procedure are orthogonal and thus free of the intercorrelation that hampers multivariate analysis. Applied to our sample, the procedure first required separate regressions in each of the 21 churches, assignment of the slopes and group means to parishioners within each church, and construction of new measures representing both conservatism measures: individual and church-level theology. The resulting theology measures are uncorrelated, which makes it possible to enter them simultaneously into regression equations explaining moral conservatism and self-described ideology.

Following the procedures suggested by Boyd and Iversen, the variables were centered and then incorporated in the following model:

$$\begin{aligned} \text{conservatism} &= b_0 \\ &+ b_1 (\text{congregational theology}) \\ &+ b_2 (\text{personal theology}) \\ &+ b_3 (\text{congregational theology} \\ &\quad * \text{personal theology}) \end{aligned}$$

We find from the results in the first two columns of Table 3 no need to choose between personal and group theology, because both contribute to political conservatism. With each unit increase in personal theological conservatism (as transformed by the centering procedure), there is a gain of 1.7 on the moral conservatism scale. A corresponding shift in the theological climate of the church raises the score by 4.8 points. The interaction of personal with church theology magnifies the impact although the coefficient is not statistically discernible from zero. The standardized regression coefficients (not shown) confirm that congregational theology far outweighs personal religious beliefs in influence upon conservative social values. The pattern holds with conservative self-identification as the depen-

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**Table 3. Regression of Moral Conservatism and Conservative Identification on Personal Theology, Congregational Theology, and Selected Demographic and Partisan Variables (Centered Religious Variables)**

Variables	Moral Conservatism	Conservative Identification	Moral Conservatism	Conservative Identification
Religious				
Congregational theology	4.8***	.6**	4.8***	.3**
Personal theology	1.7**	.3**	1.4*	.2
Interaction of congregational and personal theology	1.0	.1	—	—
Political				
Party			.3	.2***
1984 vote			3.0***	.5***
Demographic				
Sex (1 = female)			-.9	—
Race (1 = black)			2.7	-.9***
Number of children			-.8**	-.1*
Age			.2***	.01**
Region (1 = South)			2.9***	.3*
Place (1 = rural, small town)			.1	—
Education			-.4	—
Occupational prestige	—	—		
Marital status (1 = married)			.9	.1
Constant	14.2***	3.0***	-2.6	1.3***
Adjusted R <sup>2</sup>	.10	.08	.32	.31
Number of cases	600	614	502	513

Note: Entries are unstandardized regression coefficients with coefficients less than .01 indicated by —. The table should be read down the columns, so that the first entry indicates an almost five-point increment in moral conservatism associated with every one-point increase in congregational theological conservatism.

\*Significant at .05.

\*\*Significant at .01.

\*\*\*Significant at .001.

dent variable. While personal theology enhances scale scores by over one-quarter of a point, the mean church theology raises the likelihood of conservative political identification by three-fifths of a point. The interaction effect is positive but modest in magnitude. The coefficients for personal and church theology in both equations are several times the size of the corresponding standard errors. On this evidence, the theological climate of opinion in the church has a greater impact than personal theology in promoting political conservatism among church members.

### The Specter of Confounding Effects

Thus far we have demonstrated that personal religious conservatism and church conservatism (as measured by congregational averages on the theological index) both contribute significantly to individual political conservatism. The analysis presented above does not confront the possibility of spurious correlation. The influence we have attributed to environmental properties may instead be the consequence of unmeasured individual traits correlated with patterns of

church membership. That members of any particular congregation evince a common political outlook may have more to do with their individual traits than to any congregational-level opinion formation. One response to this charge, audacious if not adequate, is to turn the argument around by insisting that individual influences are just as likely to function as proxies for unmeasured environmental influences. But a more satisfactory response is simply to try to identify the individual traits likely to promote a spurious relationship between the independent and dependent variables, measure them, and include the measures as controls in the analysis. For the problem at hand, social and political forces are the most obvious individual-level traits that might underlie an apparent contextual effect for theology.

The apparent impact of group theology on political outlooks may vanish with controls for what has been called the *modernization syndrome*. Christian orthodoxy is strongest in the sectors of society least exposed to modernizing influences (Hunter 1983). In practice, encapsulation in traditionalist environments has been indexed by socioeconomic variables such as education, occupational prestige, urban residence, and regional background. Traditionalists are most likely to report modest educational attainment, low occupational prestige, an upbringing in rural areas or small towns of the South. They are also more likely than the rest of the population to have large families. Three population groups—women, the elderly, and blacks—have been especially likely to exhibit strong ties to traditional religion and to reject moral liberalism. Perhaps the apparent link between group theology and political outlooks really reflects the clustering of traditionalist groups in certain churches.

There is also a possibility that political variables associated with membership in churches may contribute to spurious con-

textual inferences. If theologically conservative churches attract Reaganite Republicans and political liberals gravitate to more moderate denominations, that could produce an apparent congregational tie between theology and ideology. Of course, churches rarely if ever proselytize on partisan grounds but individuals who are disposed to one party or the other may be attracted to congregations with particular theological orientations. Distinctive partisan identities may also accrue to churches because of social biases in membership. Individual partisan tendencies should thus be controlled.

The equation was expanded to include measures of modernization and partisanship:

$$\begin{aligned} \text{conservatism} = & b_0 \\ & + b_1 (\text{personal theology}) \\ & + b_2 (\text{congregational theology}) \\ & \quad * \text{personal theology}) \\ & + b_4 (\text{exposure to modernization}) \\ & + b_5 (\text{partisanship}) \end{aligned}$$

Personal information items included at the end of the questionnaire were used to construct variables representing educational attainment (five categories), occupational prestige (the Duncan index for the chief wage earner in the household), age (in years), and number of children. Dummy variables were employed to indicate exposure to urbanism and Southern culture, marital status, gender, and race.<sup>7</sup> The dummies were coded with the most traditionalist category receiving the value of one. To measure partisanship, respondents picked from a five-point scale with options of *Republican*; *Democrat*; *Independent*; *Independent who usually agrees with the Republicans*; and *Independent who usually agrees with the Democrats*. We also included a measure of vote choice



in 1984 that grouped respondents into three categories: Reagan voters, Mondale voters, and nonvoters. The political measures were coded so that high scores indicated Republicanism and nonvoters were the median category.

The results in the last two columns of Table 3 provide eloquent testimony to the power of contextual influences even in the face of controls for political and social factors. With the additional measures, the equation predicting moral conservatism does increase its predictive capacity as evidenced by an increase in explained variance from 10% to 32% and by a corresponding decrease in the standard error of estimate from 9.1 to 7.9. Some of the additional regressors, notably presidential vote in 1984, family size, Southern background, and age, contribute significantly and predictably to moral conservatism. But these additional variables leave personal and contextual theology undiminished as powerful influences on socio-political outlook. As it did in the equation without controls, mean congregational theology remains a powerful predictor of moral conservatism, producing almost a five-point increment in that scale for every one-point increase in the congregation's theological conservatism. Based on the standardized coefficient (not shown), congregational theology is the single most important predictor of moral conservatism among the individuals in the study. Belonging to a church whose members embrace a conservative theology—over and above an individual's own theology or a myriad of social and political traits—enhances commitment to the application of traditional social values to policy issues.

The pattern holds, albeit to a lesser degree, for self-designated ideology. The control variables raise the *r*-square from 8% to 31% and similarly reduce the standard error of estimate from 1.3 to 1.1. As with moral conservatism, several of the new regressors exert significant influ-

ence on the tendency to embrace the conservative label in politics: presidential vote, family size, Southern background, race (with whites more likely to identify as conservatives), age, and partisanship. Unlike moral conservatism, the introduction of these controls does diminish the contribution of both personal and church theology. Despite some erosion in the coefficient, there is still evidence of a strong contextual influence. A one-point increase in mean church theological conservatism moves individuals approximately one-third of a point in the direction of conservatism on a continuum with five points. Though less powerful than partisanship, race, and age (based on the standardized coefficients), the theological climate of opinion in a church contributes more strongly to conservative political identification than several social traits and the individual's personal commitment to theologically conservative Christian values. This finding, together with the even stronger pattern for moral conservatism, seems to confirm that churches function as political communities by promoting attitudinal consensus: the more conservative the congregation theologically, the more strongly individuals within it adopt conservative political positions.

### The Self-Selection Hypothesis

Many U.S. citizens are loathe to admit that churches or clergy affect their political outlooks. Both the individualist culture and a tradition of strict church-state separation militate against accepting the church as a legitimate source of political information and persuasion. The evidence assembled here demonstrates nonetheless that churches do indeed promote distinctive political orientations. Subject to the limits of the data source, the results indicate that the extent of theological traditionalism prevailing in the congregation moves individual members to more con-

**Table 4. Relationship between Personal Theology and Church Orientation**

Personal Theology	Type of Denomination		
	Liberal	Conservative	Fundamentalist
Modernist	178 (27%)	20 (3%)	6 (1%)
Moderate	96 (15%)	73 (11%)	24 (4%)
Orthodox	23 (4%)	100 (15%)	132 (20%)

Note: Number of cases = 652.

servative preferences on social issues and makes them more disposed to identify as political conservatives. The effect operates over and above the personal theological beliefs of the individual and a variety of individual traits that contribute separately to conservatism. All other things being equal, a theologically conservative individual belonging to a theologically conservative church is more likely to develop a conservative political outlook than an equally traditionalist Protestant who belongs to a church whose members are less conservative on theological issues. Similarly, two churchgoers equally liberal in theology will differ in their propensity to adopt politically conservative positions and labels according to the theological climate in their respective churches.

If this finding is to be accepted, it is necessary to confront an alternative hypothesis that is routinely put forward in opposition to contextual theories of behavior. According to the self-selection hypothesis, what appear to be contextual influences are in reality the result of individuals choosing environments—in this case, churches—on the basis of prior political beliefs. That is, Unitarians may be politically liberal and independent Baptists politically conservative not because of anything that goes on in church but because political liberals select churches with liberal political reputations

and political conservatives search for congregations with politically conservative outlooks. According to this view, self-selection on the dependent variable produces the appearance of environmental influence on behavior.

Lacking longitudinal data, it is impossible to test this assertion directly; but we do possess data that allow an indirect assessment of self-selection processes. If the choice of church membership is as rational and purposive as the self-selection hypothesis implies, then surely religious belief plays an important role in congregational affiliation. But our data show only limited congruence between the theological orientations of individuals and the denominations to which they belong. As indicated by the pattern in Table 4, the correlation is incomplete on the dimension where self-selection is most likely. The column variable represents the predominant theological orientation of the church to which the respondent belongs. It was derived from expert judgments in the literature that commonly apportion denominations between liberal, conservative, and fundamentalist camps. The rows represent the personal theology score described above. The table reveals that over 40% of the churchgoers in the sample exhibited discrepancy between personal theological beliefs and the predominant theological orientation usually ascribed to their denomination. Even

allowing for measurement error and the roughness of the classification scheme; the magnitude of the inconsistency is remarkable. Despite access to a religious "marketplace" with ample options, it seems that many individuals do not seek out the church that most closely "fits" their theological views. Social factors, family traditions, a sense of fellowship, convenience, the appeal of a particular minister, intermarriage, and other factors probably account for the selection of churches that do not match foundational religious values.

The finding of imperfect correspondence between personal and congregational theology in Table 4 raises doubts about the larger claim of critics regarding self-selection of churches on political grounds. If many church members do not self-select on the basis of religious belief, surely a sensible basis for choosing a religious organization, then it is hardly persuasive to argue that they self-select on the basis of politics. In the hierarchy of factors that determine religious affiliation, it is likely that political compatibility ranks below theology and the social factors that probably weigh heavily in the selection of a church. With a research design that precludes a more direct assessment of the political self-selection hypothesis, the indirect test can do no more than call into question the logic that sustains an anticontextual interpretation of our findings. Together with analysis showing that contextual influences are not merely the residue of unmeasured individual dispositions correlated with the independent variables, the findings suggest the operation of politically relevant social influence mechanisms within the confines of the congregation.

### Discussion

To recapitulate the major findings, we observed a strong association between the

predominant theological temper within congregations and the political views maintained by church members. Conservatism in congregational theology was correlated both with congregants' conservative preferences on social policy and self-identification as a political conservative. Once personal and congregational theology were disentangled, the evidence suggested that the collective outlook of the church was more politically influential than the worldview of the individual church member. The relationship between congregational theology and political outlook also withstood controls for individual traits that might contribute independently to political conservatism and seems unlikely to have resulted from the selection of churches on the basis of political compatibility. We have explained this pattern from a contextual perspective. Messages from the pulpit and social interaction with congregants apparently promote a common political outlook among church members.

This analysis suggests a need to redirect inquiry on religion and politics from an individualist perspective toward models that stress the formative influence of the immediate religious environment. Typical research in the genre operationalizes "religion" as a pattern of individual religious belief; the analysis in this study suggests that religious commitment bears on politics primarily through the medium of communities of belief. The disjuncture between the individualist and congregationalist approaches can be illustrated by congregants whose religious context subverts the logical "individual" link between personal theology and political orientation. Consider the case of a theological conservative who found his or her way into a mainline church where most members subscribed to liberalism in theology and politics. To the extent it can be generalized, our evidence suggests that the liberal political messages emanating from the congregation would move that individual

away from the conservative political beliefs implicit in traditionalist theology—even as the personal theology was maintained without change. By the same token, a theological liberal in a religiously conservative congregation should respond to clerical and congregational influence by embracing more conservative political positions than personal theology would otherwise dictate. Were they to be included in a typical national survey that correlated individual theology with political preference, the two individuals just described would be classified as deviants and counted as evidence *against* the political impact of religion. Inferring from Table 4 that a considerable number of churchgoers do indeed find themselves in congregations with theological traditions different from their own, the extent of such deviance among church members is likely to be substantial. Without a contextual approach sensitive to the theological milieu that confronts individuals in their churches, the true magnitude of religious-political linkage is thus likely to be significantly underestimated.

The discovery that individual churches promote political homogeneity consistent with their dominant theological orientation has serious implications for contemporary U.S. political life. The significance of church influence on mass political orientations is magnified by two factors already noted. First, churches are the most widespread form of voluntary organizational affiliation in the United States, far outstripping labor unions, political and social action groups, and other secondary associations. To the extent that churches mobilize politically, they operate on a sizable share of the population. Secondly, a large number of individuals within churches are apparently ripe for political conversion. The lack of perfect correspondence between personal and church theology, a function of the individualist strain within U.S. religion and the social selection of churches,

enlarges the scope for contextual influence. If the mechanisms of influence demonstrated in our church samples apply elsewhere, then churches deserve more political attention than they have heretofore received.

To return to the major theoretical implication of contextual influence, the individualist view of political decision making requires sharp qualification. Individuals do not simply reason through the political implications of their religious views in a vacuum. When the environment sends political messages that conflict with core theological assumptions, there is a pronounced tendency among churchgoers to bend to the political guidance offered by the congregational environment. For contemporary observers of politics, as for Emile Durkheim almost a century ago, the "social fact" of religious context warrants close attention.

## Appendix

Twenty-seven Protestant congregations apportioned among four theological categories were contacted and asked to participate in a survey study about "what church members think these days." Twenty-three accepted with the four refusals attributed to (1) a lack of time or opportunity for survey administration, (2) concern that many congregants would not understand the questions, or (3) objection to specific questions by members of lay governing boards. Because of the circumstances of questionnaire administration, precise refusal rates cannot be determined, but we observed very few instances when questionnaires were refused or returned unanswered.

The "moral conservatism" scale comprised the following Likert-style items. *R* indicates that the response categories were reversed for scale construction:

1. Local governments should be allowed to ban books and movies that they

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## Political Communities

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- think are harmful to the public.
2. Birth control devices should be available to any adult who wants them. (R)
  3. Men and women should have the same legal rights. (R)
  4. There are too many shows on television that make fun of traditional family values.
  5. Women are happiest if they stick to keeping a home and raising children.
  6. There should be laws against marriage between blacks and whites.
  7. Abortion should be a private matter between a woman and her doctor. (R)
  8. The government should prohibit the private use of marijuana.
  9. Homosexuals should be able to do what they want to so long as they don't hurt other people. (R)
  10. If a man and a woman want to live together without getting married, that's their business. (R)

church constitutes an immediate network of relationships—a *context*—rather than an amorphous zone of existence, which should be called an *environment*.

5. The 10 items that compose the scale were balanced between expressions of conservatism and liberalism (see Appendix for exact wording). For scale construction, scores on the liberally worded items were reversed. The items were distributed among a pool of questions that also measured political efficacy, cognitive traits, and other dispositions. Each question was followed by five options of *agree* or *disagree strongly*, *agree* or *disagree somewhat*, and *neither agree nor disagree*. The neutral position was treated as a midpoint.

6. In pretests, some self-avowed theological liberals were reluctant to select this option as offered by Gallup because it contained the word *fables*. We thus reworded the item to take out the offending reference.

7. Respondents were asked to name the state and size of the community where they mainly lived while growing up. We followed the standard practice of the National Election Study in identifying southern states. Residents of farms, rural towns, towns, and cities under 50 thousand in population were assigned a dummy variable value of one indicating size of place.

## Notes

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1. The problems of denominational measures include the presence of respondents who are minimally committed to the church, radical theological differences within denominations, and the failure of many surveys to partition large groups like Baptists, Lutherans, and Methodists into more meaningful categories.

2. We excluded church visitors and members under the age of eighteen from the analysis reported here. The restriction to Protestant congregations reflected our belief that a single survey instrument could not satisfactorily assess core theological values in more than one religious tradition.

3. Most of the excluded cases were missing information about personal characteristics used as control variables.

4. The terms *context* and *environment* are used interchangeably in much of the literature. As a center of social interaction, it is likely that the

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## LIBERATION THEOLOGY: THE RECOVERY OF BIBLICAL RADICALISM

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*L*iberation theology is not a Marxism in Christian disguise. It is the recovery of a biblical radicalism that has been harbored in the Judeo-Christian tradition virtually from its founding. As such, liberation theology is the revitalization of one of the most profound social, political, economic, and religious challenges to established hierarchies that the West has ever known. Liberation theologians turn to modern Marxism chiefly to gain a comprehensive understanding of contemporary class conflict and poverty. Their primary concerns remain to define themselves as an "emergent church" in the biblical tradition, and to reflect upon the praxis of the "base community" movement they have spawned throughout Latin America. However, the principal liberation theologians of our day exhibit persistent ambiguities about how individuals should relate to their communities. The biblical materials suggest certain solutions to them, but ones so radical that not even the most extreme of the liberation theologians appear ready to embrace them.

**L**iberation theology is a powerful presence throughout Latin America, and is attracting increasing international attention. Recently the popular press has taken up the story, largely because of contentions between the Vatican and certain Latin American priests, most notably, Fr. Ernesto Cardenal, a prominent member of the Nicaraguan revolutionary government, and Fr. Leonardo Boff, a prolific theologian in Brazil. The press accounts leave the impression that liberation theology, with its pleas for "class struggle" against entrenched local elites and imperialist "capitalist forces"—principally located in the United States—is a Marxism only thinly *disguised* as Christianity.<sup>1</sup> A number of widely believed propositions appear to sustain this view.

First, Marxism is widely understood to despite all religions—and none more than Christianity—as "opiates" of the people. As such, Marxism is thought to be incap-

able of sustaining any authentic religious focus. This position is reenforced by Marxism's frequently declared commitment to "atheistic materialism." Second, Christianity in its turn, it is thought, must categorically reject all Marxisms not only because of their godless materialism but also because their commitment to "class struggle" collides with Christian beliefs in the need for indiscriminate love for all persons without regard to social and economic status. Moreover, Christianity is widely understood not to share Marxism's central concern with worldly social and economic issues. Christianity is supposed to be centrally concerned with "mystery" and the problems of personal redemption from sin. On this view, Christian concerns for alleviating human misery in this world should be expressed through charitable institutions, not down avenues of political activism.

The supposed incompatibility of Christianity and Marxism depends on narrow,

even peripheral, definitions of both. In their actual, full complexity, many points not just of similarity but of genuine congruence between them can be found. In particular, Marxism's "materialism" is far more "prophetic" and "spiritual"—and Christianity's "spiritualism," especially in its biblical mode, is, as we shall see, far more "materialistic"—than is often thought. But the concerns of this essay go well beyond issues of this sort.

It needs no detailed exposition to show that the liberation theologians are indeed, in a formal sense, mainly Marxists or that they are so for many of the same reasons that attract non-Christians to that ideology. By their Marxism, liberation theologians tilt their Christianity, decisively and consistently, to enthusiasms for the transformation of this world in its current historical dimensions. Marxism drives them not only to theories of the conflictual nature of modern society but also to an implacable outrage over the facts of poverty, especially in the extreme forms in which it is experienced in Latin America. And, most obviously, Marxism supplies the liberation theologians with a systematic, comprehensive, structural explanation for the occurrence of poverty in the modern world and, even more specifically, for the "dependence" of third-world economies on those of the major capitalist nations. In this context, the serious questions that have to be asked all pertain not to whether the liberation theologians are Marxists but to the nature of the positive, substantial, causal relationships between the liberation theologians' Christianity and their Marxism.

The thesis of this essay is that the liberation theologians are essentially Christian radicals and approach Marxism from that foundation. What they most represent is the recovery, within the general context of the Christian tradition, of a profound biblical political radicalism. This is the basis for their almost paradoxi-

cal claim that they are true to Christianity's most ancient roots and at the same time constitute the "emergent church." Biblical radicalism is also the source of the distinctively pre-Marxist, almost Rousseauian slant that the liberation theologians give to their understanding of their theology's most prominent practical expression, the Christian "base community" movement that has spread throughout Latin America.

### Two Theistic Traditions

Essential to any discussion of this hypothesis is a division that can be seen running through the whole of Christianity's two-thousand-year history, a division that is most fundamentally expressed in differing conceptions of the godhead. Willis B. Glover, in an article discussing Hobbes's supposed atheism, makes the distinction well:

The intellectual and religious heritage of Western Europe includes two theistic traditions: (1) the theism that developed out of Greek rationalism, in which God is perfect, unchanging, pure act without the possibility of action, rational and yet impersonal; and (2) the theism of the Biblical tradition in which God is personal, Creator, immediately sovereign over nature, one who acts in history. These two understandings of God are ultimately irreconcilable. The insight of Pascal at this point led to his sharp distinction between the God of the philosophers and the God of Abraham, Isaac, and Jacob. Many, if not most, of the difficulties in Christian theology result from the contradictions between these theisms which are so complexly intertwined in the history of Christian thought. (1965, 163)

This distinction historically was far more than the occasion for much blind misunderstanding and for the trading of bitter charges of atheism and the like. The two sides also strongly tended to develop sharply opposed social, political, and economic outlooks that sometimes overshadowed their theological differences. Though Glover is right to emphasize the way the two sides were constantly inter-



twined, it can be held nevertheless as a broad generalization that the cosmological, rationalist god came to be associated with proponents of well-ordered, clearly institutionalized societies. The ordering applied to both church and state, supplying content to both political philosophy and ecclesiology. In the process, faith itself was transformed into—specifically—doctrine to be taught, studied, believed, and professed. The conservative and system-sustaining overall tendency was toward hierarchically defined orders of church and state—of bishops and kings each sovereign in their own realms yet cooperative.

On the other side, the personalist, voluntarist, virtually existentialist and historicist understanding of God, tended as powerfully toward association with separatist sects often having politically radical characteristics. It was no accident that the proliferation of such groups frequently coincided with the sudden availability of the Bible in translation in cheap, popular editions. On this side, there was a particular focus on *faith*, on a concern for personal acceptance and living commitment. Faith, in these terms, took Joan of Arc to the stake, enabled Luther to take his stand, and sent John Winthrop to Massachusetts and John Brown to Harper's Ferry. Moreover, beyond these often startling degrees of personal, evangelical commitment was a sustained evocation of a radical, wholly generous egalitarianism that extended from purely religious concerns deeply in to the social, political, and economic arenas.

The full spectrum of shadings between the polarities of this fundamental division in the Christian tradition was laid out in the period of the English seventeenth century civil wars—from divine-right Catholicism to high-church Anglicanism to middle-class Presbyterianism to the extremisms of the Levelers and Gerrard Winstanley's Diggers (Hill 1972; Morris 1949; Walzer 1965).

### A Divided Church in Latin America

An assertion fundamental to the present argument is that this ancient division in the Christian tradition has now split the supposed monolithic structure of the Roman Catholic Church in Latin America. Liberation theology and its attendant Christian base community movement can be seen occupying the left wing of this division, leaving the other side to various accommodationists, rightists, and establishmentarians. In this light the liberation theologians can be seen to have recovered for their own time and place a biblically derived political radicalism. Their concerns are modern and particular, and many of their techniques are indigenous; but their mood, their overriding goals, and even much of their language is centuries old. They are the latest recruits to a line stretching back not only through contemporary radical Protestant sects such as the Amish and the Mennonites but also back through the Levelers and Diggers, the Anabaptists and Lollards, all the way back to the first century Christian communities before the time of Constantine, rooted in New Testament experiences (Acts 2:43–47; 4:32–37).

As essentially the recovery of a biblical political radicalism, liberation theology has enormous strengths and many tactical advantages. It is these biblical roots that have enabled liberation theology to use in creative and legitimate ways a wide array of Marxist concepts.

### Biblical Radicalism

#### The Prophetic Mindset

It has been popularly suggested that the liberation theologians—like the black slaves in the United States of the nine-

teenth century—have recovered the memory of Moses' liberation of the Hebrew people enslaved in Egypt. But the issue cuts far deeper than this. Liberation theologians have recovered the Bible's prophetic mind-set, a mind-set that colored and formed the Moses stories, indeed, but also the historical books, the writings of the great social prophets, and the New Testament accounts of the ministries of Jesus and Paul.

The core of the Bible's prophetic mind-set, at the most fundamental philosophical level, is a concern for immediate, factual, sensuous experience, an absolute nominalism.<sup>2</sup> Inevitably, this is a concern for experience in its totality and without clear possibilities for categorical differentiations except as between interrelated particulars within an experienced, factual totality. What is, is what is sensed—comprehensively. It can be argued that it is this original "existentialist bias"—an innate, primal materialism—that renders the Hebrew biblical mind-set incapable of making abstract distinctions between body and soul, mortality and immortality, heaven and earth, and, at a more practical level, between public and private, religion and politics, church and state. At the one extreme, since body and soul are experienced only in factual combination, neither has any significant reality separately. In factual experience, the body is either alive (with soul) or dead (without soul). God is not an "object" to be "known," up there in a supernatural realm. "Nature" is the only realm the Hebrew knows ("experiences"). Therefore, for the Hebrew, God is a subject to be not known but *met* (feared, loved, served) in the here and now. Similarly, since religion and politics and their institutionalized expressions in church and state appear only as elements in the whole experienced life of a community, any separation between them must be artificial, prudential, and a denial of their essential theoretical and factual unity.

These reflections heavily underscore the realization that what awakens and attracts the Hebrew mind is not just *factual* existence but *active* existence as experienced, factual life. At the core of the Hebrew mind is an abiding concern with vitality and thrust—in a phrase, *factual will*. For the Hebrew mind, the whole "world" is alive; men and women hate and fear, work and love; grass grows and withers; stones hear, wind sings; and the great god Yahweh is before all else the giver and breather of life, immediate, sensuous, factual—and active.

The central concern of the Hebrew mind-set is, then, with action—action *in fact*—and this concern necessarily spills over into two further dimensions. The first of these is time, a sense of story, as in both history and biography. This is not linear time as in science but time remembered, felt, and anticipated by a subject, as in narrative literature. Time is a story told by a subject about a present—an experienced now between a past recalled and a future anticipated.

What gives the story substance is its second dimension, its meaning. This derives from judgments about the relationships between the components of time and people alive in these components. In those past, present, and future components of time people acted, are acting, will act. But will the relationships and commitments that were established between these actors in time endure? How will their story end? Will promises of life and loyalty be kept? Will identities be sustained? How will the tests of time be met? These judgmental questions are the prophetic element in the Hebraic mind-set. Characteristically, they are essentially interpretations of a present, sensed between its past and future. The prophetic mind is a mind in questioning confrontation with its existential situation; "Here I am! So? What have I done? What god have I served? By that service, who have I become? What will become of me now?"

### Liberation: Old Testament Tradition

Nothing more marks the modern day liberation theologians as *biblical* than that they begin their work from a fact, a fact of their existence, an overwhelming fact that oppresses their consciousness at every turn, the indescribable misery, poverty, and marginalization of a huge proportion of the populations in the midst of which they live and work and profess to be Christians. Before that fact they feel compelled to stand and take sides. Traditional, more cosmologically inclined theologians might survey social poverty within the context of speculations about the divine nature and the ordering of creation. They might conclude by finding in the divine order a place, honorable and perhaps even deserving, for the poor. The liberation theologians (Barreiro 1982; Boff 1985; Bonino 1979; Gutierrez 1973, 1983; Metz 1981; Miranda 1974; Santa Ana 1979; Tamez 1982) reverse all those older priorities. Like the ancient prophets they not only confront poverty, they narrowly focus on it and denounce it without quarter and with the promise that the god who defines their natures commands that it be overcome here, now, in this world. Traveling through one channel or another, it was the influence of these theologians with their prophetic focus on the facts of poverty that prompted the General Conference of the Latin American Episcopate, meeting in Puebla, Mexico in 1979 to include in its final document the famous paragraphs.

From the depths of the countries that make up Latin America a cry is rising to heaven, growing louder and more alarming all the time. It is the cry of a suffering people who demand justice, freedom, and respect for the basic rights of human beings and peoples.

A little more than ten years ago, the Medellin Conference noted this fact when it pointed out: "A muted cry wells up from millions of human beings, pleading with their pastors for a liberation that is nowhere to be found in their cases."

The cry might well have seemed muted back

then. Today it is loud, clear, increasing in volume and intensity, and at times full of menace. (Eagleson and Scharper 1979, 134)

In the context of this judgmental existentialism, the liberation theologians are truly recoverers of Moses. But what they recover in this context is not Moses as religious lawgiver and builder of a holy nation but Moses as a liberator of an oppressed people. What they recover is Moses as the founder of a tradition that besides being religious is at once political and radical and that stretches through the whole Bible to the Messiahship of Jesus himself, however much it may have changed, strengthened, and developed as it went.

There can be little argument about these assertions in the present context. Of course Moses was a religious figure, but—without differentiation—he was also a political figure. Essentially, he was the people's heroic leader, with responsibilities that ran the gamut from divining their future and defining their historic identity to organizing their government, decreeing their laws, and leading them in battle. In this, his figure reflected the comprehensive character of the biblical concern for the totality of a people's experience. Centuries later, Nathan, the court prophet, stood before the king to condemn him for his personal sin; but Nathan's story is told as a morality tale directed at the corporate guilt of the whole people. Elijah is listed as the first of the great social-political prophets, but what he condemns the people for is their sin in following the religious idolatry of their queen. Amos thunders religious imprecations against the people equally for their personal impieties and their political, economic, and social oppression of the poor. Can we suppose that Jesus, coming out of this ancient and unbroken tradition, had motives and concerns that were any less comprehensive and complex?

That this tradition was also radical is

only somewhat more problematic. That Moses was a liberator is obvious. The oppression in Egypt is vividly portrayed, as is also the escape from it. But so also is the need for purification and transformation, in short, for conversion. The episode of the golden calf, with the subsequent slaughter of the guilty by the Levites on Moses' personal command, is the outstanding example of this (Ex 32; Walzer 1985). But the whole forty-year period of wandering in the desert under Moses' rule can be seen as an experience of preparation and renewal before the entry into the Promised Land. Certainly the Gospel writers appear to have interpreted it that way when they made Jesus' period of temptation in the wilderness precisely forty days long.

The radicalism of the prophets is even more patent. For them, reform of policies, observance of the letter of the law, and appropriately executed ceremonies and rituals would never be enough (Amos 5:21-24). Only a radical reordering of priorities, a total recovery and recommitment of loyalty to the ancestral god, Yahweh, would bring the changes of spirit and behavior necessary to a restoration of the Covenant. Only then could Israel claim to have renewed its identity as a holy people. A transforming, deeply confessional, radical change of identities and gods was required.

### Political Radicalism: New Testament

On the other hand, in the New Testament, the political radicalism of Jesus and Paul is obscure; that is, it has been obscured, consistently so by authorities with ulterior motives. Liberation theologians and North American scholars such as John Howard Yoder (1972) argue persuasively that the political radicalism of the New Testament has been authoritatively denied, veiled, and ignored from the beginning of Christianity down to our own day by established hierarchies who

had no interest in admitting its being in the texts, much less in teaching it. The actual radicalism of Jesus was too strong and too specifically political to be countenanced. For the established hierarchies of the European Christian world, Jesus came instead to be authoritatively portrayed as a specifically religious, spiritualized figure—and biblical study by ordinary folk on their own was actively discouraged or even on occasion suppressed.

For those who make the effort to see it, however, the political radicalism of Jesus and Paul is plain enough and deeply rooted in the biblical tradition. In consists of three, interlocking principles:

1. *Radical egalitarianism.* All human beings are, existentially, equal. The distinctions between them drawn in our social orders—by age, sex, wealth, power, skills, learning, beauty, and so on—are all of little, artificial, and passing significance. ("Blessed are the meek" [Mt 5:5]; "There is neither Jew nor Greek, there is neither slave nor free, there is neither male nor female; for you are all one in Christ Jesus" [Gal 3:28].)<sup>3</sup>
2. *Radical communitarianism.* All persons should "love" one another indiscriminately. This is love in the technical, Hebraic sense, which sees "souls" (vitalities) drawn (merged) by love into one life (experience—including death). ("This is my commandment, that you love one another as I have loved you. Greater love has no man than this, that a man lay down his life for his friends" [Jn 15:12-12].)
3. *Radical pacifism.* The essential tactic by which to achieve these goals is a direct, nonviolent confrontationalism. It is engagement through a revolutionary passivity. ("Bless those that persecute you . . . do not curse them. . . . Repay no one evil for evil. . . . overcome evil with good" [Rom 12:14-21].)

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## Liberation Theology

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Each of these propositions is given extensive elaboration and illustration in the records we have of Jesus' ministry and Paul's letters. However, some of the most important passages where this elaboration appears require reinterpretations that turn "traditional," hierarchically approved, understandings of them inside out. Thus the famous phrases (Mt 22:15-22) about rendering unto Caesar the things that are his, and to God his, far from being primarily significant for making a distinction between public and private life, in fact are to be interpreted, in a spirit of radical egalitarianism, as showing that Caesar gets only money, the symbol of a despised materialism; while God, radically, gets all honor, glory, power, and so on. In the same way, radical communitarianism requires us to love not only "good" Samaritans but also all other strangers and outcasts in our midst and also, even more importantly, our "enemies," even while they hate and persecute us. However much we may fear and, in our turn, hate what they do, we are urged to join lives with them—even as we still clearly see them as enemies.<sup>4</sup>

Finally, there is the radical pacifism of the Crucifixion. Without compromising religious perceptions that the Crucifixion was a redemptive act, it is important to notice, in the context of the events leading up to it, that Jesus had, and rejected, clear political alternatives. Faced with the weight of Roman imperialism and the seething presence in the Palestine of his time of a repressed Jewish nationalism, Jesus could have withdrawn "into the desert," as did the Essenes and the community at Qumran. Or he could have conspired to lead an armed uprising, such as the Zealots of his day hoped for and the Maccabees undertook before them with mixed results. Or, like Herod, he could have collaborated. Instead, he "set his face" to go to Jerusalem. He came as the king, the Messiah—on a donkey, not a horse of war. He accepted the plaudits of

the masses, who waved palms, not spears and banners. And he directly engaged—passively—the Roman authorities and the Jewish collaborators. In this light, the Crucifixion was a deliberate public act that administered an unmistakable and massive rebuke to all the established "powers and principalities" of the day—and was proved victorious in the subsequent record of the Resurrection.

Of course—as before with Moses—Jesus' political radicalism is also a religiosity. Radical egalitarianism, radical communitarianism, and radical pacifism can all be seen as the social expressions of taking the two great, ancient, religious commandments with an absolute, uncompromising personal seriousness: "Thou shalt love the Lord thy God with all thy heart, all thy strength, and all thy mind" and "Thou shalt love thy neighbor as thyself." Moreover, it must also be said that throughout the Gospels, Jesus' political radicalism is consistently set in the context of a highly personal religiosity. His baptism, temptation, and much of the content of the Sermon on the Mount, including the Lord's Prayer, are all testimony in support of this. So too, dramatically, are the scene at Gethsemane and the last words on the cross. Personal, confessional prayer, in the record of the Gospels, is a major element in Jesus' life. But having said all that, it must be added immediately that this personal, confessional element is never portrayed as something separate and different from Jesus' public behavior. Jesus in the record is not a recluse, a mendicant, or hermit. First and last, he is a public figure, a leader. The personal religiosity is uniformly and exclusively a ground and preparation for his public roles, just as it was for Abraham in the land of Moriah, Jeremiah in the court-yards of Jerusalem, and Job in his poetry.

Three other features of Jesus' political radicalism of special interest to liberation theologians need to be emphasized. First, this radicalism must be seen as not iden-

tical with political enthusiasms of the Old Testament but as growing and developing out of them. The concern for social justice runs without break throughout the biblical literature. But the expression of this concern in narrow, nationalist, legalistic terms was rejected—in language that grew increasingly stringent—in the Old Testament as well as the New, as the biblical tradition developed. Second, parallel to the first point, is the fact that what Jesus was centrally calling for, as were the great prophets before him, can only be termed a *revolution*, a radical restructuring and re-ordering of social values and priorities (Mt 5:3–10). Jesus did not have the institutional sense and technical precision to make his demands with the programmatic detail a modern radical would. But once his demands are understood in political terms and set in a politicized context, there can be no mistaking either their depth or their breadth. The call is for the reduction of the structures of the world's powers and principalities to paltry significance and their transformation into the patterns of a radically egalitarian, communitarian, pacific anarchism.

The final feature of Jesus' radicalism of special interest to liberation theologians is the claim that he did indeed, speaking out of the whole of the biblical tradition, politicize his context. The liberation theologians scour the literature to support this claim with appropriate citations—and are amply rewarded. Their most usual findings (Brown 1984; Miranda 1974, chaps. 1–2; Tamez 1982) include the story of David and Nathan (2 Sm 11–12) and of Jeremiah and Jehoiakim (Jer 22) from the Old Testament and, from the New, Mary's song, the Magnificat (Lk 1: 46–55), and Jesus' vision of the day of judgment on the nations (Mt 25:31–46). All of these are extended selections and they are given a full, literal, word-by-word reading in their contexts. What comes out of them is an insistence that the Gospels in particular but also the Bible as

a whole from the Exodus onward is good news for the poor and oppressed and bad news for the mighty and the rich (see also, specifically, Mk 10:17–31). The thesis is, as well, that piety is not best expressed by worship in churches and synagogues but by doing justice in the homes, markets, and work places of the poor and the oppressed. Nothing expresses this thesis better than the preference the liberation theologians have for Luke's version of the beatitude "Blessed are you poor" (Lk 6:20)—for it does not add the phrase *in spirit* as in the Matthew version (Mt 5:3)—and its follow-up, "But woe to you that are rich," which has no parallel in Matthew.

For the liberation theologians, all these points come together in the account given in Luke 4 of Jesus' return at the beginning of his public ministry to the synagogue at Nazareth:

And he stood up to read; and there was given to him the book of the prophet Isaiah. He opened the book and found the place where it was written,

"The Spirit of the Lord is upon me,  
because he has anointed me to preach  
good news to the poor.

He has sent me to proclaim release to the  
captives

and recovering of sight to the blind,  
to set at liberty those who are oppressed,  
to proclaim the acceptable year of the  
Lord."

And he closed the book, and gave it back to the attendant, and sat down; and the eyes of all in the synagogue were fixed on him. And he began to say to them, "Today this scripture has been fulfilled in your hearing." (Lk 4:16–21)

## Emergent Communities

### "An Option for the Poor"

The claim of the liberation theologians that Jesus dichotomized and politicized his social world has given to reading the Bible an unexpected excitement especially for North American and Western European readers (Brown 1984). Comparably, it has opened up for modern Christian

churches possibilities for social vitality that had been smothered by decades of obscurantism and neglect. A major consequence of this renewal of vitality has been the doctrine of the "emergent" church (Boff 1985; Metz 1981; Torres and Fabella 1978). The emergent church, looking to Jesus as its founder, declares "an option for the poor." In contradistinction, the "established" or "traditional" or "bourgeois" church is depicted as having sustained throughout the modern era "an option for the middle class." In these terms, the denunciation by the liberation theologians of the bourgeois church is scathing.

Much of this denunciation is in Marxist terms. On the one hand, it is charged that the bourgeois church mystifies the poor, dupes them into accepting their sufferings as badges of an acceptable status among Christians. It lulls them with promises of bliss in the hereafter, and comforts them with a wide variety of charitable supports but of a sort which, even when taken in total, never amount to more than "assistentialism," that is, a policy that distributes benefits and aids but does not attack causes or structures. Mostly, however, the traditional, bourgeois church, especially the Roman Catholic Church in Latin America, simply ignores "the poor," a term the Liberation theologians define inclusively as meaning "those who have no place to live, nothing to wear, and nothing to eat, . . . the blind, the deaf, the crippled; the prisoners, the strangers, the orphans, and the widows; the afflicted, the depressed, and the hopeless . . . the feeble, weak members of society . . . who have neither a voice nor an opportunity in society" (Barreiro 1982, 15). By and large, these people simply go "unchurched."

On the other hand, the traditional church is seen as having powerfully sustained the values and institutions of established elites in the church, in the state, and in the economy. In its own magisterium, it has succumbed to an "ideology of

power" (Boff 1983, chap. 5) and resists all efforts to convert it "from a 'paternalistic church looking after people' to a 'church of the people themselves'" (Metz 1981, 12). It long ago bought into the psychic foundations of the modern age, in which, by "seizing power over nature, man's identity is formed. Man is by subjugating. All non-dominating human virtues such as gratitude and friendliness, the capacity for suffering and sympathy, grief and tenderness, recede into the background" (p. 35). With the state, it concluded if only in effect a "concordat" (Cox 1984, chap. 7), often under the guise of doctrines of the separation of church and state, which appear to suggest a policy of live and let live. But these arrangements only thinly masked a practice of mutual support. The state protected the church, provided it with a full array of services that often included huge but hidden tax subsidies. In its turn, the church celebrated the state and its symbols through the full panoply of civil religion. Equally, the traditional church celebrated the values of the capitalist economy. It lauded those who accumulated great wealth—and were generous with it—and managed to accumulate great wealth itself. Meanwhile, characteristically, its clergy socialized with the political and economic elites and educated their children.

Over and against the traditional, bourgeois church, liberation theologians see the emergent church. Centrally, the emergent church is a call for "an anthropological revolution," an effort to achieve "a new subjectivity," "a new relationship to ourselves, to our natural and social environment, which is not one of domination and exploitation" (Metz 1981, 42). The will to power would be replaced by the will to love. Only through love and solidarity with others can true liberation be achieved; only then can "freedom from" and "freedom to" be transcended by "freedom with"—freedom in solidarity with others, especially the poor. As Gutier-

rez, probably the most powerful voice in the liberation theological movement, has written, "To conceive of history as a process of the liberation of man is to consider freedom as a historical conquest; it is to understand that the step from an abstract to a real freedom is not taken without a struggle against all the forces that oppress man, as a struggle full of pitfalls, detours, and temptations to run away. The goal is not only better living conditions, a radical change of structures, a social revolution; it is much more: the continuous creation, never ending, of a new way to be a man, a *permanent cultural revolution* . . . in order to achieve an ever more total and complete fulfillment of the individual in solidarity with all mankind" (1973, 32-22).

For liberation theologians the church's role in this "permanent cultural revolution" will be primary but "only insofar as it is converted and becomes more and more an incarnation of the Gospel" (Boff 1985, 64), the reemerged Gospel in all its social and political radicalism. Central to that conversion to the recovered Gospel will be the church's involvement, with all of its resources, with the poor—not the middle or the top of society but its *base*. The connecting argument here, between the emergent church, the recovered Gospel, and the poor, is that the poor, the dispossessed, the marginalized in society are uniquely placed to read, interpret, and respond to the Bible. The Bible was written *by* people much like themselves, *about* people much like themselves, and, decisively, *for* people much like themselves. Having no interest or stake or identity in present society, having indeed no interest anywhere beyond their own need for liberation from misery and oppression that is both spiritual and physical without distinction, they alone of society's classes can read the Bible directly.

### The Base Community Movement

In the context of this argument, liberation theologians reflect on the so-called Christian base communities that have proliferated throughout Latin America by the tens of thousands. For liberation theologians—to repeat—theology is reflection on historical praxis, on the facts of life as lived; and the praxis that has most attracted their recent attention is the activity of these communities, particularly how they study the Bible.

The origins of the base communities (technically known as *Basic Ecclesial Communities* [CEBs]) is obscure. But the descriptions of them, often idealized, even romanticized, by the liberation theologians (Barreiro 1982; Boff 1985, chap. 9; Cardenal 1976-82; Torres and Eagleson 1981, esp. pt. 2), show extraordinary uniformities for a movement so widespread, uncoordinated, and spontaneous in fact.<sup>5</sup>

Typically, Christian base communities are described as small, face-to-face groups of fifteen to twenty families bonded by physical proximity and poverty. They meet irregularly but frequently, perhaps once or twice a week. The role of priests and other religious professionals in bringing them into existence and organizing their activities appears to have been often important but in styles sharply different from more traditional parish groups. The atmosphere in the communities is said to be across-the-board egalitarian, with the religious professional workers acting simply as catalysts, hardly even as guides. If only because of an extreme shortage of religious professionals in Latin America, their efforts have been focused on getting the base communities to operate on their own.

The most surprising uniformity in the description of them has to do with their functions. These reduce invariably to three:



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## Liberation Theology

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1. *Bible study.* This activity is what brings the communities into existence and keeps them going. Sessions proceed for an hour or so and involve the reading of selected passages and the unrehearsed discussion of them based on personal experience and reflection. These discussions are often combined with equally unrehearsed prayer and some liturgical experimentation, perhaps a communal meal.
2. *Communal action.* The expression of biblical concepts and precepts in communal self-consciousness and shared action is regarded as essential—as praxis. Group projects can vary from assistance to particular members in need to programs of education, health, and entertainment services. They can extend to demonstrations of solidarity before local merchants and landlords and employers. Often these community actions extend to the overtly political but not unless other avenues are blocked. There is a bias against violence, in favor of mass demonstrations and civil disobedience.
3. *Self-consciousness.* The real poverty of the poor is understood in these communities to be a poverty of self-understanding. The poor, oppressed, and depressed must be wrenched out of those denigrating self-understandings society imposes upon them and brought to new understandings of themselves as human beings, as proper members of society, as beings able to love and be loved. They must also be brought to a clear and unobstructed understanding of their actual material condition—that they are oppressed and that this is a scandal that must not be endured. What is called for here is nothing less than a revolutionary transformation in self-understanding, a genuine social, political, economic, and religious self-transcendence.

A fundamental Christian conception in-

forms, throughout, the liberation theologians' understanding of how these functions are discharged. When men and women gather in true congregation of mutually supportive, unselfish love, it is held that the spirit of God will find expression and articulation in and through them. Each will then be enabled to contribute to the group life and solidarity from whatever personal resource and talent ("charism") they may find themselves possessing. The thrust of this understanding is explicitly Pauline: "Now there are varieties of gifts, but the same Spirit; and there are varieties of service, but the same Lord; and there are varieties of working, but it is the same God who inspires them all in every one. To each is given the manifestation of the Spirit for the common good" (1 Cor 12:4-7). This understanding is also pure Rousseau. To a remarkable degree the liberation theologians, in reflecting on what they see happening in the common life of the base communities, have captured, however unwittingly, the exact spirit of Rousseau's famed sentences: "The passage from the state of nature to the civil state produces a very remarkable change in man, by substituting justice for instinct in his conduct, giving his actions the morality they had formerly lacked. Then only, when the voice of duty takes the place of physical impulses and right of appetite, does man, who so far had considered only himself, find that he is forced to act on different principles, and to consult his reason before listening to his inclinations" (1973, 177-78).

In the end, to bring about this "very remarkable change" in the poor, to transform them from self-debasing, purely instinctual clods into self-respecting moral beings able to "build a different society, freer and more human" (Gutierrez 1973, ix), is what the liberation theologians see as the overriding goal of base community life. And it is their unshakable adherence to and understanding of this goal that

most brands them as pre- or perhaps, post-Marxian.

None of this should be held to suggest that the liberation theologians, specifically in their understanding of the base community movement, are non-Marxist, much less anti-Marxist. On the contrary, their understanding of the base communities as revolutionary instruments in a conflictual world, as the first weapons in a class struggle, is shot through not only with Marxian vocabularies but also with Marxian perceptions, especially about the economic character of modern, mass societies. Moreover, there are startling resemblances between the liberation theologians' celebrations of the base communities and the reliance on communes and soviets by Marxist revolutionaries elsewhere in the modern world.

Nevertheless, there is an emphasis in the liberation theologians' understanding of base community life on interior dialectics that has only the palest parallels in Marxist literature. In Rousseau and in the liberation theologians there is a stress that virtually equates the processes of mutual support, reflection, dialogue, group action, self-growth, and social consciousness. For them these processes are the wellsprings of historical movements. Only the poor can help themselves, can transform themselves. In their lives, their dialogues, they are the initiators of social action. In contrast, the Marxist understanding of the essentials of group dynamics—of "politics" in the generic sense—is effectively nonexistent.

In support of these emphases, some liberation theologians (Boff and Boff 1984) make a distinction between liberation and salvation. The latter is taken in the classic Christian sense of a gift through grace of faith and the possibility of love, of freedom from the bonds of sin and the sting of death. Liberation is relief from the miseries and indignities of social existence. But for these liberation theologians, liberation and salvation are inseparable,

the one being meaningless without the other. Liberation, filled with hope, must lead on in faith to salvation. In this these liberation theologians betray not their Marxist enthusiasms but their biblical roots. In their interpretation of that heritage there can be no fast distinctions between body and soul or the conditions of each. For the Hebrews, salvation was both liberation from Egyptian enslavement and purification in the desert, and also entry into a land flowing with milk and honey. In this perspective, there can be no salvation for the soul except as the body is liberated from dehumanizing misery. As Gutierrez writes, "When we assert that man fulfills himself by continuing the work of creation by means of his labor, we are saying that he places himself, by this very fact, within an all-embracing salvific process. To work, to transform this world, is to become a man and to build the human community; it is also to save. Likewise, to struggle against misery and exploitation and to build a just society is already to be part of the saving action" (1973, 159).

### **The Extremes of Biblical Radicalism**

#### **Church Discipline**

Liberation theology has recovered much that is central to biblical radicalism and has revitalized the Christian, progressive message. Through the Christian base community movement, liberation theology has also developed a vehicle for mounting its challenges to traditional hierarchies in practical ways. It thus can become—and, in measure, already has become—theologically, historically, and politically significant.

But there are weaknesses here, too. Mostly they stem from liberation theology's hesitations about taking its nearly exclusive root-biblical orientation to its ultimate conclusions. It is not easy to draw

attention to these hesitations. They are committed by persons engaged directly in situations involving not only extremes of human suffering but also omnipresent, immediate, and vivid threats to human life. Yet major issues of political philosophy as well as theology are at stake.

The problems and difficulties most obviously arise in connection with two all-too-practical issues: church discipline and violence. Both can be briefly stated.

The liberation theologians assert that the poor in their base communities are well positioned to read and interpret Scripture without instruction. This assertion is essential to the whole grand theme of liberation of the poor and specifically to the function of the base communities as arenas for the recovery of self-respect and human dignity by the poor. The unmistakable implication of this assertion is a radical egalitarianism, first existentially, then specifically in roles having to do with interpretations of faith and morals. This egalitarianism is profoundly at odds with the hierarchical structure of the Catholic Church. By virtue of its exalted position, the Vatican is among the *least* well positioned bodies to interpret Scripture. But no liberation theologian has drawn anything like this conclusion. Boff, in his often-cited footnote 17 (1985, 171-72) describing at length—but without commentary—the easy parallels that can be drawn between the organizational patterns of the Roman church and the Communist party in Russia, may be thought to have come closest to doing that. But the chapter to which this footnote is appended appears to uninvolved readers to be mostly a series of casuistries pointing almost meekly in the opposite direction. It is clear that the liberation theologians, as a practical matter, are not prepared to take on the magisterium of their church. Yet the philosophical issue and all that it represents is essentially biblical and is as old as the church itself.

The issue of violence is even more poig-

nant. The biblical materials lend themselves more or less easily to multiple interpretations on the legitimate uses of violence as much as on many other issues. But once a radical interpretive approach has been adopted generally, the pacifism of the New Testament is inescapable. Paul is as emphatic: "Beloved, never avenge yourselves, but leave it to the wrath of God; for it is written, "Vengeance is mine, I will repay, says the Lord." . . . Do not be overcome by evil, but overcome evil with good" (Rom 12:19-21). Yet Fr. Ernesto Cardenal, after witnessing the cowardice, treachery, and unbridled brutality of Somoza's National Guard as it destroyed his base community at Solentiname, joined the revolutionary forces. In human, practical terms, such conduct is understandable, even laudable. But is it congruent with the Gospel's law of love? Is there anywhere a clear line of argument that could lead biblical radicals from their interpretations of the tactics of the cross to the tactics of the gun? Is it enough to claim that not to resist violence with violence is to participate the more in it? What other than pacifism can be a radical interpretation of the commandment that we should love our enemies and pray for them that persecute us?

Again, the issue is both biblical and age-old. Nevertheless, these questions of church discipline and political violence are practical issues that take on greater significance as they lead to primary questions about the fundamental relationships individuals in base communities should have with their neighbors.

### Radical Egalitarianism

Reflecting once more on the egalitarianism that is supposed to pervade the base communities, we can observe that this equality is seen bestowing among members something absolute and unqualified. Anything short of this would demean self-worth, denied to the poor by society at

large but affirmed by the poverty-stricken person's newfound neighbors in the base communities. It is this sense of self-worth that enables the poor person—male or female; black, white, or Indian; old or young; ill or well; skilled or wholly ignorant—to escape societal feelings of oppression and marginalization and assert within the base community a new self that is both loving and lovable, that can with dignity both give and accept membership with others.

The egalitarianism of the base communities is thus a social concept through and through, and as such it bears in train a distinctively social concept of freedom, that most sophisticated sense of freedom that puts men and women in free solidarity with others. Only then do they become truly free and freely human and find a moral liberty in obeying laws they together prescribe to themselves.

The nobility of these conceptions cannot be denied, but, formulated by the liberation theologians as purely social conceptions, they are vulnerable to slippery dissolution into their opposites: equality into hierarchy, freedom into tyranny, and individuality into incipient totalitarianism. For, as Paul saw, any small community—perhaps more than a large impersonal one—will look to its members for different gifts. As these are given, what brake is there on their being proportionately valued—wisdom over brute strength, and brute strength over the capacity to perform mindless, merely tedious chores? Worse, what is to forestall that community in its meditations from transferring its valuations of the different gifts into parallel valuations of the different *givers*? Unless forestalled, hierarchies of differently valued persons will stabilize narrow elites; and these in turn, in the name of efficiency and in the face of ever-present external threats, will increasingly discipline the manner and modes by which lesser folk make contributions to the common welfare. Most important will

be an inevitable tendency to restrict participation by the indecisive, the ill-informed, the untutored, the intellectually lame, the slow and inarticulate in the group discussions and processes of policy formulation. More and more they will become spectators. "Membership," especially for this class, will soon have a hollow ring, as hollow as was that "citizenship" afforded them in the society at large. The very thing that had been most sought and most prized will now have been perhaps irretrievably lost.

### Incommensurable Value of Personhood

The only way this slide into moral and political inversion can be halted is to arrest it before it begins; and the only way to accomplish that is to place on the initial act by which a person becomes a member of the community an *incommensurable* value, that is, a value so great that the value of any subsequent service he or she may perform for the community (or any aggregate of such services) must be seen as less than it, both in the eyes of the community and those of the individual (Replogle n.d., 7).

The group and individual rationales behind this valuation process are different and require separate treatment. But both equally require recognition and both have political implications so extreme that it is no wonder that the liberation theologians, as a practical matter in twentieth century contexts, have drawn back from pursuing them to the end.

On the group side, members of base communities established in the tradition of biblical liberation must come to recognize the special character their communities possess. These communities are uniquely concerned to restore the personhood of each of their members. That function is the essence of their identity as communities. In consequence, to withhold that function from even the least of their members would radically diminish these

communities' quality and identity. There can, therefore, be no compromising, directly or indirectly, practically or theoretically, of any true base community's radical egalitarianism. The political implications of this absolute egalitarianism must now be plain: with the utmost patience, with unbounded concern and respect, base communities conceived of in these terms must collectively wait upon each of their members to come forth spontaneously with whatever contribution to the common welfare they may offer. Any discipline, of whatever kind and however imposed, must be anathema. In a word, these communities must be, in concept as much as in practice, wholly deinstitutionalized. Moreover, the demand for anarchism is across the board. In these communities, bishops and kings, priests and generals, teachers and constables have no standing whatsoever, actually or incipiently.

In twentieth century contexts, even those of third-world nation-states, demands such as these are, by all practical requirements, simply foolish. Yet once explained, the biblical paradigms of the New Testament, in the descriptions of the early church communities given in Acts and the Pauline Epistles, not to mention the Peaceable Kingdom models suggested by the Old Testament in Isaiah, are plain and incontrovertible. Add to this the germane teachings of Jesus himself in the Sermon on the Mount and elsewhere and then ask what other political implications can be drawn from the willful and willing crucifixion of the Messiah.

The valuation of the individual's acquisition of personhood as incommensurable by the individual him- or herself is more complicated than its social recognition but no less extreme in its implications. Like the group, individuals must be brought to recognize the character of the act by which they enter the community, that by it and in it they become persons and, as it were, come of age morally. But

they must also recognize that the acquisition of personhood in community is not a gateway act to be completed by a single entry. It is rather a process to be sustained day by day and day after day. Moreover, it is a process that is as much a completion as it is a commencement. Becoming a social person is not an act of magic. Animals, much less inanimate objects, cannot perform it; even among humans, not every one is able or might want to undertake it. It is a process for which a dense and sustained preparation is necessary. One needs standing, a foundation, from which to become a person and a citizen.

### **Biblical Contribution to the Idea of Personhood**

In the West, as we now understand the full process of citizenship development, many wellsprings can be seen contributing to its definition, most obviously the Greek, in its civic dimensions, and the Roman, in its universal and legal dimensions. But the biblical contribution to it is also essential and perhaps core. For at the core of the Western understanding of personhood is a capacity for existential self-awareness and self-acceptance without which no man or woman is held to be truly whole. That capacity is special to the Hebraic tradition and is central to what the base communities, in their biblicism, explicitly or implicitly attempt to instill in all their members.

It is important to stress that this is an existential self-awareness. It is something quite different from, and perhaps also deeper than, Greek philosophical talk about an unexamined life not being worth living. It is that distinctively Hebraic talk that says in prayerful confrontation with mere other, "Here I am!" It is the language of Job, alone, abandoned, and—if not distinctively presocial—at least, in the moments of his ultimate despair, utterly asocial. Existential self-awareness when

driven to its root—as Job was by the end of his book—is a quest for standing in nothing but the time and space of mere existence, bare to the bone. Optimistically, the biblical tradition before Job, in Job, and ever more emphatically after Job, declares that standing in pure existence can be found, that through prayer and confrontation the mere self can ground itself in a stability before mystery. The task is not easy and the search for its completion is never fully done. But it is this stability of the soul, so far as it may be had, upon which the liberation theologians could explicitly ground the sense of citizenship to be imbued by the base communities.

What must be finally stressed is that this existential egalitarianism, however difficult its attainment may be, is available to every person. Jeremiah's prophecy of the new covenant makes this plain:

In those days they shall no longer say:

"The fathers have eaten sour grapes,  
and the children's teeth are set on edge."

But every one shall die for his own sin; each man who eats sour grapes, his teeth shall be set on edge.

"Behold, the days are coming, says the LORD, when I will make a new covenant with the house of Israel and the house of Judah. . . . I will put my law within them, and I will write it upon their hearts; and I will be their God, and they shall be my people. And no longer shall each man teach his neighbor and each his brother, saying, 'Know the LORD,' for they shall all know me, from the least of them to the greatest, says the LORD; for I will forgive their iniquity, and I will remember their sin no more." (Jer 31:29-34)

But this is no more than a natural conclusion. The confrontation with bare life is the *province of any life*.

### Political Implications of the New Covenant

Beyond all these theological points lie the inescapable political implications of the merely existential Jobean paradigm

and the New Covenant. In the light of these, biblical political radicalism in its fullness and in all of its dimensions is simply unavoidable. Its propositions now go further than the transcendent reenforcement of the absolute egalitarianism of base communities and any other similarly constituted groups. They certainly do that much and powerfully so. This kind of positively asserted, absolutely existential egalitarianism prompted Gerard Winstanley—to name one prominent and especially germane example—to refuse to remove his hat when ushered into the presence of General Fairfax of Cromwell's command, "because he was but a fellow creature" (Sabine 1941, 15). But even beyond these matters, as in Winstanley's own larger vision, absolutely radical existential self-respect must demand in all economic and social organizations a proportionately radical, utopian socialism, an absolute communism. Before existentialisms of this order, pretensions of property—of "This is mine; that is yours"—are as inadmissible as any pretensions of personal pride. In this context, the words of Acts 2:43-46 become a clarion call: "And fear came upon every soul; and many wonders and signs were done through the apostles. And all who believed were together and had all things in common; and they sold their possessions and goods and distributed them to all, as any had need. And day by day, attending the temple together and breaking bread in their homes, they partook of food with glad and generous hearts." Still more wonderful is the sublime belief that all this utopian communism can be practiced and will come to pass in the community at large *pacifically*. True, the saints must yearn for it and work for it and put their bodies and possessions on the line for it, as Winstanley and his followers, pathetically, did. But it *will* come *pacifically*: so real Christians in the biblical tradition must believe.

This kind of political thinking, to all the

practical requirements of our twentieth century times, is foolishness; and it is no wonder that liberation theologians draw back from it. Unanimously they call for socialism, but consistently (coily?) they are unclear and unspecific about the kinds and degrees of socialism they see as required for the future of Latin America. Winstanley's fate would appear to be instructive. On the other hand, the disruptive, explosive potential of his radicalism—rooted in the bible and now implicit in many of the not fully explored assertions of the liberation theologians and their emergent church—remains.

### Notes

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1. E. J. Dione, Jr., "Vatican Briefs Brazilians on New Decree," *New York Times*, 16 March 1986; Stephen Kinzer, "War's Sad Toll: The Divided House of Nicaragua," *New York Times*, 22 May 1986.

2. The generalizations of this and the following two paragraphs apply broadly across both the Old and New Testaments. But for specific support consult especially the story of Abraham's attempted sacrifice of Isaac (Gn 22), the Exodus narratives, especially the Covenant ceremony in Exodus 24, the Covenant renewal ceremony in Joshua 24, the Saul and David stories (1 Sm 13-31), the Elijah cycle (1 Kgs 17-22), any of Jeremiah's confessions (11:18-12:6, etc.), and the narratives of Jesus' ministry, especially in Mark. Consult Pedersen 1926, pts. 1-2, especially pp. 99-181; and Boman 1960, especially pp. 27-48, 137-53.

3. Biblical quotations are taken from *The Oxford Annotated Bible* (RSV), ed. Herbert G. May and Bruce M. Metzger (New York: Oxford University Press, 1962).

4. Consult Yoder 1972, for comparable reinterpretations of the phrase *the acceptable year of the Lord* in Luke 4:19, and of Paul's injunction to obey the powers that be in Romans 13.

5. There is a small but growing body of literature on the base communities by scholars having fewer overt commitments than the liberation theologians (see Bruneau 1982; Cleary 1985, esp. chap. 4; Levine 1979). This literature largely corroborates the liberation theologians' description as an "ideal type," but goes on to emphasize the diversity of style, size, and

purpose among the CEBs, and also the widespread existence of competing types, especially the charismatic, pentecostal type, whose outlooks are sharply conservative and otherworldly. Also emphasized are organizational weaknesses of the classic ("post-Medellin") CEBs, especially in leadership development and coordination.

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# ROUSSEAU'S GENERAL WILL: A CONDORCETIAN PERSPECTIVE

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**W**e identify three basic elements of Rousseau's theory of the general will: (1) there is a common good; (2) citizens are not always accurate in their judgments about what is in the common good; and (3) when citizens strive to identify the common good and vote in accordance with their perceptions of it, the vote of the Assembly of the People can be taken to be the most reliable means for ascertaining the common good. We then show that Condorcet's (1785) model of collective judgment shares these assumptions with Rousseau and that understanding the implications of Condorcet's (1785) "jury theorem" enables us to clarify many of the most obscure aspects of Rousseau's treatment of the general will, including his discussion of the debilitating effects of factions and his confidence in the ability of the Assembly of the People to discern the general will by means of voting.

**R**ousseau's seminal contributions to democratic theory are his views on the development of the social contract and his notion of the "general will." Although the "general will" has been given various interpretations, there has been little understanding of how, in practice, political institutions might ascertain the general will for the purpose of effectuating public policy.

We illuminate the logic underlying Rousseau's notion of the general will by making use of long-neglected ideas of Rousseau's contemporary, Condorcet, especially those about the judgmental competence of individuals and groups. We also present some new results about the linkages between (individual and collective) preferences and (individual and collective) judgments about the nature of the public good. In the process, we show how

some of the most obscure passages in Rousseau can be clarified by referring to results about features of majority rule first demonstrated by Condorcet some decades after *Of the Social Contract* was published. In particular, we examine the relationship between the general will and the will of all, the likelihood that the general will will err, and the subordination of individual opinions to the collective judgment. Our aim is to understand how collective decision-making processes may be appropriately used to ascertain the general will.

Almost no scholars dealing with Rousseau mention Condorcet (for important exceptions see Barry 1964, 1965, and Baker 1980), and even those who mention him customarily cite not his 1785 essay on voting but other works dealing with quite different topics (see, e.g., Ellenburg 1976,

46, 84, 85). A leading interpretation of Rousseau's general will, made famous by Runciman and Sen (1965), interprets it in the context of a prisoner's dilemma game, and treats it as a problem of reconciling conflicting individual preferences rather than as a problem of developing a reliable judgment of what is in the collective interest. We view our approach, with its focus on social judgments, as complementary to that of Runciman and Sen, and providing a needed corrective to a current focus, in social choice theory, on treating all value questions in democratic theory as if they could be reduced to some aspect of the problem of aggregation of preferences.

### Rousseau's General Will

The general will can only direct the forces of the State in keeping with the end for which it was instituted, which is the common good; for if the opposition of private interests has made the establishment of societies necessary, the harmony of these same interests has made it possible. That which is common to these different interests forms the social bond; and if there were not some point in which all interests agree, no society could exist. Now it is only on this common interest that the society should be governed. (Rousseau 1984, 66 [3.1])

In this quote from Book 3 of *Of the Social Contract*, Rousseau recognizes that people differ in their interests but asserts that there is a common (or public) interest on which all humankind can agree in principle—even though not all would wish to pursue it: "Indeed, each individual may, as a man, have a particular will contrary to, or divergent from, the general will which he has as a citizen. His private interest may speak to him quite differently from the common interest; his absolute and naturally independent existence may make him regard what he owes to the common cause as a gratuitous contribution, the loss of which will be less harmful

to others than will the payment of it be onerous to him" (p. 53 [1.7]).

How is the general will to be ascertained? Rousseau's answer is, by voting: "The voice of the greater number always obliges all the others" (p. 328 [4.2]). However, the vote is not to be an aggregation of self-interested preferences. Rather, "when a law is proposed in the Assembly of the People, what is asked of them is not precisely whether they approve the proposition or reject it; but whether or not it conforms to the general will which is their own: each in giving his vote states his opinion on that question, and from the counting of the voting is taken the declaration of the general will" (p. 329 [4.2]).

This passage in Rousseau is often misunderstood. It represents an understanding of the process of voting not as a means of combining divergent interests but rather as a process that searches for "truth." Somewhere in the course of the development of capitalism, what we may think of as an essentially religious idea of voting—a search for God's mandate as revealed through man's finite cognitions (still present in some contemporary religious groups, e.g., the Bruderhof Commune [Zablocki 1971]) was replaced with a much more individual interest-based notion (Riley 1986).<sup>1</sup> Contemporary social welfare economics, beginning with Arrow's work (1963), has focused entirely on voting as a means of preference aggregation. The notion of voting as a process that can be thought of as a direct search for the common good—indeed, the very notion that there can be a common good that is something other than some form of summation or reconciliation of the preferences of individuals—has been lost.

There is another important implication of this quote about the nature of voting in the Assembly of the People, namely, that even those individuals whose vote is based on their perception of the common

good may err in that perception:<sup>2</sup> "When the opinion contrary to mine prevails, that only proves that I was mistaken, and that what I had considered to be the general will was not" (Rousseau 1984, 329 [4.2]).

Because the voters who seek the general will are fallible in their judgments, the collective judgment can also sometimes be wrong: "The general will is always upright and always tends toward the public utility, but it does not follow that the deliberations of the people always have the same rectitude. One wishes always his own good but does not always discern it. The people is never corrupted, though often deceived, and then only does it seem to will that which is bad" (p. 75 [2.3]).<sup>3</sup>

Nonetheless, Rousseau expects the vote of the popular assembly (i.e., its "declaration of the general will") to coincide with the general will under reasonable conditions: "If, when an adequately informed people deliberate, the citizens having no communication among themselves, . . . the general will would always result" (p. 75 [2.3]).

We should also note that Rousseau, in the passage quoted directly above, sees the "deliberative process" as one taking place within individuals rather than in terms of a process of group debate. Thus, each voter is seen as seeking to reach an individual and independent judgment about alternatives.

There are three elements of Rousseau's theory of the general will that we wish to single out:

1. There is a common good.<sup>4</sup>
2. Citizens are not always accurate in their judgments about what is in the common good.<sup>5</sup>
3. When citizens strive to identify this common good and vote in accordance with their perceptions of it, the vote of the Assembly of the People can be taken to be the most reliable means for ascertaining the common good.<sup>6</sup>

### Condorcet's Jury Theorem as a Formalization of Rousseau's General Will

Rousseau has long been acknowledged as one of the great political philosophers. In contrast, Rousseau's contemporary, Condorcet, languished long in obscurity until his idea of the "paradox of cyclical majorities" was rediscovered by Black (1950, 1958) and helped lay the foundation for modern social choice theory (see, e.g., Arrow 1963; Farquharson 1969; Plott 1976; Riker 1964; Sen 1966; and a host of others). However, the idea for which Condorcet is now most famous, the paradox of cyclical majorities, was actually only an incidental by-product of the problem on which he was working, which was the problem of ascertaining how groups could best make choices that were collectively optimal (Black 1958; Grofman, Owen, and Feld 1983; Pinkham and Urken 1982; Young 1986).

Two hundred years ago Condorcet (1785) recognized that majorities of individuals are likely to be more often correct than individuals. Whether understood by the participants or not, this is one fact that makes democracy "work." Condorcet's result, however, was lost for most of the next two hundred years (Black 1958) and even today is nowhere near as well known as it deserves to be (Barry 1965; Grofman 1975; Grofman and Owen 1986a, 1986b; Miller 1986). The Condorcet jury theorem (Black 1958; Condorcet 1785; Grofman 1975) says that if each individual is somewhat more likely than not to make the "better" choice between some pair of alternatives (along some specified evaluative dimension) and each individual has the same probability of being correct in this choice, then (with each voter voting independently) the probability of the group majority being correct increases as the number of individuals increases, towards a limiting

value of 1. Moreover, even if individuals have varying competence—where by *competence* we mean the individual probabilities of making the “correct” (dichotomous) choice (i.e., the choice that has the higher value along the specified evaluative dimension)—then so long as the *average* competence is greater than .5, the probability of the group majority being correct still increases to 1 as the group gets large (see Grofman, Owen, and Feld 1983).<sup>7</sup>

We can provide a statement of Condorcet’s jury theorem in a form in which its resemblance to Rousseau’s theory of the “general will” will be readily apparent. We assume that

1. There is a common good and a set of alternatives that more or less share in its virtues. Thus, alternatives can in principle be evaluated with respect to the underlying normative dimension of consonance with the public interest (general will), and this evaluative dimension permits us, in principle, to rank-order alternatives.
2. With respect to choice between any pair of alternatives, each citizen  $i$  has a probability  $p_i$  ( $0 \leq p_i \leq 1$ ) of choosing that alternative which is more in the public interest (closer to the general will).
3. A group of size  $N$  chooses between any two alternatives by means of a majority vote in which each voter is polled about his or her independently reached choice, without any group deliberation.

Though Rousseau was not at all a formal mathematical thinker, and despite the fact that some of the basic probabilistic ideas needed to make sense of the Condorcet jury theorem were still in a very preliminary stage of development in the 1750s, we believe that the Condorcet jury theorem accurately captures the basic ideas underlying Rousseau’s notion of the

general will. It seems virtually certain that ideas similar to those later to be formally developed by Condorcet were “in the wind,” and influenced both Rousseau and, later, Condorcet; and there certainly were various social and intellectual linkages between Rousseau and Condorcet. One connection is via the *Encyclopédie* project, in which Diderot and D’Alembert were involved. Condorcet became a close friend of the mathematician and philosopher D’Alembert, whose notion of the pursuit of the common good anticipated that of Condorcet; Rousseau, as a close friend of Diderot, may have learned of these ideas.<sup>8</sup>

There are five key points in understanding the relationship between the ideas of Rousseau and those of Condorcet: *First*, the Condorcet jury theorem is based on a notion of common judgment, not separate individual preferences. It thus permits us to understand better how Rousseau’s distinction between the general will and the “will of all” can be implemented in a voting assembly. In Rousseau’s own language, “There is often a great difference between the will of all and the general will. The latter regards only the common interest; the other regards private interests and is only the sum of particular wills.”

*Second*, the Condorcet jury theorem permits us to understand how the majority can be a representation of the general will (when its members act in judgment of the common good and not in terms of their particularized self-interests) without the majority will being *identical* to the general will. The Condorcet jury theorem states the “limit result” that as the group size grows large, if the average citizen is more likely than not to judge correctly which of any pair of alternatives is more nearly in the public interest, the majority vote of the group will be *almost certain to be correct* in its judgment of the public interest.<sup>9</sup>

Nonetheless, the general will may err, or—in Rousseau’s terminology—“the

characteristics" of the general will "may not reside in the majority" (1984, 102 [4.3]). However, even for average group competence  $\bar{p}$  near .5, the expected judgmental accuracy of large assemblies is considerable.<sup>10</sup> For example, even if  $\bar{p}$  is only .51, a 399-member assembly has a competence of .66, while if  $\bar{p} = .55$ , a 399-member assembly has a competence of .98. For a reasonable level of  $\bar{p}$  (e.g.,  $\bar{p} = .6$ ), even relatively small assemblies (of size greater than 41) have a group competence level  $P_N$  above .9 (see Grofman 1975; Miller 1986). For  $p = .7$ , an assembly of only size 11 will have a group competence level of above .9.

Third, knowledge of the Condorcet jury theorem (and recent extensions, e.g., Miller 1986; Owen 1986) helps us better understand the logic undergirding another somewhat puzzling passage in Rousseau:

But when factions are formed, partial associations at the expense of the whole, the will of each of these associations becomes general with regard to its members and particular with regard to the state: one is then able to say that there are no longer as many voters as there are men, but only as many as there are associations . . . and [this] yield[s] a *less general result*. Finally, when one of these associations is so large that it overcomes the rest, . . . then there no longer is a general will, and the opinion which dominates is only a private opinion. (1984, 27 [2.3], emphasis ours)

We translate this remark of Rousseau in Condorcetian terms as an observation about factions reducing the *effective* size of the assembly. As the *effective* size of the assembly is reduced—because people vote as a herd (part of a faction) not as separately thinking and independently acting individuals—the Condorcet jury theorem tells us, group accuracy will be reduced. Indeed, at the extreme, if there is a majority faction, this faction is equivalent to a single voice deciding things; thus the benefits of large numbers are lost completely.<sup>11</sup> More generally, if individual choices are positively correlated with one another beyond the correlation to be ex-

pected from similarities in competence alone, group accuracy will be reduced (Owen 1986; Shapley and Grofman 1984).

Fourth, a focus on the judgmental basis of voting allows us to provide a mathematical foundation for Rousseau's observation that "the closer opinions approach unanimity, the more dominant is the general will" (1984, 322 [4.2]).

Using the Condorcetian probability framework, it can be shown that the more votes there are on the majority side, the more likely is the group majority to be correct.<sup>12</sup>

Fifth, a probabilistic approach to group judgment allows us better to understand Rousseau's views as to when supermajoritarian decision rules are called for: "The more important and serious the deliberations, the closer the prevailing opinion should approach unanimity; [on the other hand,] in decisions that must be resolved immediately, a majority of one vote should suffice" (p. 103 [4.2]).

If certain kinds of decisions are more subject to error than others (or are simply more important than others so that we wish to have a higher level of confidence that the group vote is an accurate expression of the general will), we might wish to require more than a bare majority vote, since this will reduce the error level since it can be shown that the more votes there are in favor, the more likely is the group judgment to be correct (Grofman 1978; Nitzan and Paroush 1985; cf. Buchanan and Tullock 1962; Rae 1969).<sup>13</sup>

Our emphasis has been on how individual judgments about what is in the public interest aggregate to indicate the general will. We believe that this approach captures the central notion of Rousseau's concept of the general will. However, Rousseau recognized that individuals were not always so nobly motivated and that they sometimes expressed their personal preferences, rather than seeking the general will (see n. 5). He also noted that the general will could some-

times emerge as the residue from the canceling out of individual self-interest in the process of aggregation: "The [general will] looks only to the common interest. The [will of all] looks only to private interest and is only the sum of particular wills: but take away from these same wills the pluses and minuses which cancel each other out and the general will remains as the sum of the differences" (1984, 76 [2.3]).

While this language has proved incomprehensible or nonsensical to some (e.g., Plamenatz quoted in Gildin 1983, p. 55), Gildin (1983, 55-57) provides a simple illustration of what Rousseau almost certainly meant. In Gildin's illustration of a Common's Dilemma, each fisherman would like to fish above the limit set by long-run social advantage, since, *ceteris paribus*, the few fish netted will not be sufficient to drive the fish population to extinction; yet if each fisherman "cheats," all will suffer (cf. Goodin 1982). Clearly, each fisherman wishes the rule to be one where *all other* fishermen must obey the limit. If we subtract out these egocentric peculiarities, the "common" preference is for a ban on fishing above the socially optimal limit for *all* fishermen (see Gildin 1983; cf. Runciman and Sen 1965).

However, as is clear from many passages from him, Rousseau believed that the most certain route to finding the general will was one in which individuals were primarily oriented toward the general will rather than to their own narrow self-interest,

## Conclusions

We hope that our reconstruction of Rousseau's theory can lead to a broader understanding of democracy as a means to collective ends, rather than as just as a means for aggregating narrow interests residing in, and confined to, individuals. While it is often assumed that democracy

should be based upon individuals following their own self-interests, Rousseau's and Condorcet's contributions suggest that democracy "works" better when individuals try to see beyond their narrow self-interests to the collective good. Democracy may require a certain amount of shared collective consciousness to achieve competent collective judgments; consequently, polities that lack such consciousness may not function well as democracies.<sup>14</sup>

Even while many politicians and researchers recognize that voters often vote to further the collective rather than their individual interests, other researchers and theorists (especially those working in a social choice framework) tend to overlook the importance of these collective orientations. Politicians obviously recognize that appeals to "right," "good," and "fair" policies have some political appeal. Similarly, empirical research consistently shows that norms of citizen duty are one of the main determinants of whether citizens vote at all. However, theorists of democracy may fail to recognize that even relatively small amounts of collective orientation (i.e., only slightly more than none), especially in the context of negatively correlated individual interests, can aggregate to collective decisions with a high probability of serving the public good. That small differences in each individual's amount of collective orientation can make large differences in the likely ability of the electorate as a whole to make collectively beneficial judgments is one of the clear implications of the Condorcet jury theorem. Thus, it may not matter that individuals are not very "sociotropic" in their voting, as long as there are *some* elements of sociotropic voting present in a significant number of voters (cf. Miller 1986).<sup>15</sup>

We see Rousseau as a propounder of enlightened democracy, not dehumanized collectivism.<sup>16</sup> For Rousseau, as for Condorcet, the process of voting is a means

## Rousseau's General Will

whereby the common good can be identified and implemented, albeit imperfectly.<sup>17</sup> In voting, however, social judgments, not individual preferences, are to be the basis of voter choice.<sup>18</sup> Democracy as a process for making good decisions based upon the aggregation of individual judgments has only recently begun to receive the attention that it deserves (Grofman and Owen 1986a, 1986b; Miller 1986; Nitzan and Paroush 1985; Pinkham and Urken 1982; Young 1986).<sup>19</sup> We hope our reinterpretation of Rousseau's views may encourage others to pursue the topic of collective judgments in terms of both descriptive and normative theory of democratic behavior.<sup>20</sup>

### Notes

This research was begun while Grofman was a fellow at the Center for Advanced Study in the Behavioral Sciences, Stanford. We are indebted to the staff of the Word Processing Center, School of Social Sciences, UCI for typing and table preparation and to Dorothy Gormick for bibliographic assistance.

1. Riley (1986, ix) characterizes his book as "a study of the transformation of a theological idea, the general will of God to save all men, into a political one, the general will of the citizen to place common good of the city above his particular will as a private self, and thereby to 'save the polity.'"

2. Compare this idea with that of the "impartial spectator" that, according to Adam Smith in his *Theory of Moral Sentiments* (1971, 171) is contained within each of us and would be "forced" to agree on what is right. Such intuitionist notions of moral judgment are common in the seventeenth and eighteenth centuries.

3. We have omitted the continuation of this quote, which indicates what happens when the assembly is divided into factions. We will return to this problem later.

4. "If there were not some point in which all interests agree, no society could exist" (Rousseau 1984, 66 [2.1]).

5. Moreover, individuals may falter in their allegiance to the common good over self-interest: "If it is not impossible that a private will will agree at some point with the general, it is at least impossible that this agreement should be lasting and constant; for the private will naturally tends to preferences, and the general will to equality" (Rousseau 1984, 68 [2.1]).

6. Thus, from the vote of the Assembly "is taken the declaration of the general will" (Rousseau 1984, 329 [4.2]).

7. Let  $P_N$  be the majority judgmental accuracy of a group of size  $N$  ( $m = (N + 1)/2$ , if we assume, for convenience,  $N$  odd), i.e., let  $P_N$  be the probability that the group majority will, in a pairwise comparison, pick the alternative that is better with respect to the common interest. Let  $\bar{p}$  be the average accuracy level of voters. Then, if voter choices are mutually independent,

$$P_N \approx \sum_{h=m}^N \binom{N}{h} (\bar{p})^h (1 - \bar{p})^{N-h}$$

and if  $\bar{p} > .5$ ,

$$\lim_{N \rightarrow \infty} P_N \rightarrow 1.$$

If  $\bar{p} < .5$ ,

$$\lim_{N \rightarrow \infty} P_N \rightarrow 0,$$

while if  $\bar{p} = .5$ , then, not so intuitively,

$$1 - e^{1/2} < \lim_{N \rightarrow \infty} P_N < e^{-1/2}$$

(Grofman, Owen, and Feld 1983).

8. In his *Encyclopédie* essays on natural rights and on the ancient Greeks, Diderot himself deals with the conflict between individual wills and the general will (see Riley 1986, 203-05). We are not, however, aware of any mention of Rousseau by Condorcet in his 1785 essay.

9. Grofman (1975) characterizes this aspect of the Condorcet jury theorem as "vox populi, vox dei," i.e., the voice of the people approaches infallibility.

10. The calculations below are based on a normal approximation to the situation in which all group members have identical accuracy levels. However, for  $m > 10$  or so, distributional effects of competence are minimal, and the results given above may be used even for extreme cases (e.g., ones where some members of the group have  $p_i = 0$  or 1). Grofman, Owen, and Feld (1983) and Grofman, Feld, and Owen (1982) give precise bounds.

11. See n. 10.

12. To look at the differing competences of group judgments with differing margins of votes, we make use of the formula for the ratio of the probability of the correct choice by a group majority of size  $m + k$  to the probability of an incorrect choice by a group majority of that same size, where  $m$  is a simple majority ( $= (N + 1)/2$  if  $N$  odd), and  $\bar{p}$  is mean group competence. We have

$$\frac{(\bar{p})^{m+k} (1 - \bar{p})^{N-m-k}}{(\bar{p})^{N-m-k} (1 - \bar{p})^{m+k}} = \frac{\bar{p}^{(2m+2k-N)}}{(1 - \bar{p})^{N-2m-2k}}$$

It is apparent that the above expression increases with  $k$ .

It should be clear that no single will can ever be expected to divine the general will reliably (i.e., more formally, we would not expect any  $p_i$  to equal 1). Furthermore it is likely that any long-lasting faction, even if a majority, will turn away from a search for the common good to a concern for the private good of its own members (see n. 5; cf. *Federalist Papers*, no. 10).

13. If we insist on a supermajority, of course, then we must risk deadlock; e.g., if juries require unanimity, this opens the possibility of hung juries (see Grofman 1979, 1981; cf. Buchanan and Tullock 1962).

14. Thomas Schwartz (personal communication, March 1985) has proposed that "Rousseau and other radical democrats want to have a society without politics." In one sense, this is correct; but we prefer to stress the way in which, for Rousseau, politics in effect becomes redefined as the search for the general will.

15. Compare the view of Tocqueville: "Not only is common opinion the only guide which private judgment retains among a democratic people, but amongst such a people it possesses a power infinitely beyond what it has elsewhere. At periods of equality men have no faith in one another, by reason of their common resemblance; but this very resemblance gives them almost unbounded confidence in the judgment of the public; for it would not seem probable, as they are all endowed with equal means of judging, but that the greater truth should go with the greater number" (1945, 2:11).

16. Some of the same language we have cited to show how the majority will can become the general will if citizens strive to identify the common good and vote in accordance with their perceptions of it, other authors interpret to mean that Rousseau takes the position that individuals do not count, only society matters. For example, Peter Drucker contends that Rousseau believes that

whatever human existence there is; whatever freedom, rights and duties the individual has; whatever meaning there is in individual life—all is determined by society according to society's objective need of survival. The individual, in other words, is not autonomous. He is determined by society. He is free only in matters that do not matter. He has rights only because society concedes them. He has a will only if he wills what society needs. His life has meaning only insofar as it relates to the social meaning and as it fulfills itself in fulfilling the objective goal of society. There is, in short, no human existence, there is only social existence. There is no individual, there is only the citizen. (1971, 51).

This seems far too extreme a reading of Rousseau. Certainly, the language in Rousseau about the

"general will" is susceptible to a much more straightforward interpretation. Rousseau merely sees a public interest that is the proper concern of citizens acting in the legislative arena but does not make the extravagant claim that the legislative arena is the sole, or even the superordinate, arena (see Riley 1986, 248–50; cf. Cobban 1964).

17. The clause "albeit imperfectly" is a critical one. It is sometimes claimed that Arrow's theorem demonstrates the inherent impossibility of there being such a thing as a general will: "The theorem provides an unambiguous answer to the question 'Is there a foolproof way to derive complete and transitive social preference relations?' The answer is no. This clearly negative result casts doubts on all assertions that there is a 'general will,' a 'social contract,' a 'social good,' a 'will of the people,' a 'people's government,' a 'people's voice,' a 'social benefit,' and so forth" (Feldman 1980, 191). We believe this (common) view of what Arrow's theorem allegedly demonstrates about democratic theory is simply wrong. Our position, like Rousseau's, is that the general will may exist but that the outcome of any voting process is but an imperfect reflection of it.

18. As previously noted, we believe that the Condorcetian perspective on social judgments provides a useful corrective to the standard emphasis of economists on social welfare as the aggregation of *individual preferences* (cf. Dummett 1984, 170; Grofman, Owen, and Feld 1983; Lehrer and Wagner 1981; Margolis 1982, 66–69; Nurmi 1984). We also believe that the educative role of politicians in the political process must also be acknowledged (cf. Kelly 1987; Shklar 1969, 186–87). Similarly, accounts of political actors as vote maximizers neglect the role of politicians as innovators of organizational solutions to common problems (Glazer and McMillan 1987).

19. As we have seen, Condorcet, who was concerned with the search for the public good, is remembered only for his analysis of the effects of combining individual *preferences* in such a fashion as to give rise to intransitivities (the paradox of cyclical majorities).

20. One such application might be the analysis of judicial decision making. Clearly, Supreme Court justices are not supposed to be reconciling competing personal preferences for policy when they vote; rather they are seeking to arrive at a judgment of what the Constitution—or some statute—means (in the given context). Thus, to use terminology from early U.S. (and Continental) political thought, Supreme Court justices are to exercise "judgment," not "will" (Chamberlin n.d.). Whether such distinction is meaningful and, even if meaningful, whether human beings can be expected to restrain their will and exercise only their judgment are questions relevant to the present debate over the proper role of evaluation of judicial philosophies as a factor in shaping Senate nonconfirmation of U.S. Supreme Court justices such as Robert Bork.



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## CONTROVERSIES

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## RATIONALISM OR REVELATION?

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*Are there appropriate limits to the application of rational choice in political decision making? Does rationalism in politics lead to absolutism? Is there a "pressing threat" to liberal democracy "posed by the irreverent conviction of the hegemony of reason"? In the June 1987 issue of this Review, Darrell Dobbs drew lessons from Homer's epic poem, the *Odyssey*, to argue the limits of rationalism in politics. In this Controversy, Robert Grafstein argues that Dobbs's case against rationalism is not proved. In turn, Dobbs holds to his construction of the relevance of *Odysseus'* nod to sacred values.*

According to Dobbs (1987), Homer's *Odyssey* offers us a valuable lesson about the limits of reason and, more specifically, about the limits of rational choice approaches in politics and political science. The lesson apparently extends to both rationally guided action and belief. These limits, moreover, are avoidable. Once, Dobbs argues, we recognize matters inaccessible to reason, the sacred, and recognize non-rationalistic means to apprehend them, revelation, we will be able to recognize with Homer the degree to which many of our intellectual and political problems are self-imposed.

These are very important conclusions and if true would have far-reaching implications for political science. Rather than argue that they are false, which I think they are, I will argue that Dobbs has not presented a persuasive case for them. Specifically, his argument is based on a misunderstanding of decision theory and rational choice. The premises of his negative argument are thus faulty. Furthermore, even if the verdict against rationalism is nonetheless true, this does not by itself justify the endorsement of revelation as a higher or supplementary means to wisdom.

### Rationality and Dominance

The crew on *Odysseus'* boat decides to eat the sacred cows, reasoning by the rational choice principle of dominance that regardless of what the gods do, the consequence of slaughtering is always better than the consequence of refraining. Homer condemns the crew's decision as reckless, implying, according to Dobbs, that the sacred must be respected even if doing so is irrational. Rationality has reached its limits.

Decision theory, however, does not recommend the principle of dominance in this kind of case (see, e.g., Levi 1980, 107). For this principle to apply, the alternative states of nature must be independent (causally or statistically, depending on the decision theorist) of the act chosen by the crew. It is difficult to imagine that the decision of the gods to cooperate and punish is independent of the crew's decision to do something punishable. Dobbs (1987, 507) concedes that expected utility considerations may indeed conflict with the dominance principle. One of his responses is that such conflict is "rare." Yet its rarity is not obvious; and even if such cases are rare, the story of the slaughtering of the cattle may still be one;

and even if rare, such cases may still be important. Jeffrey (1983, 2, 8-9), for example, illustrates the relevance of the conflict to arguments over nuclear deterrence and disarmament.

Dobbs believes, in addition, that it would be reasonable to construe the crew's decision problem so as to meet the independence requirement and thus restore the relevance of the crucial dominance principle. He cites textual evidence that, in translation, is unclear to this non-expert. More strongly, Dobbs suggests that even if there are problem cases, they can be reformulated to exhibit the requisite independence. His illustrative case is Newcomb's Problem. While this problem has political analogs (see Brams 1976, 197-212; Frydman, O'Driscoll, and Schotter 1982) I will briefly describe it in its original form. You are presented with two boxes and a choice between the contents of the first box or both. In the second box there is one thousand dollars. As for the first, which is opaque, there is someone or something that puts one million dollars in it if that being predicts you will choose only the first box; it puts in nothing if it predicts you will choose both. The being, by the way, is an excellent predictor. In more than one million trials, it has been correct 90% of the time. What is your choice, given the apparent conflict between the principle of dominance (choose both boxes) and expected utility (choose the first box)?

Following Brams (1976, 200-203)—who in turn follows a suggestion by John Ferejohn—Dobbs (1987, 506) believes the problem can be solved "by recasting its gaming elements into independent decisions." Specifically, interpret the two states of nature not in terms of whether the being put the money in or not but as *being is correct* and *being is incorrect*. Note, however, that as a result of this reformulation there is no longer any dominant choice; so the implications for Odysseus' crew are unclear. In any case

there are at least two problems with this kind of proposal.

First, correctness and incorrectness are not so much states of nature as characterizations of states of nature; and work by logicians such as Alfred Tarski suggests that mixing the two can be perilous (see Grafstein 1983). Second, conflict between dominance and expected utility may be obviated in this particular case, but I do not see that independence—which allows us to apply dominance when relevant—has been secured. Suppose the being has been correct 900 thousand times when one box was chosen, correct ninety times when two boxes were chosen, incorrect ten times when one box was chosen, and incorrect 100 thousand times when two boxes were chosen (this example is inspired by Levi 1975). The being has been correct 90% of the time yet the states of nature are not statistically independent of the choices. Nor, it seems, is the being's correctness causally independent of what the decision maker does.

One can in fact always reformulate decision problems to ensure causal independence, although, as Newcomb's Problem illustrates, one cannot always additionally guarantee statistical independence (see Luce and Krantz 1971). To complicate matters further, there are incorrect ways to secure causal independence, at least in the eyes of those focusing on this form of independence (see Skyrms 1980, 128-39). What rationalism recommends for the crew, it turns out, transcends any reflexive reliance on dominance.

### Representation of the Problem

Since the dominated choice of the crew—respect the cows—leads to the same consequence regardless of the state of nature, one might still argue that by coincidence the dominance principle produces the correct rational choice with or

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without independence: no juggling of probabilities will make the dominated choice preferable. This objection, while correct, simply highlights another problem—Dobbs's representation of the crew's decision problem.

To see this, note that the crux of Dobbs's argument is that dominance would be correct (as in the Penelope example he cites) were it not for the role of the gods, their commands, and the sacred. But it is not rational choice theory that ignores these additional elements. Rather, the fault lies with Dobbs's depiction of the choice situation: the crucial differences between the Penelope and crew examples do not receive appropriate recognition in his characterization of the consequences for the crew, which are described by Dobbs in purely secular terms. And note that the crew's preferences and choices, as Dobbs sets up the decision problem, range over these possible consequences, not over the states of nature themselves.

If, in short, there is more to killing the cows than the possibility of a quick death, namely the violation of the sacred, that fact ought to be reflected in the consequences the crew considers. Their recklessness does not lie in their choosing rationally but in their accepting Eurylochos' characterization of their decision problem, which Dobbs reinforces. Given this characterization, the crew could reveal their preference but not their reverence.

### **Commensurability**

The preceding diagnosis is wrong-headed, Dobbs would argue, insofar as it still does not confront the fundamental failure of rationalism, its insistence that everything, sacred and profane, is commensurable: "Whatever their other differences, ancients and modern alike acknowledge commensuration as the characteristic *modus operandi* of reason" (1987,

506). If this is true of the ancients, so much the worse for them. It decidedly misrepresents modern decision theory. In the marketplace, commensuration does indeed describe the way people typically behave. Some would argue that as a matter of fact, this is the way nearly all people behave in general (e.g., Rothenberg 1961, 234–35). This is one reason why the assumption of commensurability has been so widely adopted for specific empirical models of rational choice. Another is a technical consideration: without commensurability, preferences cannot be represented by a real-valued utility function. Yet this does not mean that choice without the assumption of commensurability—lexicographic preferences—is seen by theorists as irrational or nonrational.

Is there any consensus among the "moderns" for treating commensurability as a cornerstone of rationality? Not for von Neumann and Morgenstern (1953, 630) who describe that assumption as "probably desirable." Chipman (1960) even argues that lexicographic preferences, which induce vector-valued utility functions, are the most general and genuine basis for rational choice theory, with commensurability constituting just a special case. Taylor's (1973) analysis of politics employs the lexicographic assumption. Its use by Rawls (1971) is well known. Finally, Dobbs simply misunderstands Elster (1979) on this point. In some special cases, what appear to be lexicographic preferences are better understood, according to Elster, as constraints on decision making. But contrary to Dobbs (1987, 508) he does not mean that these constraints represent limits on rational thinking about the world. Rather, they represent limits on our ability to change the world. On the other hand, in cases when there truly are lexicographic preferences, Elster observes, the failure of commensurability to apply, "of course, does not mean that [those cases] are unamenable to rational analysis" (1979, 127).

### Revelation

I do not suggest that reason has no limits. Any instance of deductive reasoning involves assumptions that are not themselves deduced. Gödel's Theorem tells us no consistent set of assumptions can generate all truths; although this does not mean there are truths no consistent set of assumptions can generate (Quine 1976, 66). Inductive reasoning faces fundamental challenges (Goodman 1965). And what is the rational choice is not always clear. So in these senses there are limits to reason apart from any problems in using or relying exclusively on reason or in treating it as the whole of science.

Yet this admission falls far short of the conclusions Dobbs recommends. For even if reason cannot confirm every "self-evident" or revealed truth, the converse is not necessarily correct: no claims about the sacred, the self-evident, or anything else become true just because reason is unable to prove or disprove them. (Tocqueville's consequentialist argument for faith, cited by Dobbs [1987, 492], is irrelevant to the question of truth.) Revelation, in sum, is no automatic antidote to the real limits of reason. By the same token, in response to Leo Strauss's observation, "Philosophy has never refuted revelation," I would ask, To whose satisfaction? Dobbs certainly gives practitioners of reason no reason to question their understanding of the way knowledge of the world comes about, an understanding that has survived confrontations with revealed biblical truths about creation and other matters. Those, on the other hand, who have faith in an epistemology of revelation even when it conflicts with or transcends reason have thereby insulated themselves from the commands of rationality. What could a refutation of revelation possibly look like in their eyes?

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In my essay on reckless rationalism and heroic reverence in the *Odyssey* (Dobbs 1987) I called attention to a discrepancy in the poet's evaluation of two rational, strategically dominant choices. Homer condemns as utterly reckless the decision of Odysseus' crewmen to slaughter Helios' cattle, but he certifies the wisdom of Odysseus' formally identical decision to conceal his identity. I argued that this discrepancy is neither an artifact of editorial patchwork nor a "Homeric nod" but is instead indicative of a distinction that remains obscured by an exclusively rationalistic orientation, the distinction between foolhardy recklessness and discerning wisdom. The recklessness of Odysseus' crewmen lies in their treatment of the divine sanction of Helios' cattle as if it were merely a price that might be weighed in a common balance with other circumstantial considerations, such as their hunger. Their confidence in the boundless range of commensuration is, I suggested, the hallmark of a rationalistic cast of mind. The wisdom of Odysseus, by contrast, is manifest in his tempering a superlative intellect with respect for the *limits* of commensuration. Odysseus' reverence, according to my account, is established upon an insight or revelation that occurred in the course of extraordinary conversation with the shade of Achilles. In this encounter Achilles declares his preference for even the most miserable of lives over his current station as king of the dead. I pointed out that Achilles' speech is incongruous with his irreproachably courageous deeds, incongruous, that is, unless one sees that life itself is sacred. As indicated by Achilles' choice, the "value" of human life is not arrived at via rational commensuration in a nexus of exchange. As Odysseus learns, the sanctity of human life *defies* commensuration; it demands respect on its own terms, not by virtue of comparison with,

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or analogy to, anything else in this world (cf. Mt 16:26). Odysseus consequently rejects the boundless competitive ambition of his past, which he recognizes as nothing more than reckless commensuration in heroic guise. His remarkable change of heart disposes him to respect even the sacred cattle of Helios. For Helios, the sun-god, is sovereign over the days and seasons that measure the limits of our mortality. Odysseus' reverence for this god manifests itself in a proper respect for these limits, including the limits of human reason.

I suggested that we are in no way exempt today from the pitfalls of reckless rationalism. Our situation is precarious in that it is the commercial character of our own polity that inclines us toward such recklessness. For commercialism is merely rationalism in a three-piece suit. With its exclusive orientation toward "bottom-line" comparisons, commercialism takes for granted that everything has its price, including the very principles that make commercial freedom, to say nothing of our other freedoms, possible. As against rationalism, I suggested that *reverence* for common-law liberties and the equal rights with which we are endowed by our Creator may well prove to be our most reliable support in the struggle to keep the republic we have been given. Now Grafstein, in response, maintains (1) that the case I make against rationalism "is based on a misunderstanding of decision theory and rational choice"; and (2) that "even if the verdict against rationalism is nonetheless true, this does not by itself justify the endorsement of revelation as a higher or supplementary means to wisdom." He devotes only a concluding paragraph to the latter charge, directing his greatest effort toward establishing the former.

### **Rationality and Dominance**

Grafstein lodges three objections to my use of decision theory in interpreting Homer's *Odyssey*, contending (1) that "decision theory . . . does not recommend the principle of dominance in this kind of case"; (2) that my representation of the crewmen's choice situation is faulty; and (3) that my emphasis on commensurability as the crux of reason is inaccurate, at least as concerns modern decision theory. Let us consider these objections in order.

Is my use of strategic dominance in describing the crewmen's irreverent decision appropriate "in this kind of case"? Notice that the issue here calls for an interpretation of the specific circumstances presented in the *Odyssey*. The critical question, as Grafstein agrees, concerns the independence of the gods' decision to cooperate in punishing the crewmen from the crewmen's decision to slaughter the cattle. Grafstein addresses the issue of independence but his volleys miss their mark and, as we shall see, merely trace tangents to the central point. The problem is that Grafstein shows little interest in the specific circumstances that constitute this case. He dismisses specific textual evidence, which I cited to support my formulations, as "unclear to this nonexpert"; and he asserts, without any reference to the relevant data—the speeches in Homer's poem—that the necessary independence between the decisions of the gods and the crewmen is "difficult to imagine." Now, the independence condition is satisfied, in plain English, as long as it is not the crewmen's decision itself that causes divine cooperation, or more strongly, if other gods are as likely not to cooperate as they are to cooperate with Helios in punishing Odysseus' crewmen. This, I submit, is not at all difficult to imagine. Cooperation is under no circumstances to be taken for granted among Homeric gods. On the contrary, factiousness is their most notorious characteristic.



But we need not leave the disposition of this issue at the level of imagination. There is, as I have already indicated, specific textual evidence to support a positive claim for independence (p. 507). In the lines I cited, Helios delivers a powerful threat to Zeus, chief of the gods. Helios declares that unless the other gods cooperate and punish Odysseus' crewmen to avenge his cattle, he will leave his appointed station and go instead into Hades and shine for the dead. Now, it seems to me self-evident that Helios would not have resorted to threats had he expected the gods to cooperate in the punishment of the crewmen's sacrilege. Helios supposes the gods' cooperation is not to be taken for granted, *regardless of what the crewmen have done*. In other words, he sees the gods' decision to cooperate with him as independent of the men's action. This conclusion is confirmed by Zeus's response in the sequel. For Zeus takes Helios' threat seriously. He urges Helios not to depart and promises him in exchange that he will punish the Ithakans immediately by hurling a thunderbolt to split their ship into little pieces. It is finally Helios' threat, not the crewmen's sacrilege, that invokes Zeus's punishment (see *Odyssey* 12.382-88).

By Grafstein's own admission, the demonstration of independence suffices to undermine his objection to the use of strategic dominance of this case. But a miracle intervenes. From the ashes of Know-Nothingism concerning the plain speech of Helios and Zeus there arises a veritable phoenix among decision theorists. Grafstein rehearses at length (and with full *apparatus criticus*) his misgivings with the scholarly literature on Newcomb's Problem. If Grafstein is more interested in Newcomb's Problem than he is in Homer's *Odyssey*, that is, of course, his privilege. But if in opposition to my argument he means to assert the *relevance* of Newcomb's Problem (with its inherent violation of the independence condition)

to the case of Odysseus' crewmen, then he assumes the responsibility for a more careful study of the substantive details of Homer's story than he has thus far undertaken. Certainly, if one does not understand the "kind of case" *presented by Homer*, one is in no position to dictate appropriate theoretical devices for its analysis.

### Representation of the Problem

Next, Grafstein claims that I misrepresent the crewmen's choice situation by leaving all but secular considerations out of their calculus. I expressly stated that I constructed the decision matrix describing the crewmen's choice situation "directly out of the alternatives and consequences as they are formulated in the text" (p. 496). Thus Grafstein's quarrel is really with Eurylochos and the rest of the crewmen, who all assent to Eurylochos' formulation of the problem. The real problem, however, is that Grafstein fails to grasp the significance of this assent. He considers the crewmen's assent merely a tactical blunder. As a result, he holds that "the crew could reveal their preference but not their reverence"—as if the matter of their reverence remains unsettled. I maintained, however, that it is precisely the crewmen's assent to Eurylochos' characterization of their decision problem, *as being amenable to commensuration in the first place*, that testifies conclusively to their culpable irreverence. The crewmen err in treating the divine sanction of Helios' cattle as though it were simply a signal of the price charged by the gods for Thrinakian roast beef. They carelessly transgress the limits imposed by the sacred upon rational commensuration. This rationalistic cast of mind, which recognizes no bounds to commensuration and thus holds nothing sacred, is what Homer condemns as reckless.

## Rationalism or Revelation?

### **Commensurability**

In a third attempt to establish his thesis, Grafstein opposes my observation that commensuration is central to modern as well as classical conceptions of reason. Although he has no quarrel with the accuracy of this observation as it bears upon ancient thinkers, he is vehement in his denial of its validity for modern decision theorists. His argument, however, supports neither the denial nor its vehemence. For I agree that the "assumption of commensurability" is questioned by some of the most thoughtful decision theorists. The problem is that Grafstein misconstrues the point of their questioning. He mistakenly supposes that it somehow concerns whether commensuration is, in fact, the characteristic *modus operandi* of reason. He has confused what the "assumption of commensurability" means. The authors he cites do not, in fact, deny the role of commensuration in reason but only whether all objects of human choice should be assumed to be commensurable. In other words, the authors Grafstein cites are pointing out the fallacy of imposing a scheme of commensuration upon matters that are not amenable to commensuration. In this respect these decision theorists are to be applauded for avoiding precisely the recklessness for which Homer blames Odysseus' crewmen. I accordingly credited Elster (1979, 125) and others for accepting the existence of incommensurability "as defining limits to their rational-choice theory of politics" (p. 508).

Grafstein counters by pointing out that incommensurability does not "imply limits on rational thinking about the world." His argument is that choice without commensurability is not irrational. Now if one stipulates a different, and considerably scaled down, conception of rationality from the one I argued for in my essay, it would indeed follow that choice without commensuration is not irrational.

But what is new in this? I already made this point in reference to Achilles' preference for the most miserable of human lives over being king of the dead: "Achilles' preference, like all lexicographic orderings, does not violate transitivity. Lexicographic preferences are rational in this purely ordinal sense, though they preclude rational comparisons in the more meaningful sense of reason as commensuration" (p. 507). One may indeed say that "rational thinking" about the world remains possible apart from commensurability, if one is interested in maintaining a thesis at all costs. But it should give us pause to note that the cost paid for this stipulation is by no means insubstantial; "thinking" is now to be understood as restricted to checks for transitivity or consistency. As against my observation that commensuration is the hallmark of reason, this view would imply that the enforcement of consistency is reason's characteristic function. In short, it seems to me that Grafstein's defense of rationalism rests, ironically, upon a conception of reason that does not give reason its due credit. I hold reason in higher esteem, notwithstanding my criticism of rationalism.

### **Revelation**

Grafstein in a final paragraph elaborates his contention that "even if the verdict against rationalism is nonetheless true, this does not by itself justify the endorsement of revelation as a higher or supplementary means to wisdom." He notes that claims about the sacred should not be accepted as true "just because reason is unable to prove or disprove them." I heartily agree. Moreover, if this is what Grafstein means by "automatic," then his assertion that revelation is "no automatic antidote to the real limits of reason" strikes me as singularly uncontroversial. Who, really, would disagree?

Why this failure, then, to join any serious issue? The problem is that Grafstein is operating here with a surprisingly crude notion of revelation. Consider his *coup de grace*, the contention that science provides a better account of creation than does revelation. Note that in promoting this contest, Grafstein assumes without argument (1) that the questions that science seeks to answer are the same as those toward which biblical revelation is directed, and (2) that the interpretation of Scripture that he finds antagonistic to science is the correct interpretation. There is ample reason, it seems to me, to dispute these assumptions. Grafstein's view of the opposition between science and faith presupposes a degree of convergence of purpose that simply does not exist. Broadly speaking, modern science aims at prediction and control; while faith, it may be said, is directed toward discerning God's providence and will. This difference in purpose should be kept in mind in any attempt to pit modern science against the Bible. Besides, even the most literal reading of Genesis need not conclude, against the scientifically established geological record, that all of creation took place in six solar days. After all, the sun was not itself created until the fourth "day." Thus the meaning of "creation day" in Genesis is unclear and requires interpretation, an interpretation obviously different from the one presupposed in Grafstein's account.

I will resist the temptation to delve further into the serious issues to which Grafstein's concluding remarks merely nod a distant greeting. My essay, after all, is an interpretation of the *Odyssey*, not the Bible. I suggested that a revelation, or noetic insight, brings about a decisive change of heart in Odysseus. Homer presents this change of heart and the reverence that accompanies it as the critical factor in Odysseus' heroic return to home and throne. Nevertheless, Odysseus remains for Homer, and for

Homer's audience, the quintessential man of reason. Thus the very fact that it is *Odysseus* who acknowledges the limits of commensuration stands as an impressive, though admittedly not a conclusive, argument for the existence of these limits. It would be as wrong, however, to close one's mind to the impressiveness of this argument as it would be to suppose that it, taken alone, is conclusive. In this spirit, and with the tools of decision theory, I recounted the story of Odysseus as the Homeric case of (to paraphrase Hume) a thinker "turning the weapons of reason against the rationalists." I hoped as well to warn against the self-destructive rationalistic tendencies present in our own commercial republic. But, in the last analysis, it must be admitted that what Homer taught and what we find beneficial are not, for these reasons alone, to be certified as true (though once again the evidence is impressive). Nevertheless, I consider the truth of Homer's teaching—once we correctly grasp what that teaching is—to be the most important question. It is all the more regrettable, therefore, that Grafstein's comments to not more successfully join this issue.

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## Rationalism or Revelation?

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## POLITICAL CULTURE AND POLITICAL PREFERENCES

**A**aron Wildavsky has argued that it is theoretically more useful to think of political preferences as rooted in political culture than to entertain alternative bases such as schemas or ideologies. In the APSA presidential address in which he made his case, Wildavsky also advocated a program of research on political cultures, and welcomed "challenges and improvements." David Laitin accepts the invitation; he variously takes issue with Wildavsky's concept of political culture.

**W**ildavsky (1987) poses an important question: What accounts for people's political preferences? He also outlines a research program that can discriminate among competing explanations—for instance, culture, self-interest, tradition, and class—to account for political preferences. Finally, he contends that *culture* can explain a wide range of political preferences.

Unfortunately, Wildavsky's approach is subject to some of the same criticisms as were leveled at the "political culture" school that had its "hour upon the stage" in the 1960s. By trying to explain generalized preferences with cultural variables those scholars depleted the culture concept of any analytic power.<sup>1</sup> Lest we spin our disciplinary wheels on the same frictionless framework, I will propose three theses that should guide future work in political culture.

1. Culture instills not values to be upheld but rather points of concern to be debated.
2. Culture is Janus-faced: people are both guided by the symbols of their culture and instrumental in using culture to gain wealth and power.
3. Culture traffics in symbols, and symbols must be interpreted in full ethnographic context.

### Values versus Points of Concern

Wildavsky defines *culture* as "shared values legitimating social practices" (p. 6) even though he understands all too well that there is rarely a value consensus within a culture. He implies that environmentalists share one culture and Reaganites another in that one group trusts central authority and the other does not. But this stretches our common understanding of what culture is. We can think of Iranian Shiites or Californian surfers as each sharing a common culture. But to say that a person who seeks to protect society from nuclear catastrophe by opposing the expanded use of nuclear energy but doesn't object to the enrollment of a student with AIDS in his or her child's schoolroom belongs to a different culture than those who feel otherwise is to misrepresent what is meant by culture. These sorts of political difference are common among members of a single culture.

Once we accept that within a culture there are diverse strands of opinion and conflicting values, what can be deduced from the notion of people "sharing" a culture? Thomas Metzger has an answer. In his essay on neo-Confucian society (1977), he sees culture not as values that are upheld but rather as "points of concern" that are debated. Rather than ex-

amine Confucian bureaucrats' preferred answers to questions of value, Metzger examines the questions generally considered worth asking and the clichés implicitly accepted. "We can understand" the Neo-Confucians, Metzger suggests, "by understanding what they worried about . . . what their shared points of uncertainty and concern were. . . . Therefore I suggest looking at Neo-Confucianism as a widely shared 'grammar' defining the problems of intellectual struggle by positing a discrepancy between the goal of life and the given world. Sharing this grammar, Neo-Confucians differed in terms of their solutions for these problems" (p. 14). To be sure, cultures do support some common values.<sup>2</sup> But a general focus on points of concern rather than shared values provides a richer appreciation of why political action differs across cultures (Laitin 1986).

With this idea of "points of concern" kept in mind, Wildavsky's four-box matrix of culture types (1987, 6) illustrates something important about his own culture. The central issue addressed by this matrix is the trade-off between individual freedom and government authority. Indeed this issue has been a central point of concern in the Western political tradition. Questions of whether the state has the right to impose risk on individuals will therefore be ever present on the political agenda of Western societies. But in other cultures, for instance, the neo-Confucian, these issues are not juxtaposed as opposites or as obvious trade-offs. What we learn from Wildavsky is the central importance of the debate about freedom and hierarchy in his culture, and we can predict that consensus will often fracture along this fault line. Good research in political culture should illustrate how debates get framed in other cultural arenas.

By focusing only on shared values, Wildavsky misses the point that people with strongly opposed views can share a

culture and that people with similar views can come from different cultures. He also misses the true comparative possibilities of a good culture theory.

## Two Faces of Culture

By arguing that the sources of political preferences are answers to two questions (Who am I? What shall I do?), Wildavsky does not answer his question about the source of preferences but merely pushes it back one step. For we can ask of him: What is the source of answers to those questions? (Could it be *interest*?) Wildavsky ignores the question of how people make decisions about their loyalty by treating it as if it were a trivial act. "By figuring out their master preferences, as it were—who they are and are not, to what groups they do and do not belong—they can readily figure out the rest" (p. 8). He is obviously begging the question here; for people may be strategic in answering these questions. Wildavsky derives from his Table 2 (p. 17) that the cultures people are in give them clues as to how to think about the poor. But the fact that businessmen (the "culture" of business?) attribute poverty to the actions of the poor smacks of self-interest rather than culture. Could they have answered the questions about who they were with a careful eye to what would serve their interests? Wildavsky doesn't address this important question.

In trying to discredit interest theories, Wildavsky sets up a rigid dichotomy between exogenous and endogenous sources of preference.<sup>3</sup> Wildavsky's battle is with the economists who assume that preferences are exogenous and consequently refrain from analyzing them. He argues that the source of these preferences requires explanation and that "culture" is the most compelling explanation for them.

Wildavsky, however, provides us with a one-faced view of culture. He joins (un-

successfully, as I shall argue) an army of cultural anthropologists that has long argued that microeconomic theory cannot explain the different preference functions that vary systematically across cultures.<sup>4</sup> Any culture theory obviously must analyze how these preferences are created and sustained. This is culture's endogenous element.

But culture has a second face, one that Wildavsky's conceptualization misses entirely. To share a culture means to share a language or a religion or a historiography. Very rarely do these cultural systems coincide perfectly within a large society. People must often choose which among their religious group, language group, and so on will be their primary mode of cultural identification. This choice is often guided by instrumental reasoning, based on the potential resources available for identifying yourself, for example, as a Catholic as opposed to a French speaker.

The instrumental face of culture reveals even more. Once a cultural group organizes politically, the common symbolic system makes for efficient collective action. Tamils in Sri Lanka, French speakers in Quebec, and Jews in the Soviet Union can be easily maintained as groups not because culture is more real than class but because organizational costs are relatively low when common and powerful symbols are readily available and rules of exclusion easily formulated. Political entrepreneurs exploit this organizational advantage to make collective claims for resources. This second face of culture—in which shared symbols are used for instrumental ends, ends for which preferences are best taken as exogenous—is entirely missed in Wildavsky's culture theory.<sup>5</sup>

To illustrate this point from my field research in Yorubaland—I found that there were two candidates for primary cultural identification. Yorubas could see themselves primarily in terms of their association with an ancestral city or in

terms of their association with a world religion. Both foci had embedded in them points of concern that implied different political emphases. That most Yorubas see their primary cultural framework to be that of their ancestral city and see their attachment to a world religion as peripheral to their real identity can be explained by exogenous factors. (An external *hegemon*, in order to foster social control at low cost, played a crucial role in making that the preferred cultural framework.) Political entrepreneurs then exploited the powerful and available symbols of ancestral city identification to organize political action seeking to enhance their group's wealth and power.

Once the cultural framework is established, however, endogenous factors having to do with the points of concern in ancestral city life can help explain other political preferences, and make sense out of intra-Yoruba political debates (Laitin 1986). Wildavsky is therefore right to point to the endogenous cultural element as a source of preferences. Indeed, out of the construction of a common culture, certain points of concern will emerge as obvious and important.

But a good theory of culture must also point to the fact that people are instrumental about which aspect of their cultural repertoire is of primary significance and that shared symbols constitute a political resource that can be effectively exploited by political entrepreneurs. Here is where a culture theory can usefully rely on exogenous sources of preferences. This is the Janus-faced element in cultural theory that Wildavsky's theory misses.

### Ethnographic Analysis

By using simple attitudinal data as indicators of culture, Wildavsky breaks

with those anthropologists who otherwise agree with him that culture is endogenous. For example, in a stimulating critique of the Douglas and Wildavsky volume, *Risk and Culture*, (1982) Marcus and Fischer (1986), who are in general sympathetic to an endogenous theory of culture, criticize the authors for attributing a set of culture traits to members of the Sierra Club, Friends of the Earth, the Environmental Coalition on Nuclear Power, and the Clamshell Alliance without having provided field data to support their observations. Douglas and Wildavsky liken these organizations to irrational sects of religious zealots. Unfortunately, as Marcus and Fischer pointed out, "no ethnographic evidence [on the organizations or on religious sects] is provided to support these positions" (p. 149).

In his presidential address, Wildavsky attempts to justify his lack of ethnographic data by praising the virtues of parsimony (1987, 4). The question then becomes, Can you do culture theory without doing ethnography?

Unfortunately for Wildavsky's position, the survey research data on which he relies is woefully inadequate to explain the cultural sources of political preferences. Consider his hypothesis that on the debate over technology risks the issue is really "a referendum on the acceptability of U.S. institutions. The more trust in them, the more risk acceptance; the less trust, the more risk rejection" (p. 14). To support his culture theory, Wildavsky reports that nuclear energy experts and the military are far more likely to see nuclear power plants as safe compared to movie producers and elite journalists. But Wildavsky lets us down with his lack of data. Do the military find acceptable all U.S. institutions, for instance the Bill of Rights, more than elite journalists? Is the connection between a preference for a strong defense and a willingness to take risks in regard to nuclear energy a cultural

one? Perhaps because nuclear power is an everyday reality in the military, army personnel are inured to the everyday dangers of nuclear energy.

Wildavsky is insightful in seeing packages of apparently unconnected political views as emerging from a common framework concerning general views about authority. But what draws these people together? Is it common symbols, common interests, or intense interaction? How strong are these alliances? How much slippage occurs when, for example, undergraduates in the Sierra Club become corporate lawyers? (Do they then change culture?) What about a retired officer who settles near a polluted lake and joins an environmental group seeking to put controls on the disposal of waste by local industries? Has he changed his culture? From the attitude surveys Wildavsky cites, we cannot even know whether the answers to the questions, Who am I? What shall I do? have any connection to political positions in regard to nuclear energy.

Is ethnography necessary to answer these questions? It is clear that better-designed survey data could address many of them. Surveys, however, have inherent limits. The claim that culture provides guidance for political action implies that symbols frame and rituals sustain a particular political vision. Part of the power of culture is that its members are not fully conscious of the sources of their visions and, even if honest, would not necessarily provide the relevant data to survey researchers. Participant observation and the other tools of ethnography are therefore crucial in making connections between world view and political action.

Having lived in what he calls a "political-medical museum" for a good many years, Wildavsky is well placed to provide ethnomedical data. The third thesis on culture demands that he report those data and interpret them in full social context. Any culture theory would be improv-



erished without the collection of data on the symbols, rituals, and interactions that constitute and reconstitute cultures.

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From the very beginning of political studies, efforts have been made to create categories and concepts that would facilitate the comparison of institutions bearing different labels and sporting a variety of practices. My cultural theory of why preferences come from institutions is but one of the recent entries. In view of the well-known difficulties facing this enterprise, skepticism is in order. But I would not have thought that the task itself would be rejected in favor of a return to the nominalism—country or tribal labels—from which we have striven so hard to escape. To speak of a Yoruba or a U.S. or a neo-Confucian “culture”—as if all members shared the same values and legitimated the same set of social relations—denies the possibility of conflict within such a group, as Laitin mistakenly accuses me of doing.

Adherents of cultures learn their identity by knowing not only what they are for but also what they are against. It is cultural conflict that gives meaning to cultural identification. Where Laitin states in his first thesis that “culture does not instill values to be upheld but rather points of concern to be debated,” I might agree that his definition was useful if it referred to debate among rival cultures within a country or group rather than to debate within a single “culture” identified with a single group. For in my terms his formulation would have to mean that adherents of a single culture (say, hierarchy) are at least as far apart from each other as they are from adherents of individualism or egalitarianism or fatalism. Conceiving of countries or groups as compounds of cultures avoids this difficulty.

Laitin contends that “to say that a person who seeks to protect society from nuclear catastrophe by opposing the expanded use of nuclear energy but doesn’t object to the enrollment of a student with AIDS in his or her child’s schoolroom belongs to a different culture than those who feel otherwise is to misrepresent what is meant by culture.” Adhering to a given culture on a conceptual level is determined by measures of two dimensions: strength of group boundary and degree and variety of prescription. Preferences regarding risk are a consequence, not a cause, of cultural identification (Douglas and Wildavsky 1982).

On the empirical level, I cannot misrepresent what is factually true, that is, truer than alternatives. The likelihood of a person or group who evidences an across-the-board concern with the harmful consequences of technology taking a position in favor of gay rights—including, say, opposition to compulsory testing for AIDS—is much greater than would be true of those who take a much more benign view of technology. (Consult your daily newspaper). Look and see for yourself. Because across-the-board concern with technological danger stems from commitment to an egalitarian way of life (the detailed argument is in Douglas and Wildavsky 1982), whereas support for technology as largely benign bespeaks commitment to hierarchy and individualism, these differences are well predicted by cultural theory. Those who disagree should ask themselves why, when the ideology of a group is known, its inclinations toward AIDS and technology follow as a matter of course.

According to Laitin’s second thesis—“Culture is Janus-faced: people are both guided by the symbols of their culture and instrumental in using culture to gain wealth and power”—culture is subordinate (“instrumental”) to more major concerns and unstable (“Janus-faced”). Were the claim of instability true, it would be difficult to explain how any grouping of

people coheres. There could be no social science, both because there would be no social relations that could cohere and because there would be no regularities to observe. Obviously, Laitin has something else in mind. Enter our old friend "interests," especially the businessmen "with a careful eye as to what would serve their interests."

Readers are referred back to the original article as well as to the important paper by Michael Thompson and Michiel Schwarz (1985) for a brief discussion of the difficulties of basing political science and economics on a preexistence of interests, not inquiring as to how people came to know their interests and assuming the question to be outside the scope of these disciplines. Here I wish to observe only that when Laitin denies that preferences stem from social interaction in building up and tearing down institutions, he separates politics from social life. If, as he maintains, people are *both* guided by their cultures *and* instrumental in using shared symbols to reach their own political ends, political objectives are separate from and superior to social relationships. The instrument controls the maker.

In claiming that I do not provide ethnographic data to test my theory, Laitin attributes to me and Mary Douglas a position exactly opposite from that we take in *Risk and Culture* (1982). He says that we lump together organizations like the Sierra Club and the Friends of the Earth and "liken these organizations to irrational sects of religious zealots." The whole point of describing and analyzing these two organizations was to show that they are culturally different, the Friends of the Earth at that time being an egalitarian sect and the Sierra Club more of a hierarchy. And we drew our accounts, as the footnotes indicate, from much longer ethnographic studies that would, if included, have increased the size of our book by half again. True, we did not

understand then that our use of this classical term *sect*, following such luminaries as Max Weber, would lead some readers to conclude we meant these groups were akin to *religious zealots*. Zealots, yes—as they themselves are proud to say—but religious, not necessarily. For this reason I refer to the Friends-of-the-Earth-type culture as *egalitarian*. Calling this or the other cultures "irrational" would be anathema to us. To do so would not only trivialize our theory, it would also deny our major analytical purpose, which is precisely to rationalize the conflicting positions. We wished to show that conflict over the risks of modern technology has its roots in the most characteristic human activity, the efforts of adherents of different cultures to strengthen their preferred way of life at the expense of others by claiming their institutions make people healthy whereas those they reject make people sick.

Are we correct in believing that the conflict over risk is fundamentally about the acceptability of institutions? Since Laitin challenges my view that differences over nuclear power between, say, elite journalists and others are rooted in culture, a test can be made. The extensive data Laitin wished me to put in my paper, which would have been out of place as one of a number of illustrations, has been provided in a recent issue of this *Review*. As Stanley Rothman and S. Robert Lichter (1987, 384-85, 398-99) conclude in their study of different theories accounting for elite positions on nuclear power,

our own study of U.S. social, economic, and political leaders suggests that Douglas and Wildavsky are correct in arguing that ideological factors have also played an insufficiently recognized role in citizens' changing perception of risk. . . .

The argument undergirding this essay is that the new environmental movement in the United States is partly a symbolic issue. Some liberal or radical members of key strategic elites (especially the news and entertainment media and public-interest groups), who are alienated from the social and political system but live in an environ-

ment in which they cannot directly criticize the hegemony of liberal capitalism, are drawn to concrete issues that serve as a surrogate for such a critique.

The process of substituting such issues for a broader critique of the society is not necessarily a conscious one, and the issues chosen are often genuine, that is, problems with which the society must cope.

No doubt this is not the last word. But at least the study indicates that a reasonable person with respect for evidence could believe that our theory has merit.

Laitin would like me to explain how and why individuals make their most important commitments, their choice of culture or way of life—a tall order. Michael Thompson and I are writing a book to be called *The Foundations of Cultural Theory* (n.d.), in an attempt to answer this question. But perhaps a few sentences will indicate what a cultural approach would look like.

In chapter one, "The Problem of the First Week," Thompson and I lay our social cards on the table: in the beginning, we say, were cultures. There are no isolated individuals because they would be unknown and unknowable outside a group context. Self-dealing is a curse, not a context. The cultures with which we are concerned have always existed. The problems of the first week are to explain why cultures (1) are limited in number and (2) take the form we say they do. This requires an analysis of *which* values are sharable and *which* kinds of social relations are livable. "Pushing [the source of preferences] back one step," it turns out, is a formidable task; fortunately, it is unnecessary.

The problem of the second week (and all the weeks thereafter) is to explain how and why individuals organized into the various cultures change their culture. Where the problem of the first week is stability—How and why does any set of social relationships and supporting values cohere amidst the primordial flux?—the problem of the second week is to explain

change. I have speculated about the possible motives and directions of change (Wildavsky 1985). Without attempting to go further now, I would merely observe that if such explanations were successful, the task of social science would be over before it had hardly begun. While I appreciate the desire to know it all, I don't think social scientists are in imminent danger of technological obsolescence just yet.

"What about a retired officer who settles near a polluted lake and joins an environmental group seeking to put controls on the disposal of waste by local industries?" Laitin asks. "Has he changed his culture?" Leaving aside the culturally broader question of how pollution is socially defined (Silicon Valley water wells are said to be "contaminated" even though their carcinogenic content per glass is less than that of a corresponding glass of Berkeley tap water because chlorine forms chloroform, which is a weak carcinogen), the answer to the question depends on what else we know about our hypothetical officer. If he has reason to believe that he and his family will be adversely affected by conditions in the lake, considering that pesticides in the food chain amount to ten thousand times less than those naturally occurring in our daily food, the officer may be exerting his sense of local rationality (see Ames 1983, 1987). When a tornado approaches, I expect to get me and mine out of the way, not to invoke cultural theory. Should this officer join other groups and engage in other actions protesting the effects of technology, however—the "across-the-board" condition mentioned earlier—it is likely that he *has* changed his culture and not only his address.

Just as people dispute *tastes* all the time, there is likewise reason to argue about *definitions*. My strong preference is to build a cultural theory from social dimensions, following Mary Douglas's "grid" (which I call "prescription") and "group" (1970, 1982). In this way, dimensions

form categories of cultures. Relative degrees of adherences to these ways of life can then be measured. And people's position in regard to their cultural context—not only the cultures to which they adhere but also the strength of rival cultures—can be used to make falsifiable predictions about many matters of interest. This progression—dimensions, categories, predictions—seems to me a fruitful mode of theorizing. A concept such as Laitin's "points of concern" does not. Of course, the predictions generated through use of cultural theory are more explicit and hence easier to try to refute. I thought that generating instructive failures (no Popperian refutations without prior conjectures) was what theorizing was all about.

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### Notes

1. See the claims for the culture concept in Pye and Verba 1965. In fact, Wildavsky's concept of culture harks back to Truman's (1951) notion of the latent interest group.

2. For example, Tocqueville's observations that U.S. citizens value linguistic uniformity but religious diversity still hold today. This is a clear example of value consensus.

3. Exogenous sources—the basis of interest theories—suggest that preferences are taken as givens, coming from outside of the system, and provide guidance for making political decisions; endogenous sources, the basis of Wildavsky's counter-theory, suggest that preferences are formed and reformed as part of the process of making political decisions.

4. In this regard, Clifford Geertz's works have been most influential in political science. See his "Thick Description" in Geertz 1973.

5. Social anthropologists have emphasized this exogenous face of culture. See Barth 1969 and Cohen 1974. In political science, Robert Bates (1983) has developed this framework with great rigor.

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### Forthcoming in September

The following articles, controversies, and research notes have been tentatively scheduled for publication in the September 1988 issue:

Hayward R. Alker. The Dialectical Logic of Thucydides' Melian Dialogue.

Gabriel A. Almond, Sergio Fabrini, Theodore Lowi, and Eric Nordlinger. The Return to the State. A Symposium.

Lawrence Baum. Measuring Policy Change in the Supreme Court: Civil Liberties in the 1946-85 Terms. A Research Note.

Stanley C. Brubaker. Can Liberals Punish?

Steve Chan. Defense Burden and Economic Growth: Unraveling the Taiwanese "Enigma." A Research Note.

Harry Eckstein. A Culturalist Theory of Political Change.

Scott L. Feld and Bernard Grofman. Ideological Consistency as a Collective Phenomenon.

James C. Garand. Government Growth in the States: A Longitudinal Test of Competing Explanations.

Charles J. Helm, Stanley Rothman, and S. Robert Lichter. Social Science Research as a Surrogate for Ideological Critique: Asking the Right Questions. A Controversy.

George A. Marcus. The Structure of Emotional Response: 1984 Presidential Candidates.

George A. Quattrone and Amos Tversky. Contrasting Rational and Psychological Analyses of Political Choice.

Bradley Richardson. Constituency Candidate and Party in Japanese Parliamentary Election Voting.

James N. Schubert. Age and Leadership Style: The Active-Passive Dimension.

Kaare Strom and Eric Brown. Model-fitting and Explanations in Coalition Studies. A Controversy.

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**First Things: An Inquiry into the First Principles of Morals and Justice.** By Hadley Arkes (Princeton: Princeton University Press, 1986. xii, 432p. \$50.00, cloth; \$11.95, paper).

Hadley Arkes, professor of jurisprudence and political science at Amherst College, sets himself no modest task in this study. There can be no escape from the confusion and paralysis that attend contemporary political and moral discourse, Arkes urges, until we recognize that "in morals, as in mathematics," satisfactory solutions to specific problems must be grounded in "axioms or necessary truths" (p. 6). Philosophers from Plato to Locke and politicians from Madison to Lincoln sought to provide such a grounding, but contemporary thinkers have largely given up the task.

The foundation, he insists, must contain only universally valid principles, and they must be true not contingently but of necessity. This condition can be satisfied if such principles are shown to be derivable from undeniable and self-evident truths about human beings and human actions.

The opening chapters of the book provide a clearer statement of the philosophical stance that Arkes rejects—the agnostic cultural relativism that, he argues, constitutes the credo of contemporary politics and social science—than of the details of his own theory. An unusual and ambitious program emerges, however, both from the expository chapters 4 and 5 and from summary versions of the same argument throughout the book.

A specific judgment concerning morality and justice, Arkes believes, is valid only if it is grounded in the fundamental "principle of natural equality," the principle that persons should be treated alike unless there is some justification for treating them differently. This principle in turn follows from two propositions: "(1) that human beings possess the capacity to reason, and (2) that morals exist" (p. 71).

And the latter statements, Arkes argues, are not merely but necessarily true. Anyone who

denies the former can be shown to have refuted his own assertion, for to discuss whether we have a capacity to reason is to exercise precisely that capacity. To deny the latter is to forego all distinction between wanton cruelty and selfless charity, between Hitler and Mother Teresa. Moreover, the skeptic who would deny the existence of morals cannot avoid using moral language to make his own case. But if the basic principles of morality are entailed by necessary truths, they are themselves necessarily true. Thus is relativism put to flight.

These matters of moral epistemology take up part 1 of *First Things*. In part 2 Arkes applies his theory to specific issues: conscientious objection, international intervention, the duty of rescue, welfare rights, privacy, and abortion.

One cannot but admire the scope of a work that begins with Kant and ends with detailed discussion of *Griswold v. Connecticut* and *Roe v. Wade*. A forceful political vision is evident throughout—a conservatism of principle that has no patience either with liberal idealism or with the moral nihilism of many libertarians. But when Arkes seeks to derive from his theory the conclusions that proportional taxation is unjust (p. 314), that abortion should be prohibited under all circumstances, including rape and incest (p. 403), and that "the level of force used by the United States [in Vietnam] was not out of proportion to the evil that was being resisted," not all who share his principal perspective will be willing to follow. (The moral defense of U.S. involvement in Vietnam is a leitmotif running through the book—*Vietnam* and *abortion* are two of the three largest items in the index.) Nor is Arkes's argument strengthened by *ad hominem* attacks on Senator George McGovern (p. 271) and Justice Harry Blackmun (p. 306) or by extended and graphic descriptions of aborted fetuses (pp. 412–14).

There are serious problems in Arkes's central argument as well. He rests his entire case for morality on the necessary truth of his basic moral principles, but he overlooks the crucial

distinction between sentences that assert logically necessary propositions ("Six is an even number") and sentences that, when spoken, constitute self-confirming utterances ("I am speaking now"). The latter can be called "undeniable," for to assert their falsehood is to provide grounds for their truth. But they characteristically express contingent and not necessary truths. Arkes insists that the two propositions cited above as the foundation of morality are necessarily true, but in supporting this claim he shows only that the attempt to deny them is self-refuting. A pragmatist might argue that we should ask for no more in the foundations of ethics. But it is far less than Arkes himself insists we need.

Further difficulties arise when we proceed from foundation to superstructure. Arkes's principle of natural equality—the requirement that all be treated equally except where there is a justification for unequal treatment (pp. 68–71)—is phrased in such broad terms that it seems extremely permissive. (Imagine a marriage vow: "I promise to be faithful to you unless I have a good reason not to be.") Yet he understands it to entail as a direct consequence the illegitimacy of any but a democratic polity and the right of any democracy to invade any monarchy or oligarchy for the purpose of toppling its government (pp. 46–48). The inference seems to rest either on unstated premises or on rather special rules of logic.

*First Things* is a book difficult to classify: a serious essay in political philosophy that seems at times intended as revenge against old adversaries, a work that enlists supporting argument from Aristotle and Kant in its opening chapters and from Senator Jesse Helms in its last chapters. The sympathetic but critical reader will be challenged to reflect not only on whether Arkes's philosophical proposals can meet the objections they invite but also on whether they might be enlisted in support of a vision of society less ideologically constricted than that articulated here.

DAVID A. HOEKEMA

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**The Strange Liberalism of Alexis de Tocqueville.** By Roger Boesche (Ithaca: Cornell University Press, 1987. 288p. \$29.95).

Not long after the publication of the first part of *Democracy in America*, Tocqueville complained that while many praised him, few understood him. The difficulty of his meaning and the struggle to lay claim to his authority have continued unabated into our own time. In *The Strange Liberalism of Alexis de Tocqueville*, Roger Boesche (also editor of the welcome *Alexis de Tocqueville: Selected Letters on Politics and Society* [1985]) has joined the lists. Boesche's effort deserves comment for both methodological and substantive reasons.

Boesche insists that understanding Tocqueville requires that one replicate Tocqueville's own historical method—that one must begin by seizing "the true spirit of the age" (p. 19). This is the aim of part 1 of this study. Boesche displays a broad familiarity with the intellectual currents and concerns of Tocqueville's generation, moving easily from discussions of philosophy, art, and literature to prominent themes in the popular press. Balzac, Hugo, Bonald, Maistre, Carlyle, Stendhal, Sand, Chateaubriand, and Guizot make frequent appearances in these early pages. At his best, Boesche reveals a striking similarity of vantage point among writers of the most various temperments and political allegiances. (On occasion, however, Boesche's effort to show the breadth of a shared point of view by piling citation upon citation robs the analysis of some of its energy.)

At the center of this generational vantage point is a preoccupation with the collapse of traditional ties and the unleashing of an untrammelled egoism. Boesche focuses upon the "disgust" (p. 39) felt by Tocqueville and many of his contemporaries for bourgeois culture and social relations and their hatred of the petty passions and purposes of their own time. But while Tocqueville shared the anxieties and what might be called the social aesthetics of his generation, he rejected the romantic and historicist responses to which his contemporaries characteristically turned. In these matters, Boesche breaks important new ground in Tocqueville studies.

Tocqueville's own response is detailed in parts 2 and 3 of Boesche's study, part 2 focus-

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ing on Tocqueville's "hopes" for freedom and part 3 on his "fear" of despotism. The most valuable aspects of these later analyses are those that most directly extend the social and cultural perspectives developed in part 1. Boesche underscores Tocqueville's contempt for the middle classes and (somewhat less successfully) the distance between Tocqueville's economic views and those of liberal political economists such as Nassau William Senior (with whom Tocqueville had a long personal relationship). These arguments are preface to an elaboration of Tocqueville's "strange liberalism."

In parts 2 and 3, Tocqueville's generation fades into the background as Tocqueville emerges as the latest in a French tradition whose members include especially Pascal, Montesquieu, and Rousseau. Whatever their differences, the members of this tradition are notable for having rejected "the great undertaking of modern liberal thinkers," namely, to conceive "republican institutions without republican virtue" (p. 194).

Boesche saves Tocqueville from those who would make him a theorist of interest (a worthy task) by portraying him in large part as a classical republican (a risky business). For example, Boesche's Tocqueville comes close to equating the seeking of freedom with the seeking of glory. While there is more than a little truth in such a portrait (Tocqueville always hoped that democratic societies might yet pursue "lofty purposes"), it also risks depriving Tocqueville's analysis of modern society of its contemporary salience. Tocqueville described himself as "a liberal of a new kind." The difficulty and the great value of Tocqueville's teaching stems from the tensions and paradoxes embedded in his thought. His limited confidence in constitutional mechanism sits uneasily with his praise of Madison. His love of independence exists alongside his fear of individualism. His conviction that liberty is an instinctive taste is not readily reconciled with the instrumental rationality of democratic peoples. Boesche is well aware of these ambivalences, but in his struggle to wrest Tocqueville from the old liberals, the parameters of the new liberalism and its relation to the old liberalism are not fully elaborated.

BRUCE JAMES SMITH

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**Is Democracy Possible? The Alternative to Electoral Politics.** By John Burnheim (Berkeley: University of California Press, 1985. vii, 205p. \$22.50).

**Democracy and Capitalism: Property, Community, and the Contradictions of Modern Social Thought.** By Samuel Bowles and Herbert Gintis (New York: Basic Books, 1986. x, 244p. \$16.95).

*Is Democracy Possible?* Not today but possibly sometime in the future, says John Burnheim of the University of Sydney. He maintains that both capitalist and socialist states have failed to produce democracy. What they have produced in practice is bureaucracy, centralization, and oligarchy. "Neither the constitutional state nor the unlimited state can be controlled effectively by the people. Neither can do very much to improve the provision of public goods without increasing either the scope and rigidity of the state apparatus, or its arbitrariness and lack of accountability" (pp. 49-50).

Burnheim argues that electoral systems inherently breed oligarchies and are "inimical to rule by the people for the people" (p. 83). Campaigns mystify issues through advertizing and image-making techniques. Issues are aggregated so that voters cannot separate them out from each other in making a decision. Elections select leaders who are not the best and brightest but those most willing to "deal with the continual jockeying for influence and position in party life, the toadying to those already in positions of power" (p. 101).

What is to be done? Burnheim maintains that democracy is possible only if (1) the state system is abolished, and decision making becomes radically decentralized; (2) most decisions are made by specialized autonomous agencies that are coordinated by negotiations; (3) participation extends to those affected by the decision in question; and (4) decision makers are chosen by lot from those affected and volunteer to hold public office. "Democracy is possible only if the decision-makers are a *representative sample* of the people concerned" (p. 9). Burnheim calls such a system "demarchy." He admits that his project is utopian but argues that it can be brought about only by convincing people that it ought to be tried.

I would question whether the right form of

procedural democracy will ever be sufficient to create substantive democracy. Inequalities in resources and power or consensus on anti-democratic values may still obstruct the development of democracy despite its form. Decentralized government by lottery might not have prevented the Nazi regime from terrorizing Jews if the majority of Germans consented to it. Negotiators are not all equal at a bargaining table if some are backed up by external sources of power. Despite these questions, I believe that Burnheim's book is penetrating, subtle, and original.

Like Burnheim, Bowles and Gintis begin their project with a critique of liberalism and Marxism. Whereas liberalism is too obsessed with liberty and private space, Marxism is too obsessed with classlessness and public space. Whereas liberalism is silent on exploitation and the community, Marxism is silent on choice, liberty, and democracy. Liberalism teaches us that power emanates from the state; Marxism teaches us that power emanates from class structure. Both, however, fail to comprehend the basic emancipatory thrust of capitalist societies: "Progressive social change in the liberal democratic capitalist societies has followed the logic of collective opposition to oppression suggested by Marxian theory, while adopting the liberal language of rights and the goal of democratic empowerment" (p. 25).

Bowles and Gintis see liberal democratic capitalism as an intrinsically conflictual social system. Taking a "clash of rights perspective," they see democratic capitalism as being driven by the contradiction between personal rights and property rights. Thus the history of U.S. capitalism is characterized by four basic accommodations: Lockean (limiting participation), Jeffersonian (distributing property), Madisonian (fostering heterogeneity), and Keynesian (redistributing income). The logic of the clash of rights divides society into public space and private space and into learners and choosers.

Given the logic and history of the clash of personal and property rights, Bowles and Gintis argue that history may work itself in three directions: global liberalism (worldwide expansion of markets and mobility of capital), neo-Hobbesian liberalism (the bureaucratic state, patriarchal family, and capitalist firm), and postliberal democracy (human development). The authors favor the development of

postliberal democracy, a vision based on "learning governed by the exercise of personal rights" (p. 178). This book makes a significant contribution to further understanding the conditions limiting and extending democratic culture and institutions. And as a by-product, it sharpens historical structuralism as a method of analysis.

DICKINSON MCGAW

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**Justice: Views from the Social Sciences.** Edited by Ronald L. Cohen (New York: Plenum Press, 1986. xii, 283p. \$32.50).

**Justice in Social Relations.** Edited by Hans Werner Bierhoff, Ronald L. Cohen, and Jerald Greenberg (New York: Plenum Press, 1986. xvi, 364p. \$39.50).

These two edited volumes explore the frontiers of research regarding justice, especially distributive justice. Although they were obviously seen as part of the same project, they are very different. *Both* could be considered candidates for your library. What are their merits?

*Justice: Views From the Social Sciences*, is a volume designed by formula. Its essays, edited by Ronald Cohen, describe justice as perceived by each of the major social science disciplines, philosophy, and the field of public policy. These are solid essays, each one oriented around the notion that our conceptualization of justice can be seen to be determined by the nature of man and the cultural environment of those doing the defining. Each essay is about the conceptions and controversies regarding justice in each of the disciplines and, as such, reviews the history of the discipline's literature from these perspectives. In general, as reviews, they are of high quality, informative, broad, and taken from a perspective of obvious interest to the authors. Each essay pursues aspects of the defining characteristics of the subject of justice as conceptualized within that literature. As such, the entire volume can be seen to live up to its title and is a useful, broad view of the subject from virtually the entire gamut of the social sciences. But if as review essays they share some

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strengths, they also share some faults. They are far too timid in their exploration of the more technical, yet fundamental, findings from the social and public choice and game theory literatures. However, the volume is still pedagogically useful at the advanced undergraduate, graduate, and professional level.

*Justice in Social Relations*, edited—or perhaps not edited—by a team or collective that includes Ronald Cohen, is quite a different kettle of fish. In vain do we search for the formula uniting these essays. They do not revolve around a common conception of justice. For some justice is retribution, for others it is distribution, and for others it is fairness. Our editing collective is quite frank as to what happened here: as they put it, "From July 16 through July 21, 1984 a group of American and West German scholars met . . . to discuss their common work on the topic of justice in social relations. . . . To give the participants the freedom to present their ideas in the most appropriate way, we, the conference organizers and the editors of this volume gave them complete control over the form and substance of their presentations" (p. vii). As political scientists we know what giving control to an unstructured community without disciplined parties yields: a bloated and formless barrel of pork. Most publishers do not enjoy the prospect of such a publication. Indeed, one could imagine that Plenum agreed to this volume in exchange for the other. Although it would be a rare reader who would be interested in this collection as a whole, there are some redeeming essays. Some of the essays in *Justice: Views from the Social Sciences* struck me as having unusual strengths, others as illustrating the central weakness of the volume. In what I applaud as a proper opening move, the volume's first substantive essay, by Buchanan and Mathieu, deals with the philosophical view. Although it is a wide-ranging and highly informative account of the philosophical development of conceptions of justice and the controversies about them, it betrays a lack of familiarity with some of the technical literature in the area. For example, the essay juxtaposes the utilitarians to Rawls. The authors do not notice that rather than being two discrete options, these positions can be viewed as two points in a continuous space of options. This has been suggested by numerous scholars in-

cluding Donald Wittman, Roger E. Howe, and John E. Roemer. Another shortcoming is the authors' failure to make interpersonal comparisons in order to form judgements regarding distributive justice. This is a central point in Amartya K. Sen's important, yet too often overlooked, works on redistributive justice.

The same two shortcomings figure in the essay by Worland, "Economics and Justice." It, too, attempts to deal with justice in distribution and fails to go beyond the marginalist analysis of classical economics because it, too, does not consider the relations between interpersonal comparisons and justice. Indeed, discussion of Sen's work and work of others such as Hal R. Varian is sorely missing from all these essays. The pattern is quite obvious: there is no serious discussion of technical literature. The technical nature of some of the literature is all the more reason to ensure that it is properly reviewed and discussed in a volume such as this.

Arthur DiQuattro's essay regarding political science's view of justice develops the basis of liberalism and then asks, "What does liberalism have to say about justice in distribution?" DiQuattro makes many good points. He discusses liberalism as based on the status and centrality of the individual and shows that this does not have to generate market inequalities. But he fails to cite the major findings of welfare economics, which go directly to that point. Further, much of the documented conflict between liberties and distributive justice is ignored, for example, the proof by Charles R. Plott that serial dictatorship underlies a popular interpretation of the Rawlsian notion of distributive justice. These gaps seem to reflect DiQuattro's general lack of acquaintance with the more technical (and perhaps fundamental) work in the area, rather than its innate dismissability.

One essay is a pleasant exception to this deficiency. "Public Policy and Justice," by Karol Soltan, covers a number of technical points very well. Indeed, he makes quite casually some technical points that are sure to interest many. For example, he notes that utilitarianism without interpersonal comparisons of utility becomes reduced to the Pareto condition. Soltan's further critique of utilitarianism as ambiguously based on the utility of individuals and hence on their values (including the value of altruism) is instructive. It

certainly leads him to another technical follow-up that is correct and important: that if there is a multitude of values, we must consider the trade-offs between them. But his is the only discussion of these technical points (which are important in defining the frontiers of the research on the subject in the volume).

The discussion of experimental work is a surprising strong point in these essays. It is least surprising in Furby's "Psychology and Justice." Her critique of experimental designs is a joy to read. But Soltan and others also spend some time discussing experiments in a useful fashion.

The collectively unedited *Justice in Social Relations* is also rich in experimental discussions and devoid of serious *theoretical* (in the formal, game-theoretic sense) argument. This is even more unexpected in a volume focusing on the work of psychologists. But we can still easily identify five quite informative essays focused on experiments.

Deutsch's "Cooperation, Conflict, and Justice" summarizes the considerable experimental research that has been done on the psychological orientation toward distributive justice. In this review Deutsch identifies in stark relief the big findings: people like proportional rewards more when there is some group identification (p. 4); unless the tasks are very individualized, as well as the context, performance is not found to be a function of incentives (pp. 8-9). A real pioneer in the area, especially with regard to prisoner dilemma experiments, Deutsch's outline and interpretation of historical developments is instructive to follow. This essay is a serious addition to the perspectives we have on the development of the literature.

Cook and Hegtvedt's "Justice In Social Relations" looks at power and attitudes toward justice in relations such as exchange relations. They review a number of three and four-person experiments; but as the experiments are limited to prisoner's dilemmas, we may legitimately wonder whether their findings can be generalized to justice in exchange relations. As in *Justice*, the lack of familiarity with the relevant formal literature works to the authors' disadvantage. They could well have used Thomas Schwartz's argument that there is an instability inherent in exchange.

Another experimentally oriented essay of interest to political scientists is Messe, Hymes,

and MacCoun's, "Group Categorization and Distributive Justice." They consider how group identification mediates the application of distributive justice evaluations. This is currently a subject of interest to a number of political scientists, including R. Simmons and J. Orbell.

Perhaps the most fascinating essay for political purposes was that by Crosby, Burris, Censor, and MacKethan, "Two Rotten Apples Spoil the Justice Barrel." They describe how the perception of discrimination varies as a function of the number of clear-cut cases rather than the statistical pattern of discrimination. They show that a couple of clear-cut cases lead people to perceive discrimination. This leads to an excellent discussion of experimental design issues. The obvious political relevance for political organizing should not be lost on students of collective action.

Martin's "When Expectations and Justice Do Not Coincide: Blue Collar Visions of a Just World" (pp. 317-36) reports on an experiment to discover how workers regard the appropriateness of pay scales. She observes that the disadvantaged workers are able to perceive not only the small intraclass injustices but also the large (interclass) ones (p. 324). She shows that the evaluation of these injustices is a function of specific psychological characteristics of the subjects, such as how much they feel empowered over their own fate in life and over monetary matters in their job. Thus her conclusion is the nonsurprising notion that views of distributive fairness depend upon one's experience and cultural beliefs (p. 333). I was not convinced. I was far more struck by how *little* actual variance in fair pay estimates there was between groups (p. 328).

There are other essays of more than passing interest. The essay by Tyler asks how people evaluate the performance of political leaders. Is evaluation best understood as a function of outcomes or of procedures? If outcomes, is it the value of outcomes or these values in contrast to expectations (p. 301)? In all cases, is it fairness or some other characteristic, such as general level of benefits, that matters? Are fairness judgements merely a reflection of self-interest? Tyler looks at these questions using telephone survey data. His findings are not earthshaking, but the essay is of clear relevance.

Most of the remaining essays in this volume



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are disappointingly thin or weak in their contributions. In this category I would certainly put the theoretical discussions of Lamm and by Cohen. These essays are not organized around any explicit theoretical idea; and although they review considerable literature in sweeping terms, they do not generate any new results. Other essays, such as those by Mikula and Greenberg, do not justify the space they use and could have well been edited or eliminated.

JOE OPPENHEIMER

*University of Maryland*

**Politics and Ambiguity.** By William E. Connolly (Madison: University of Wisconsin Press, 1987. xii, 168p. \$25.00).

In this work William Connolly draws heavily on the ideas of Michel Foucault to examine the interplay between forms of social control and democratic politics. Connolly contends that the freedom, dissent, and variety required for a healthy democracy are continually threatened by "disciplinary pressures" produced by social institutions. At the same time, he recognizes that democracy requires norms and authority that may assume the characteristics of disciplinary controls. His resolution of this dilemma is a politics that candidly confronts the essential ambiguity in democratic authority and enables an aware citizenry to make informed choices. Although 7 of the 10 chapters in the book have previously appeared elsewhere, the work holds together well.

Beginning with the declaration that democracy is "the pride and hope of modernity" (p. 3), Connolly first examines the forms of disciplinary control that endanger it. The next three chapters focus on Connolly's concern that the Democratic party and proponents of the welfare state do not understand the causes of working class alienation. He traces much of this misunderstanding to a failure to recognize that the social controls accompanying and supporting the United States' infatuation with the goal of economic growth have engendered a general pessimism about the future that has led to civic disaffection. He is particularly concerned that the movement toward reindustrialization will foster an increase in disciplinary restraints in society. Connolly returns to this topic in chapter 7, where he describes how

"visible" and "unconscious contrivances of social control" encourage citizen conformity and a retreat into privatism (p. 100).

In chapters 5, 6, and 8, Connolly analyzes the ideas of Jürgen Habermas, the thought of Richard Rorty, and the failings of liberalism. Habermas has understood the importance of threats to political legitimacy posed by modern society. But in offering the "ideal of discursive rationality" as the answer to these challenges, Habermas has foreclosed other approaches and may have distanced himself "from the concrete imperatives of our political economy" (p. 71). One would think that Rorty, who is examined in chapter 8, had avoided the error of positing an exclusive approach to social theory; but Connolly contends that Rorty uses "rhetorical strategies" to disparage alternative positions and to assert the primacy of his priorities (p. 123). In chapter 6, Connolly details the conceptual flaws of liberalism that Foucault did so much to expose. While trumpeting the liberal values of productivity and individual rights, U.S. institutions have used subtle but effective disciplines to harness individuals into the productive mode. Yet here Connolly draws a distinction between his position and that of Foucault. Unlike the latter, Connolly clearly expresses the view that democratic society must accept the need for norms, even though they may have subjugating and excluding effects.

In chapter 9, Connolly returns to the dilemma posed by authority in the democratic state. For him, even a democratically acceptable system of authoritative norms and standards constitutes an ambiguous achievement because it "excludes and denigrates that which does not fit" (p. 138). In the final chapter, Connolly examines the role of discourse in establishing and reinforcing norms.

In these essays Connolly has made an important contribution to an exceedingly complex issue in modern political thought. Connolly suggests that there are no final or easy answers to the democratic relationship between authority and freedom. For him the route to a democratic future lies in creating a politics that leaves "slack" for alternative ideas. Political discourse must be framed in terms that acknowledge the continuing existence of others and other possibilities. One might demur that the ambiguity that Connolly offers is an exceedingly heavy burden for the

citizen—and society—to shoulder. But the essentially tentative and ambiguous character of the contemporary democratic condition is one that needed to be stated, and Connolly has done that well.

ROBERT HEINEMAN

*Alfred University*

**The New Conservatism: The Counter-Revolution in Political, Economic, and Social Thought.** By David G. Green (New York: St. Martin's, 1987. xi, 238p. \$32.50).

David Green's treatment of the "new conservatism" provides a good sense of the way that conservatives who identify with classical liberalism view their theoretical origins and justify policy recommendations. As a research fellow at the Institute for Economic Affairs in London as well as an author of studies on the political economy of social services, Green is involved in the conservative movement. The book is directed to the "non-specialist reader," so its scholarly limitations are acknowledged.

In the introduction, Green defines the new conservatism as simply the old classical liberalism enhanced by a selection of insights from individualist schools of thought that prefer the marketplace to the state. He excludes from consideration the traditionalist conservatism of Burke and Scruton because it values the state too highly, and the neoconservatism of former liberals and leftists on the grounds that "most of them began as collectivists and despite modifying their views remain collectivistic" (p. 4). Thus Green leaves aside the advocates of traditional morality among modern conservatives and bypasses the libertarian-traditionalist split.

Part 1 sketches the intellectual background: the liberal tradition (chap. 1), anarcho-capitalism (chap. 2), the Chicago school (chap. 3), the public choice school (chap. 4), and the Austrian school (chap. 5). In the necessary condensation, what is left out is revealing: Locke emerges as a "critical liberal" skeptical of allowing much latitude for common decisions (pp. 20-21). This tradition is favored by Green over continental "egoistic liberalism" with which Rousseau and, later, T. H. Green are identified. The appropriation of Locke

(and indeed the term *liberalism* itself) for the new conservatism proceeds from the development, then, of a direct line from Locke through Smith, Burke, and Hume. There is nothing of Locke's concern for accumulation that is not justified by labor. Mill is barely mentioned.

The influence of the various schools is dealt with in a succinct and informative, though generally uncritical, manner. Public choice theory is effectively presented, though in the familiar reductionist terms of self-interest versus altruism. Chapter 5 offers an insightful explanation of the relation between Hayek's epistemological skepticism and his ambivalent attitude toward the role of authoritative institutions.

In part 2, Green discusses policy implications of the "new liberalism" (the phrase *new conservatism* rarely appears). United States readers may be interested in the variety and subtlety of policies proposed by libertarian conservatives in England. In his final chapter, Green argues that the new conservatism is compassionate and involves a "safety net" for the poor. He observes that "a free society is only supportable if its adherents advocate a moral code which among other things constrains the pursuit of selfish gain and requires the successful to help the less fortunate" (p. 218). Given this view, perhaps traditional conservatism is not as irrelevant as the introduction states—nor the values of the Right so sharply distinct from those of the Left, as the author elsewhere suggests they are.

Specialists will find this a book to argue with; upper-level students will benefit from the suggestions for further reading, while remaining somewhat puzzled by the typological framework. Though the writing is clear enough, the chapter introductions and summaries are rather disjointed and some generalizations overshoot the mark, as in "the market promotes universal service" (p. 220). Nevertheless, the book amounts to an interesting map of a significant quadrant of contemporary political thought.

KENNETH R. HOOVER

*University of Wisconsin, Parkside*

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**Ethics, Economics, and the State.** By Alan P. Hamlin. (New York: St. Martin's, 1986. x, 198p. \$32.50).

Alan Hamlin's *Ethics, Economics, and the State* explores the relationship between rationality, ethics, and the formation and justification of government. It does so in a way that should be familiar to anyone who has followed the development of public choice from Buchanan and Tullock's pathbreaking *Calculus of Consent* to more recent work by others associated with the movement, work that attempts to reach beyond the confines of economics to moral and political philosophy by applying the traditional tools of the economist, principally the notion of rational economic man. What distinguishes Hamlin from others, however, are his detailed analysis of the standard economist's model and the deep faults that he ultimately finds with it.

Hamlin begins by making about the relationship between the individual and the state, three claims fraught with "uncertainties and ambiguities," which the remainder of the book is dedicated to clarifying: (1) that individually rational behavior may or may not produce collectively desirable results; (2) if not, it may be possible to improve upon the situation by constraining individual behavior within rules; and (3) that these rules constitute the state. Stating these propositions leads to deeper, philosophical concerns such as, What constitutes individual rationality? What is collectively desirable? How are these two concerns linked? How is the institutional structure of the state affected by views of rationality and ethics? and How might states vary if grounded on different ethical stances? Hamlin's recurring theme throughout the book is the tension between rationality and ethics, and how this tension is reflected in the philosophical debate between advocates of differing ethical-political stances over the relationship between the individual and the community.

The exploration of this theme begins with an extended discussion of rationality that takes as its starting point the neoclassical view of rational, economic man. He is, first off, a utility maximizer. But Hamlin provides a fuller description of what economists mean by rational, economic man: he exhibits consistency and efficiency of choice, ends-rationality, and belief-rationality; that is, he is an "omniscient,

calculating, egotistical maximizer." This view, Hamlin concludes, has major defects that have been frequently explored. Imperfect knowledge, bounded rationality, and weakness of will, combined with the exclusion of altruistic motivations from the model due to unrealistic assumptions about human motivation as exclusively egoistic, make two alternative views of rationality more acceptable and allow for the possibility that a collective entity like the state could be more socially advantageous than individuals left to their own neoclassical devices and trapped in various plays of the prisoner's dilemma.

Under "imperfect rationality" there is room for norms or the law to serve to shore up the decisions of imperfectly rational individuals. Thus, they might precommit themselves to law as a strategy for enhancing their rationality. The "extended rationality" alternative recognizes that individuals hold other values in addition to self-interest and directs that individuals precommit themselves to law precisely to constrain themselves.

Hamlin next explores five currently prevalent ethical views: utilitarianism, libertarianism, liberalism (particularly its libertarian variant), contractarianism, and Marxism. Through extended discussions of Rawls, Nozick, Buchanan, and Roemer he attempts to show that there are certain similarities that extend from the libertarians to the Marxists that would allow all of these views to be represented as the result of a contractarian bargain. The objectives of both Marxian freedom and liberal rights can be expressed in the contractarian device. There is nothing in this device, he argues, that would necessitate the generation of an exclusively liberal ethic.

This claim to have found a device to equate competing ethical views even though they are thought to be incommensurable I find unconvincing. This social contract, drawn by imperfectly rational or extendedly rational individuals bargaining from an Archimedean point to rights and to obligation to a state, glosses over both the Marxists' fundamental anti-individualism and concern for the collective good over the individual and the libertarian liberals' contention that fundamental rights precede the state and form no part of any bargain about institutional arrangements that individuals might make. Hamlin claims for the Marxist that even if he is ultimately col-

lectivist he could be individualistic enough to go along with the social contract because it could generate Marxian results. But then again, it might not. The contract could generate *any* results. And that is why it is also ultimately unsatisfying to a Nozickian of the natural rights variety (whom Hamlin dismisses in favor of the loosely contractarian Nozick of the latter portion of *Anarchy, State, and Utopia*) because it places human convention—agreement—above fundamental rights. Thus it seems too much of a reach to attempt to subsume all of importance in contemporary political philosophy under the contractarian model.

Somewhat unsatisfying too is Hamlin's discussion of the state as generated by the three alternative views of rationality. It seems pretty clear that Hamlin prefers the "pluralistic" state—generated by the two alternative views of rationality and the contractarian device—to either the neoclassical state or the utopian state. By the terms in which he describes pluralism—as a state that allows everyone to hold his own particular ethical values while still retaining obligation to the government—he tips his hand. Yet, ostensibly the argument remains systematically neutral, more of a typology of alternative ethics and governments than a brief for any one of them. This leaves the reader with a more refined view of alternative visions of the state, but ultimately with dissatisfaction that no one of them has been forcefully argued for as the best. Given the contractarian device, however, such dissatisfaction seems inevitable.

Despite these reservations, the book is well worth reading and quite challenging throughout. It should be of special interest to an interdisciplinary audience of those in academia concerned with the relationship between the individual and the state.

ELLEN FRANKEL PAUL

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**Studies in the History and Theory of Capitalist Urbanization.** By David Harvey. Vol. 1, *The Urbanization of Capital*. Vol. 2, *Consciousness and the Urban Experience* (Baltimore: Johns Hopkins University Press, 1985. 239p. and 293p. \$25.00 and \$22.50).

In 1969, when he was a young expatriate assistant professor of geography at Johns Hopkins University, David Harvey published the influential methodological treatise, *Explanation in Geography*. In the conclusion he argued that the next turn in his discipline should be a move toward theory. In the ensuing four years Harvey found such a theory—Marxism—and an object for this theory within geography—the city. He first signified this discovery and this focus in *Social Justice and the City* (1973), a collection of essays that proved a foundational text for the recrudescence of Marxist-inspired work on the city.

Harvey has continued to be one of the most influential of scholars working in this vein. In *The Limits to Capital* (1982) he reconstructed Marx's theory of capitalist accumulation with space built in. Focusing on finance capital, rent, and the need for capitalism to find "spatial fixes" for its recurring crises, that book is the most successful attempt to date to improve Marxist economics by making urban space a constitutive element of the analysis.

The two-volume set under review (half of which consists of republications of journal articles, and half of new papers) deepens and extends this work and moves in new directions. Each volume, though distinct in emphasis, shares in the ambitious project of creating a program of urban research wholly within the Marxist tradition. Taking note of the plethora of types of work within urban studies as well as of diverse objects of analysis outside the usual ken of Marxism, Harvey nonetheless asserts the privileged status of Marxism as the master theoretical analysis of modernity because of the central causal place of capitalism. Reflecting on urban and academic fragmentation, Harvey asks, "Does this mean we have to abandon Marx for some eclectic mix of theoretical perspectives? Not at all. If capitalism persists as the dominant mode of production, then it is with the analysis of that mode of production that we have to start" (2: 263–64).

In pursuit of this project, Harvey extends his

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reach and that of Marxist theory quite far in these exceptionally stimulating, at times brilliant, at times disappointing books. The first volume, *The Urbanization of Capital*, focuses on capitalist accumulation, rent, finance capital, residential differentiation, urban politics, and city planning. The second, *Consciousness and the Urban Experience* devotes its longest section to the redevelopment of Paris between 1850 and 1870 under Haussmann's tutelage; it includes a small essay on the construction of the Basilica of Sacre-Coeur; and it contains three theoretical essays—on money, time, and space; on class conflict and the built environment; and on what Harvey calls urban consciousness—that underpin the two case studies.

In the first volume, as in his earlier work, Harvey stresses how the development of the city has helped resolve the crisis tendencies of the system and has facilitated an enhanced process of capital accumulation. In the second, more exploratory volume, he focuses on the creation within nineteenth- and twentieth-century cities of the "urbanization of consciousness," a shorthand for the formation of individual and group dispositions based not on class but on a multiplicity of identities of territory, gender, ethnicity, and race that depend on a broad conceptualization of "community" and are rooted more in the living spaces, than in the work spaces, of cities.

Capitalist cities have thus produced a new differentiated urban consciousness unanticipated by Marx. This development poses a basic challenge to an autarchic Marxism because its patterning of dispositions and collective action does not emerge in unmediated ways from the accumulation process, nor does it reflect traditional Marxist categories. The entire second volume wrestles with this dilemma within the self-imposed limitation of not moving outside the boundaries of a self-sufficient Marxism. Therein lies its considerable originality and achievement but also its shortcoming.

The book's most interesting propositions revolve around the idea that the city—and urban space—is a mediator between structure and agency within the capitalist mode of production. Capitalist development convened the spatial relationships that made the new urban consciousness possible, especially in the separation in space of work and residence.

Thus, capitalism creates the city; the city creates a consciousness that reflects its variegated spatial and relational realities; yet that very consciousness deflects attention away from the primal forces of the capitalist mode of production that underly the production and functioning of cities. For Harvey, the city is thus doubly essential to capitalism: it secures a "spatial fix" for its recurring economic crises, and it misshapes class struggle by deflecting it into secondary bases of conflict.

As Harvey's case studies demonstrate, this framework for urban analysis has the merit of incorporating history, economics, space, politics, culture, and collective action. This is no mean achievement. But it is marred by two clusters of limitations. The first concerns the failure to develop a perspective on class and group formation that transcends a simple kind of reflectionism based on the operative assumption of a necessary correspondence between structure and meaning; the second is the insistence that Marxism be self-sufficient.

In Harvey's analysis, urban identities ultimately reflect the antinomy of capital and labor and thus have a hidden potential of challenging capitalism. But in a more immediate sense, they deflect class consciousness. The formulation of class consciousness is an all-or-nothing matter, and the reduction of urban identities to the hidden essence of class struggle makes it difficult for Harvey to ask contingent and comparative research questions regarding how and under what conditions community-based mobilizations might achieve their anti-system potential and how capitalism as an economic order sets limits and exerts pressures on the formation of meaning, identity, and patterns of collective action for various groups and social classes. Rather than ask under what conditions class emerges as the unifying cognitive map for a working class in a capitalist city and why it is that different groups of working people are formed as different kinds of actors in very similar capitalist and urban settings, Harvey evades these issues by taking for granted an automatic reflection in consciousness of material reality—whether it be capitalist or urban. This renders comparative and historical work, including his own case studies, merely illustrative.

If limitations of social theory vitiate Harvey's attempt to link theory and history, so too does his pursuit of a closed Marxism. The

costs are most apparent in his flat and functionalist treatment of the state and its role in shaping the urban built environment. Harvey assumes what must be demonstrated: that the state has only a second-order coordinating role in capitalist development and that cities—even political capitals—are almost exclusively the spatial expressions of the logic of capital, with the state merely its instrument.

His conflation of the state and capitalism into a single, jumbo macrostructure eliminates in one stroke some of the most interesting questions about urban development and the form of the city, the role of planning, the expansion of governmental services, as well as class and group formation. Even the centerpieces of Harvey's own work—the analysis of land use, rent, and the adaptation of the urban environment to accommodate the accelerated pace of the circulation of capital—cannot possibly be developed without an elaboration of the independent interests and actions of local officials and government bureaucrats, who, together with lawyers, landlords, developers, and bankers are enmeshed in a politics of property. To be sure, at every moment there has been an intertwining of government and capital; but the story of urban development in the West in the past two centuries can be told only in caricature exclusively as a tale of capitalist requirements achieved with the aid of an always compliant state or as the story of actors exclusively bearing the "real" names of labor and capital.

IRA KATZNELSON

*New School for Social Research*

**The End of the State.** By Andrew Levine. (London: Verso, 1987). 198p. \$29.95, cloth; \$11.95, paper).

The keynote of this "essay in political philosophy" (p. 3) is provided by a quotation from Rousseau's *Social Contract*: "The fundamental problem of political philosophy . . . for which the social contract provides the solution" is "to find a form of association which defends and protects with all common forces the person and goods of each associate, and by means of which one, while uniting with all, nevertheless obeys only himself and remains as free as before" (p. 28). Autonomy, freedom,

and democracy provide Rousseau with a vision equivalent to Kant's "republic of ends," according to Levine. They also provide a framework that Levine uses to explain the Marxian notions of the end of the state and the development of a classless society.

This book is not simply an explication of Marx or Rousseau—Levine makes clear throughout that much of his work is "in the spirit" of these thinkers rather than adhering to the letter of what they wrote. This finely crafted essay attempts to show that Rousseau's vision of a free people living cooperatively through the exercise of its "general will" is not merely utopian nonsense but can "become a deliberate political objective" (p. 169). For this objective to be obtainable, however, Rousseau must be expanded beyond his own limited economic and historical framework through the incorporation of Marxian political themes, especially the concept of historical materialism.

Levine's discussion of exploitation in pre-socialist and socialist societies is interesting in its own right and is additionally important because it presents a challenge that Levine himself fails to meet. Levine interprets the movement from stage to stage within the historical materialist outline of history as "the successive elimination of forms of exploitation" (p. 118). Precapitalist societies are characterized by four types of exploitation: "control over persons, over alienable means of production, and over skills and organizational resources" (p. 119). Capitalism eliminates the ownership of persons but retains the other three forms of exploitation, while socialist societies retain "only skills and organizational exploitation." While the movement of history invokes a reduction in types of exploitation practiced, this "does not ensure that aggregate exploitation diminishes. Remaining forms of exploitation can expand to fill the void left by the elimination of the form of exploitation principally responsible for generating inequalities under its historical predecessor. Existing socialist societies appear to suffer acutely from this phenomenon" (p. 119).

Thus, unlike others writing in the Marxist tradition, Levine does not deny that exploitation is still possible in a socialist society. He does not see that this exploitation in itself is enough to prevent the vision of Rousseau and Marx from becoming reality, however. It is at

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this point that I believe Levine is too optimistic in his analysis. While admitting that "organizational exploitation looks insurmountable in existing socialist countries" (p. 149), he believes that under the right conditions such exploitation will itself wither away. He fails to see that the "class interest" of a socialist bureaucracy may be as great a hindrance to the development of a classless society as is the interest of the capitalist class.

Ultimately, according to Levine, society's ability to overcome the various forms of exploitation that it fosters will require "substantial modifications in human beings' dispositions and character" (p. 177). This transformation in man's nature, Levine suggests, can be brought about by the inculcation of civic virtue through political participation. "Commitment to this ideal depends," Levine admits, "on faith in democracy" (p. 178). Whether one shares Levine's faith in "the efficacy of democracy itself in transforming human nature" (p. 172), one can learn much about the nature of political philosophy and about the interconnections between Marxist and modern liberal thought, from a reading of this extremely well-written and elegant meditation.

STEVEN D. EALY

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### **Persons, Rights, and the Moral Community.**

By Loren Lomasky (New York: Oxford University Press, 1987. ix, 283p. \$24.95).

From its classic formulation by John Locke, liberalism viewed rights "negatively," that is, as the absence of interference by others and viewed the function of government as the protection of those rights. By the midnineteenth century, liberalism had, due largely to the writings of T. H. Green, come to view rights as "positive," that is, as claims to specific things such as food or safe working conditions, and the function of the state to supply these goods and services. The debate between the two liberalisms was reviewed in the 1970s with the publication of John Rawls's *Theory of Justice* and Robert Nozick's *Anarchy, State, and Utopia*. Rawls defended the modern wing of liberalism, Nozick the classical wing.

Loren Lomasky, in his *Persons, Rights, and the Moral Community*, places himself squarely in the classical tradition. Despite his criticism of the philosophical underpinnings of Nozick's analysis, *Persons, Rights, and the Moral Community* is a fitting companion volume to *Anarchy, State, and Utopia*. Although grounding his analysis in Lockean natural rights, Nozick makes no attempt to defend them. The focus of *Persons, Rights, and the Moral Community*, however, is precisely on the derivation and defense of those rights.

Lomasky's analysis is premised on the empirical observation that individuals pursue projects. Projects, according to the author, do more than explain actions, they explain lives. They make us the unique people that we are. But project pursuit presupposes moral space within which one can choose one's projects. Thus, basic rights are primarily claims to non-interference. They are "liberty rights," not "welfare rights."

Lomasky uses his view of individuals as project pursuers to mount an effective critique of Rawlsian liberalism and to present a challenging defense of the traditional liberal order. Rawls argues that choices made by individuals behind a "veil of ignorance" would be *fair* and that one of those choices would be the "difference principle," proscribing social or economic inequalities unless they benefit everyone. But, contends Lomasky, a "fairly-agreed upon distribution" has no moral weight unless the parties have the *right* to make distributive decisions. Rawls's response is that even if our possessions accurately reflect our talents, these talents are not rightfully ours because they are genetically accidental. And if these talents are not deserved, neither are our possessions. Since this makes the "natural distribution" morally arbitrary, it can be ignored in a collective decision. This argument, Lomasky believes, proves too much, for it renders *everything* that individuates people morally arbitrary. The result, he concludes, is "thoroughly illiberal."

Lomasky's defense of traditional liberalism is thoughtful and original. From the view of persons as project pursuers he concludes that although some groups—children and the handicapped, for example—possess "positive" or "welfare" rights, basic rights are primarily liberty rights. Need is not a sufficient condition for something to be a basic right. For example,

need cannot be a sufficient condition if what is needed cannot be provided by others on demand. Self-respect falls into this category. Moreover, one must distinguish between the need itself and the best way to supply the need. State interference can at times impede the provision of a good or service, reduce its quality, or generate side-effects that may more than offset the good done by the program.

The central problem for modern liberalism, argues Lomasky, is that because rights are moral claims against others, expanding welfare rights saddles people with expanding obligations, thereby jeopardizing the entire structure of liberty rights. But liberty rights must be paramount, for noninterference is a prerequisite for whatever projects one wishes to pursue.

Lomasky's extension of basic rights from project pursuers to such potential project pursuers as children and then to nonproject pursuers such as the severely mentally handicapped is worth noting, as is his distinction between children—who are rights claimants—and fetuses—which are not.

Lomasky's book does contain a serious tension. Given his rejection of an "impersonal standard of value," it is paradoxical that he should, in the last chapter, embrace the idea of a "preexistent impersonal value." Lomasky's concern is apparent. If nothing is objectively true or valuable, then it is difficult to argue that anything is morally impermissible, including interference with others. But if there are objective values, then some ends, some projects, are "better" than others and should be accorded priority. In coming down on the side of "objective values" Lomasky risks undermining the position he had so ably defended.

Some topics, such as the provision of public goods, should be treated in greater depth. But the prose is concise and the author's reverence for the panoply of the day-to-day concerns of flesh-and-blood people is refreshing. Lomasky's people are never placed behind counterfactual veils of ignorance but in families, clubs, and other social groups. They are presented not as abstract, autonomous agents making moral choices far removed from the woof-and-warp of our actual daily experiences but as real social beings whose choices are influenced by their unique personal histories and upbringings.

*Persons, Rights, and the Moral Community* is an important work. Even those disagreeing with its central thesis will find it challenging.

DAVID OSTERFELD

*St. Joseph's College*

**The Cloistered Virtue: Freedom of Speech and the Administration of Justice in the Western World.** By Barend van Niekerk. (New York: Praeger Publishers, 1987. xxxviii, 399p. \$49.95).

Justice should not be a cloistered virtue. That is the theme of the late Barend van Niekerk's work in comparative law. "It remains one of the strange ironies of the West," he writes, "that, despite its theoretical commitment to liberty, it has raised so many sanctions against free speech concerning the administration of justice—for advantages that are mostly so illusory, and a gain that is often so trifling and at a social cost that can be so crippling" (p. 44).

The genesis of the book, the final work of an outspoken advocate of civil liberties and professor of public law at the University of Natal, was Niekerk's own trials. Three times he was prosecuted for criticizing the judiciary. First, in 1970 he was tried for contempt of court for concluding, after he had conducted a survey of lawyers, that race was a "conscious and deliberate" factor in South Africa's use of the death penalty. Second, in 1971 he was indicted for saying that the Terrorism Act was "a negation of what any true lawyer would call justice." In the third trial, 1975, the minister of justice began a libel action after Niekerk opined that a decision to reprieve a white killer and execute his black accomplice demonstrated a "lack of concern for justice and the reputation of our law" (pp. xiii-xvi).

Niekerk argues that the same premises that justify freedom of speech in all other areas of society provide justification for free speech in the legal domain. These premises range from the importance of self-fulfillment for lawyers, who normally stifle their own critical comments, to the advancement of knowledge for society, which will benefit from truth's coming out. Freedom of speech in the administration of justice is also, Niekerk claims, a device for



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broadening democratic participation, for minimizing conflict and maximizing stability, for preventing the inefficacy and frequent counterproductivity of suppression, as well as for ensuring that justice be done.

Although set against the background of South African restrictions, the book reaches into all quarters of the West, especially the German, English, and U.S. legal systems, with some case studies and comments drawn from France, Italy, the Netherlands, Switzerland, Scandinavia, and the South American countries. The differences are instructive. The most draconian—no surprise here—is South Africa. Although Germany has strict professional sanctions against lawyers who engage in scandalization of judges, one of the freest presses and one of the most responsible publications, *Der Spiegel*, are found in Germany. Elitism characterizes the English practice across the board, from use of contempt of court to a detachment and self-imposed silence in legal, academic, and journalistic circles. Except for the bright spot presented by the U.S. system, Niekerk concludes that "it cannot truthfully be said that the legal free speech is thriving and blooming anywhere in the Western world outside the United States" (p. 210).

Television, especially in the United States, poses a need for an enforceable professional and judicial code, Niekerk acknowledges. He suggests three general considerations: the invasion of privacy, which can make television a modern pillory; the possibility of an appreciable risk of encumbering the search for truth and justice; and the need to maintain civilized standards of speech.

Barend van Kiekerk died at age forty-two in 1981, shortly after completing the manuscript. Gilbert Marcus, in the afterword, brings developments up to 1987; John Dugard, in a foreword, tells us about the life and career of Niekerk, and Alan Paton presents a eulogy.

Although the book has an inadequate index and presents the reader with the frustration of having to keep one finger in the notes section in order to keep up with the running commentary and with details that would have been more conveniently placed in the text, Niekerk has given us an important work in comparative law. We need more like his. Others will have to write them now.

RON CHRISTENSON

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**Property Rights and Eminent Domain.** By Ellen Frankel Paul (New Brunswick, NJ: Transaction Books, 1987. 276p. \$24.95).

By writing this book Ellen Frankel Paul has probably solidified her credentials as a person worthy of her previous appointment to the Reagan administration, having served as U.S. representative to the UN Commission for Social Development in 1983. One should perhaps not be surprised by this text, therefore, which purports to be a critique of all regulation of property rights and which rejects all but the most limited government exercise of eminent domain.

Paul begins the book by claiming that the power of eminent domain has emerged and developed without express grants of authority in the Constitution and that the police power has been applied in the context of extraordinarily vague grants of power to protect the health, safety, morals, and general welfare. Additionally, Paul argues that without well-articulated and logically unassailable theories of property rights, it is likely that government regulation of property will become and remain capricious, ad hoc, inconsistent, and subject to capture by special interests. Paul then proceeds in several sections of the book to claim that environmentalists have been the most important contemporary influence on the use of the powers of eminent domain and the police power. In several chapters, Paul discusses many (far too many) cases to illustrate the well-known fact that the courts have been confused about the regulation of property rights. With the exception of the so-called *Lochner* era, during which the federal courts presumably operated according to correct principles, Paul sees only caprice and illogic in court decisions respecting the scope and application of the police power and eminent domain.

Paul devotes nearly 70% of the book to making these points. The rest of the book contains Paul's argument that only a natural rights justification will provide a basis for halting the continued erosion of property rights. Although reasonable people might disagree with such an approach to remedying some of the chaos in the law of police power regulation and eminent domain, it would have been interesting to see what Paul developed. However, the little space she devotes to a natural rights argument for the protection of property rights is woefully deficient. Indeed, a better expres-

sion of such a viewpoint can be found in Bernard Siegan's equally conservative *Economic Liberties and the Constitution* (1980).

Like many other more reasonable observers of government regulation and local planning, Paul raises legitimate questions (for the wrong reasons) regarding the fair treatment of property owners. Moreover, government regulation of property does pose questions regarding the maintenance of appropriate boundaries for government exercise of the police power and its power to condemn property. However, Paul fritters away the opportunity to advance the discussion to a useful arena of discourse. Instead, her book is too often a polemic, designed to exploit the many individual cases of government abuse of property rights for the purpose of pressing her case for minimal government. While the book is largely a propaganda piece, it is nevertheless an archetype of work by contemporary conservative thinkers seeking to revive what they believe was the halcyon period of government deference to property rights, the so-called *Lochner* period of jurisprudence, when, it is commonly believed, substantive due process accorded constitutional protection against government regulation of property rights.

The use of eminent domain to take property from private individuals and then have publicly sanctioned transfers of title to other private individuals for their personal gain creates much potential for abuse and unfairness. The extension of police power regulation to areas with very subjective criteria for harm and public welfare (e.g., aesthetics, historic preservation, and adult entertainment) is very hard to administer consistently, and the potential for takings of property under the guise of police power regulation has become increasingly problematic.

While she is justified in her criticism of these states of affairs, most of Ellen Frankel Paul's assessment of the causes of these difficulties and her recommendations for solving them are wrongheaded. She gives far too much weight to the role of environmentalists in expanding the police power or creating conditions for abuse of eminent domain. After all, it was not the environmental movement that pressed for the adoption of the "public benefit" criterion over the "public use" criterion in the exercise of the police power. An alliance of development interests is responsible for that one. It was not

environmentalists that pressed for state government delegation to local redevelopment agencies of the power to condemn property as blighted under the loosest of criteria. Again, one has the business and development community to thank, since it sought these government powers to be used to reduce their costs of acquiring and clearing property. The argument of many local governments today, who condemn private property and transfer it to other private owners on the grounds that the new owners will use the property in more profitable fashion, thereby generating local revenues for the community, was successfully pressed by business and development interests.

To think that zoning is the only way localities have to restrict development is naive. Communities could affect development by utilizing their discretion regarding the timing and extension of public services. The result would be even more devastating on some property owners than police power regulation. Or would Paul contend that property owners have a constitutional right to publicly subsidized services? Moreover, Paul seems to ignore entirely the possibility that a great deal of property value comes from the governments she worries so much about. Indeed Paul, like others of her ilk, want to have things both ways. On the one hand, government should not diminish the value of property; on the other hand, government should do nothing to try to recover, on behalf of the community, the values that the community produces for property owners. For every property owner adversely affected by the actions of the community through its governing bodies, there is at least one property owner who benefits from various government actions. Of course, the investment in local development politics by those who benefit from government actions is far more visible and influential than the episodic involvements of those interested in restricting development.

Finally, while it is easy to concede that the current administration of the police power and eminent domain is deficient, the remedy proposed by Paul—the adoption of a constitutional commitment to property rights, based on a natural rights doctrine—is foolish. The cause of the problems is rooted not in the absence of a natural rights doctrine of property rights but instead in the nature of liberal, interest group politics. The efforts by individuals, groups,

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and businesses to affect the nature of the development process is inevitable in an urban-metropolitan society, where there is intense competition for locationally distributed advantages and disadvantages. Finally, judgments of who is entitled to the values that inhere in the exercise of property rights is extraordinarily muddled when such values do not emerge—as Paul suggests they do—from the efforts of individual property owners but from the intermingling of many different private and government actors.

It would be incorrect to deny that public regulation of property rights is flawed. However, Ellen Frankel Paul provides a weak case for the kind of wholesale abandonment of regulation that she advocates. Perhaps the recent U.S. Supreme Court case, *First English Evangelical Lutheran Church of Glendale v. County of Los Angeles, California* (107 S.Ct. 2378 [1987]) might provide more discipline for governments that are insensitive to fair treatment of property owners, as well as ameliorate some of Paul's angst regarding the diminution of property rights.

MAX NEIMAN

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**Reason, Revelation, and the Foundations of Political Philosophy.** By James V. Schall (Baton Rouge: Louisiana State University Press, 1987. vii, 254p. \$27.50).

Since World War II a recurrent theme among neoclassical and Thomistic political philosophers has been the importance of steadfast values for political inquiry. James Schall's most recent work is a noteworthy contribution to this end. Specifically, Schall pinpoints the vitality of the revelatory tradition to political philosophy both in terms of medieval philosophy's tempering of the classical framework of human activities and modern political philosophy's inability to replace the latter with a satisfactory political metaphysics.

The crux of Schall's position is that although the proper pursuit of *what man is* is in the practical or political realm, this activity raises questions that can be appropriately addressed only in terms of the *what is* disclosed by revelation in the theoretical realm. Consequently, there are limits to what political activity can

comprehend, but it cannot comprehend these very limits independent of a philosophy open to revelation. At the same time, however, this latter life of contemplation is cultivated through leisure provided by the polity.

This distinction between the practical and theoretical realms originates in Aristotle, but Schall finds Aristotle at fault to the extent that only the soul could transcend politics to engage in contemplation; therefore, individuals understood in terms of both body and soul were relegated to the finite happiness of politics. This shortcoming is resolved in Christian philosophy by God being a personalization of Aristotle's First Mover and through the notion of the bodily resurrection of the individual.

Augustine's City of God captures this theoretic order open to revelation; yet according to Schall it is left to Aquinas to restore the indigenous importance of the City of Man—politics. Beyond just emphasizing that the very limits of politics allow for contemplation, Schall transforms Aristotle's discussion of friendship as the quintessence of human interaction in terms of the Christian Incarnation: out of respect for individuals as *what they are* (p. 126) even God comes to interact with them as a man.

Schall's exposition of how medieval thought uncovers revelation as an intelligence aimed at philosophy without overwhelming either it or the political order is of great service to the discipline. All too often, especially among contemporary political philosophers, Augustine and Aquinas are rendered as footnotes to the political philosophy of virtue articulated by the ancient Greeks. Through careful exegesis Schall shows that although in *Natural Right and History* (1953) Leo Strauss rejects Christian philosophy as being too steeped in revealed theology (p. 219), he elsewhere intimates that revelation is directed at reason and morality; as philosophies that eschew *what is* are not (p. 233).

In his assessment of modernity, Schall, like Strauss, becomes predictable. From Machiavelli to the present, the integral relationship between distinct political and theoretical realms has been abandoned in favor of the identification of human intellect and will with *what is*. Instead of nature being approached as something given, it is now treated as something formable through science. Any deference to a

reality beyond humanity must be surmounted so as to emancipate man. Therefore, personal immortality as purported by revelation can only find significance in the human collectivities that transcend finite human beings (pp. 145-147).

These modern developments echo, in part, for Schall, the Stoic equation of man with *what is* in post-Aristotelian antiquity. However, the modern "anthropological metaphysics" (p. 201) moves beyond abstractions that merely reconcile the self with the world to the actual implementation of theoretical designs in the political realm. Machiavelli's Prince, Hobbes's Leviathan, Rousseau's General Will, Kant's Categorical Imperative, Hegel's Absolute Spirit, Marx's "Generic Man" (p. 199), and Nietzsche's Superman perpetrate a politics that has no bounds and hence must founder.

Curiously, Schall omits liberalism in his treatment of "the aberrations of the modern project" (p. 223). He does mention "the acceptable elements in modern liberal philosophy" (p. 223), but he never deliberates upon what they are, especially in the U.S. context. In turn, the modern thinkers he does dwell upon are essentially painted with the same broad brush and his consideration of twentieth-century thinkers is relegated to those more attuned to the revelation tradition—Strauss, Eric Voegelin, and Hannah Arendt. Arendt's case, though, is instructive in that while her political philosophy is congenial to Schall's position, her portrait of the theoretical order is much more indebted to Kant.

Could it be, then, that particular modern political philosophers are open to Schall's metaphysical concerns but do not articulate them within his framework? As Arendt points out, it is "not that the old questions which are coeval with the appearance of men on earth have become 'meaningless,' but that the way they were framed and answered has lost plausibility" (*The Life of the Mind: One/Thinking* [1978], 10). Frankly, one wishes Schall would show the same subtle sensitivity to the moderns that he does in overcoming his differences with Strauss. Otherwise, his conclusion—reminiscent of Alasdair MacIntyre's proposition, "Nietzsche or Aristotle" (*After Virtue* [1981], 103)—becomes "Nietzsche or Aquinas." If this opposition could be overcome, Schall's clarity and insight regarding the revelatory tradition would be essential in an

authentic dialogue among contemporary political philosophers on how the foundational metaphysical questions should be framed. Why not Nietzsche and Aquinas?

JOHN FRANCIS BURKE

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**Law, Liberalism, and Free Speech.** By D. F. B. Tucker (Totowa, NJ: Rowman & Littlefield, 1986. viii, 212p. \$32.50, cloth; \$13.50, paper).

**Toleration and the Constitution.** By David A. J. Richards (New York: Oxford University Press, 1986. xvii, 348p. \$29.95).

**The Tolerant Society.** By Lee Bollinger (New York: Oxford University Press, 1986. viii, 295p. \$19.95).

These three books are united by their authors' shared commitment to a common underlying value: the idea of equal respect and mutual tolerance. In pursuit of this value, each author initially delineates and deems insufficient various established frameworks for constitutional analysis. He then devises and sets forth a new framework that he deems superior. Two of the books focus exclusively on speech issues; one treats speech and some additional concerns.

Tucker's book is drawn from the work of Rawls and Dworkin. It is an explicit effort to find a theory of free speech and judicial review that takes rights seriously. Tucker begins by pointing out the inadequacies in what he describes as functionalist-utilitarian approaches to free speech. These are approaches that attempt to test free speech against its functional efficacy in pursuit of some goal such as democratic accountability. Through analysis of several free speech issues, he attempts to demonstrate the advantages of an approach that derives rights from Rawlsian analysis. Tucker's sense is that his results accommodate intuitive judgments as least as well as functionalism while providing a better understanding of why we require governments to respect freedom of speech.

Many readers may not find his overall effort persuasive, feeling that he moves rather quickly over points that are crucial to his theory. Nevertheless, *Law, Liberalism, and Free*

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*Speech* is a serious work offering something useful to the interested reader.

*Toleration and the Constitution* is a related effort, self-described as an attempt to develop a broad theory of constitutional interpretation to illuminate the connection between our constitutional traditions and the larger cultural traditions that they reflect. The author seeks to utilize a deep understanding of history and deploy an explicit democratic political theory. Richards rejects Berger's original intentionalism and Ely's proceduralism for what he describes as a contractarian approach, one that grows out of cultural and moral traditions and calls for observance, by the state, of predictable and orderly constraints to acknowledge and express the dignity of persons as free, rational, and equal. He argues that an approach that emphasizes the abstract intentions of the Constitution's clauses rather than the expressed intentions of its framers is more consistent with the imputation of reasonable and coherent purposes to the historical understanding of the Constitution.

Holding that constitutional analysis must be informed by such broad historical understanding and by "better" philosophy, Richards attempts to show how this can be done, through his discussions of religion and speech and privacy issues. It is a very tall order and I believe it fair to say that those unsympathetic to the ongoing effort to ground constitutional analysis in rights-based philosophical approaches will not be persuaded, while supporters will applaud the book and find it a helpful contribution.

Like Tucker, Lee Bollinger in *The Tolerant Society* finds established functional rationales inadequate. But instead of rejecting functional analysis for a rights-based approach, he seeks deeper functional understanding. Bollinger identifies two conventional rationales for free speech that he believes fail to offer a satisfactory grounding for it. The first of these he characterizes as the classical model, where truth is produced through the clash of ideas and each of us gains satisfaction through self-expression. Bollinger finds it naive in many ways, most notably for its failure to deal adequately with the problem of extremist speech. The second rationale he terms the *fortress model*; it sees government as a force that cannot be trusted. This model troubles him because it introduces into our public institu-

tions an "unfortunate manipulative frame of mind, a warfare mentality."

After extensive and thoughtful analysis, Bollinger concludes that the United States tolerates extremist speech because coming to terms with it teaches each of us tolerance and self-control. He argues that this tolerance benefits democratic society by symbolically reinforcing our need to resist the ever-present tendency to intolerance. Although Bollinger may be correct in asserting that he has identified an insufficiently appreciated social good that flows from our approach to free speech, particularly when the matter is approached on the symbolic level, many observers will probably conclude that the importance of the insight is overstated and the significance of the more traditional rationales underappreciated.

And Bollinger arguably fails to subject his own thesis to the same tests he applies to others. For example, he sees the classical model as naive and unrealistic, arguing that while personal experience supports the idea that open discussion leads to truth, it does not support either the idea that extremist speech can lead to truth or that such speech is harmless. But he does not really consider whether significant support can be found in personal experience for his claim that free speech teaches a general democratic self-control. Again, Bollinger finds the fortress model to be sophisticated but manipulative. But many might apply the same description to his model of a society that tolerates worthless and harmful speech in order to practice exercising self-control over its own intolerant tendencies.

While all three books reflect learned and commendable efforts to think deeply about constitutional theory, only the Bollinger book's creative elements manage to transcend the increasingly apparent limitations and even circularity of well-established theoretical debates. Its mode of analysis and some of the insights that accompany it—even more than the specific outcome of the effort—make it the most worthwhile reading of three worthwhile books.

FREDERICK P. LEWIS

*University of Lowell*

**Tranquillitas Ordinis: The Present Failure and Future Promise of American Catholic Thought on War and Peace.** By George Weigel. (New York: Oxford University Press, 1987. x, 489p. \$27.50).

**The Nuclear Dilemma and the Just War Tradition.** Edited by William V. O'Brien and John Langan (Lexington, MA: D. C. Heath, 1986. viii, 257p. \$25.00).

In quite different ways, both these volumes are concerned with whether and how traditional theory about justice and war can be applied to the monumental potentials of modern warfare. Both also come at this issue from a Catholic perspective and both are in crucial respects prompted by the U.S. Catholic bishops' 1983 pastoral letter on nuclear policy. But while the O'Brien-Langan volume is a collection of papers done by eight authors and naturally somewhat short on synthesis, Weigel's book is an unusually integrated and comprehensive overview that sustains a high level of analytic power throughout.

I do not mean to depreciate the O'Brien-Langan effort by this comparison. It is a very useful source for anyone who wants to get quickly up to date on much important contemporary thinking on just war theory. By design it features only authors who make the fundamental judgment that war under some conditions is a just act and thereby excludes pacifists. The reason for this is that pacifism, since it precludes war by definition, is not capable of asking *when* war might justly be done nor *how* it can be justly conducted. The *when* question is traditionally answered by value assessments made by those with authority to judge: What is the nature of the threat to us (or to others)? What do we have (or what are we) to be defended? What will result from a failure to defend? What will result from defending? These are essentially questions of relative and proportionate values assigned to the various issues at stake in a war-threatening situation. If the judgment is that the stakes are such as to justify war making, then the ethical focus shifts to *how* war ought to be conducted. The principal traditional answers are: proportionately (i.e., with intent to win but with limited force) and discriminately (i.e., discriminating between combatants and noncombatants and, insofar as possible, granting the latter immunity from damage).

The O'Brien-Langan contributors share a judgment that the relative human freedom achieved in democratic politics is a value meriting defense. But they divide on other issues, two of which are particular problems for some contributors. First, the principle of discrimination is sometimes referred to as if it had an almost fixed or foreknowable character. The obvious—one may even say absolute—desirability of not harming noncombatants, if thought of in such fixed, numerical terms is a hard fit for nuclear war, which, no matter how carefully fought, would kill large numbers—and, if escalated, could slay untold numbers—of noncombatants. It is thinking in that fixed framework, presumably, that prompted one of the authors (Walzer) to state that the theory of just war is refuted by the advent of nuclear weapons. But if the principle of discrimination is seen exactly as a principle—a guiding value that, like all values, must be synthesized in moral action by proportionality and prudence—then the advent of nuclears, while dramatically raising the potential ruin of any conflict, does not in and of itself refute *anything*, let alone reason and its common-sense application to the *whens* and *hows* of war.

A second area of important, though often only implicit, dispute is over the role of the Catholic church in discussions of strategic policy in a democratic society. It is perhaps understandable that church leaders would imagine some special standing in society empowering them to instruct government itself—even thunder to it. But even if understandable, it is unseemly and unrealistic. A Great Britain or United States has long since made a fundamental choice of how authority is ordered and how government is legitimized, and that choice does not give any church hierarchy a stand-alone political capacity. Rather, it has a capacity to teach principles to any who will listen, primarily its communicants. If those principles include politically pertinent values, then the communicants—believers-as-citizens, whether clerical or lay—have the duty and the right to bring those values to bear in the political life of the nation, just as any citizen may bring values derived from any source. Langan describes a format of debate on nuclear policy that seems to imagine a more direct role for a hierarchical church (pp. 7-8) and in so doing, at one and the same time errantly diminishes,

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in my view, the believer-as-citizen and falsely exalts hierarchical potential.

Scripture and Catholic tradition contain only a few titanic principles about war. There is nothing that prevents bishops or anyone else from trying to master more detailed and particularistic issues, of course. But as Rome made clear to the U.S. bishops in January 1983, when leaving the safe harbor of central religious principles, church leaders must acknowledge diminished authority and the danger of divisiveness within the community of believers. Hollenbach seems not to know that this was the central problem of the bishops' pastoral through its first two drafts (p. 26). Whole moral analysis of, and response to, important issues requires going from principle to particular, of course. But that does not mean a church leadership qua church leadership must so go. And if it does, it must recognize the required modesty it should exhibit, as all should in problematic areas.

Despite a few problems such as these, each chapter in the O'Brien-Langan book helps define and explain crucial issues; and several are outstanding. James Turner Johnson's discussion of those things worth defending, even with nuclear weapons, is especially thoughtful.

The title of Weigel's *Tranquillitas Ordinis* refers to that tranquillity or order that was for Augustine the state's reason for being, in pursuit of which war might justly be made. But as previously noted, customary thought about just war has been strained by the advent of nuclear. Part of Weigel's contribution in this study is to establish that if the price of war has escalated hugely because of new armaments, the price of bowing to aggression has escalated no less because of the rise of totalitarian regimes. While he ascribes more of this latter phenomenon to *new* mentalities than would some—some would say totalitarian mentality can be seen throughout history but only lately in full concert with the technical capacity to implement its appetites—there is no doubt about its novelty, its importance, its *pertinence* to judging whether war remains an acceptable tool in the modern age.

For Weigel, the central political objective of this age is to avoid both the catastrophe of nuclear war and the catastrophe of totalitarian oppression's further extension. And the central

problem for his book is to explore Catholic teaching and thought as a potential source of wisdom germane to the political objective. His three theses are that Catholic and U.S. Catholic tradition contained pertinent wisdom; that from Vatican II's end (1965) to now, U.S. Catholic leadership lost hold of that wisdom; and that reclamation is possible and could help the nation in maintaining both peace and freedom.

This is a powerful study in which the author has managed to sustain rigor and perceptiveness over a very broad terrain and over a copious literature that he commands deftly. The effect of these labors is greatly to substantiate the three theses noted above. Much hard-won customary wisdom has been set aside in favor of trendier religious fashions, and reclamation needs to be tried, whether finally successful or not.

It does seem to me, however, that the author would do well to emphasize some distinctions in his use of the term *Catholic* when thinking about just war theory and generally enlightened political understanding. What he regularly refers to as the Catholic deposit of political wisdom is not especially Catholic except in the historical sense; that is, it is not contained in, nor directly derived from, doctrine. (That is why the U.S. Church leadership could so easily fly from much of that tradition without fracturing doctrinal commitments.) Much substantial thought on justice in war has been done by Catholics, but not in any uniquely Catholic way. And certainly the paramount U.S. theorists on the subject have not been Catholic. My point is simple; one who thinks it is important to encourage clear thought on war and peace, justice and freedom wants to use the widest possible categories around which rational people can gather. *Catholic* is a narrowing category in this regard and somewhat misleading, as well.

Precisely because Weigel has written a tightly integrated study, I will not try to summarize it further. It richly deserves to be read and studied by those concerned with the interaction of religion and politics and the twin terrors of modern war and modern oppression.

QUENTIN L. QUADE

Marquette University

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AMERICAN  
POLITICS

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**Seeking Many Inventions: The Idea of Community in America.** By Philip Abbott (Knoxville: University of Tennessee Press, 1987. x, 214p. \$19.95).

Abbott begins this study by citing the allegation that U.S. citizens have rejected community in favor of individualism. On the whole he agrees with this view, which has been part of the stock in trade of U.S. social critics since Tocqueville's time. But he argues that U.S. citizens have also persistently longed for community—for "more viable and stable social relationships" (p. 20). The difficulty, as he sees it, is that we repeatedly seek to satisfy this longing through "instant communities" (p. 20) that fail to meet our needs. The temptation to find instant community blocks our every effort to rediscover the real thing (p. 149).

Abbott attempts to support his thesis through discussions of five inventions to which U.S. citizens have made major contributions: the telephone, the penitentiary, the revival camp, the motel, and the pieced quilt. His focus in these discussions is not upon the scientific or technical but upon the aspect of social invention connected with each. He argues that all these inventions are pervaded by an air of "intimate anonymity" (p. 165), a simultaneous longing for and resistance to community as different from Tönnies' *Gemeinschaft* as it is from the contractual social relations to which that conception of community is usually contrasted. Because the relationships made possible by these inventions are voluntary (penitentiaries aside, presumably), the communities to which they give rise are exceedingly fragile, and therein lies the problem (p. 168).

What is the solution? First, "social space" must be won by being "wrestled and torn away from economic and political institutions" (p. 169). Abbott shares the distaste for bureaucracy that has emerged in recent years as a prevalent feature in U.S. culture. Hence he regards the welfare state and the market as equally unsuitable means for the restoration of

community. To recapture social space apparently means to make more room for voluntary associations of the kind described by Tocqueville with such curiosity and praise.

Second, Abbott argues that the future agenda of liberalism should be dominated by the creation of new communities with "purpose and commitment" (p. 172). U.S. citizens have always demonstrated remarkable energy as social inventors, but in the past their energies have been misdirected. By turning this energy toward the building of "forms of human association that capture a sense of moral cohesion, mutual aid, and intimacy" (p. ix) U.S. citizens can bring the promise of their immensely inventive energies to fruition.

One difficulty with Abbott's argument is that the relationship between his diagnosis and the evidence he offers is loose. The notion that U.S. citizens have a need for community that has long gone unfulfilled appears to be a premise rather than a finding of his study. His discussions of various U.S. social inventions are interesting in themselves, but they do not offer strong support for his thesis.

A more serious difficulty, however, lies in his conception of the kind of community liberals ought to go about creating in the future. As Abbott himself appears to recognize, the fragility of U.S. communities (or partial associations) stems largely from their voluntary character. Yet in calling for the wresting of social space away from the market and the welfare state Abbott appears to be asking for an expansion of the realm of voluntaristic social interactions. Would a "world rich in sociability" (p. 179) really emerge from such an expansion, as he suggests? I should think that any reader who accepts Abbott's thesis about the misdirectedness of past U.S. social inventiveness will look upon his remedies with a good deal of skepticism.

DAVID JOHNSTON

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## Book Reviews: American Politics

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**The Citizen's Presidency.** By Bruce Buchanan  
(Washington: Congressional Quarterly,  
1987. vii, 223p. \$9.95 paper).

A book that would "help citizens choose and judge presidents" would be very useful indeed and *Citizen's Presidency* is supposed to be such a handbook. The volume tries to answer the question, "What criteria for choosing presidents and judging their performance can yield adequate levels of public support while simultaneously holding presidents to account for their effectiveness in office?" The author claims that "anyone who reads this book seriously and carefully will be better equipped to choose and judge presidents." That is a tall order, as are other questions addressed: "What is the presidency for? How does it work? How can good, fair, and poor presidential performance be meaningfully distinguished? What kind of person makes a good president?" All of the above are offered on page 3, and I have doubts about how well these questions are answered.

The seven chapters seem logically organized in attempting to address influences on choice and judgment. Chapter 1 provides a good introduction to what follows in offering propositions relating to presidents' public support. Chapter 2 concerns political culture, and Buchanan claims that the presidency has three interrelated meanings for U.S. culture: efficacy, deliverance, and greatness; but we are left wondering why these three are important. Greatness is discussed without referencing studies by historians (e.g., Schlesinger, Maranell, Murray and Blessing, Simonton); indeed, the author claims "instability from one historian's poll to another" (p. 40), when in fact several are highly correlated. So many additional concepts are discussed in the chapter (e.g. style, leadership, character, power, success, charisma, image) that they overwhelm and are underexplained.

Only in chapter 3 does Buchanan try his hand at data analysis on an admittedly unrepresentative sample of undergraduate students at the University of Texas. This chapter concerns political culture, and the author factor-analyzes 72 "performance expectations," asking students to prioritize general, domestic, economic, and foreign policy. It is probably obvious that respondents would see the general category as more important than any

of its three narrower components. Ideology is also introduced; but it is unclear to me why Carter is the best example of a policy liberal, Nixon a conservative, and Kennedy a pragmatist, when others—empirically rather than intuitively—find Kennedy as most liberal and Reagan as most conservative of contemporary presidents (e.g., Bond and Fleisher). In any event, Buchanan argues for a pragmatic rather than liberal or conservative president. The chapter does reference the psychological literature but it is difficult to see how his concepts relate to psychological ones.

Throughout the book individual presidents are offered as examples, but these are illustrations rather than systematic evidence. Presumably students were asked hypothetical questions, so we do not know how Carter, for example, was perceived. Buchanan's inevitably favorable comparison of Reagan with Carter may be time-bound now that we have lived through the Iran-Contra crisis. While admitting that the distinction between them often is overdone, he proceeds to pick illustrations to "prove" his points. Skill is introduced in chapter 4, as are other concepts like "privileged cushion," which are not related to the empirical literature. The latter seems to consist largely of smooth Gallup support levels.

Chapter 5 on grounds for judgment is the most interesting with its six "competent process standards"; they are nicely developed propositions. However, it is hard to see how basing them on Reagan alone makes them applicable to his predecessors. They seem unique and occurred for him only part of the time. It is difficult to establish the author's claim that the six standards will help presidents "govern wisely and implement policy with lasting results" (p. 134).

In contrast to chapter 5, chapter 6 on grounds for choice is weakly developed. The author debunks the three "traditional" grounds without anchoring them well in the literature. Why are policy package, track record, and character traditional?

Chapter 7 compares the advantages and disadvantages of the proposed single six-year term. Presumably it is introduced as among "ways to reduce the importance of public support to presidential functioning" (p. 20), but it is hard to see how it follows from the preceding chapters.

The primary problem with this book is that

the main concepts are not tied together in any comprehensive theory. The organization and conceptualization of the entire book are ambiguous. The only real attempt to relate concepts is in a hidden discussion in the second appendix (Figure B-1). Overreliance on journalistic accounts make it impressionistic. Many assertions are undocumented (e.g., pp. 30, 63, 70, 76, 107, 128). Are we really to believe from this convoluted discussion that Buchanan provides the "citizen's-eye view" promised on the back cover?

Despite severe problems, *The Citizen's Presidency* has its strengths. Considering its complexity, it is well written and edited. Buchanan presents numerous propositions that are both interesting and thought-provoking. He believes that "failure to improve quality of evaluation could ultimately result in diminution of citizen power" (p. viii). Obviously it is an important topic that needs more systematic attention.

STEVEN A. SHULL

*University of New Orleans*

**Political Parties in Local Areas.** Edited by William Crotty (Knoxville: University of Tennessee Press, 1986. xi, 253p. \$27.95).

Local party organizations represent one of the more fascinating aspects of the study of political parties. Even so, there has been a paucity of research based on local parties and their activities. Many reasons have been offered to explain the failure of scholars to explore this admittedly fertile soil: the variation in local party organizations by region, state, and locale; the use of nonpartisan elections; the fact that local parties often rely on volunteers; and, that local elections suffer from low participation rates. Furthermore, there are large numbers of precincts that are normally unorganized, inactive, and lacking in leadership. All of these factors contribute to the difficulties encountered by researchers in the field of local political parties. It is all the more reason why a book such as this is a welcomed addition to the existing body of literature.

In an excellent introductory essay, William Crotty notes that contrary to the prevailing view, there is recent evidence that political

parties at the state and county levels may be better organized, have command of greater resources, and be in a better condition than has been previously thought. The determination of whether this represents permanent strength or more transitory and phenomenal growth represents one of the principal reasons this project was undertaken.

Although the studies included in this volume represent six authors writing about five cities, considerable effort was made to provide a collaborative and conceptual structure. The intention was to offer an assessment of the value and relevance of parties in the modern big-city United States, to evaluate the contributions and activities of local political parties empirically, to test the party decline thesis, and to outline a basis for future research.

Richard Murray and Kent Tedin studied levels of party competitiveness and the factors that motivate activists in Houston. They found that the Texas Republican party is disciplined, unified, and generally effective in spite of minority status. The Democrats, on the other hand, continue to dominate state and local elections but have not risen to the challenge of two-party political reality.

In her study of Nashville local party organizations, Anne Hopkins found little Republican structure and few GOP electoral victories. She speculates that long one-party dominance of an area may depress party activity and organizational development.

Samuel Eldersveld, using his own series of interviews with Detroit precinct leaders from 1956 to 1980 and Dwaine Marvick's party activist interviews for the same period, traces the changes that have occurred in operations, personnel, and work incentives as a way to assess the ability of the parties to adapt to changing environments.

Since 1956 Dwaine Marvick has interviewed party activists in Los Angeles, constructing a valuable longitudinal data set covering over 30 years. Based on this extensive data collection, Marvick reaches several conclusions regarding the party workers' policy views, strategies, ideological commitments, and their perception of the voters, candidates, and the political system itself.

William Crotty provides a fresh view of the machine-dominated politics of Chicago. He explores the reasons the machine has survived and endured and reviews the electoral division

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between the "downtown" Democratic wards and the Cook County suburban Republican organizations and he has little expectation that Chicago politics will soon change.

Although collections of this type often show little comparability, this one differs in that the authors met and planned the project beforehand and attempted to use similar survey instruments in data collection. They did not emerge with many common findings but did demonstrate that local party organizations respond to different environmental cues. They were unable to shed much light on the party decline thesis, but generally concluded that levels of activity at the local level in these five cities do not suggest much organizational atrophy.

The very fact that these scholars found that the local parties are active in campaigning and in the electoral process suggests that the accepted characteristics of local parties listed earlier are not always true. This is a useful and valuable book and an important contribution to the study of local parties.

ROBERT J. HUCKSHORN

*Florida Atlantic University*

**Women, Elections, and Representation.** By R. Darcy, Susan Welch, and Janet Clark (New York: Longman, 1987. viii, 181p. \$12.95).

*Women, Elections, and Representation* is a timely offering and one that complements Susan J. Carroll's *Women As Candidates in American Politics* (1985). Both books cover roughly the decade of the 1970s, and Darcy, Welch, and Clark also bring us data from the early 1980s. Both books treat the electoral problems of women from the perspective of theories of democratic representation. Carroll uses survey data from female candidates for state legislative, statewide, and congressional offices. Darcy, Welch, and Clark use data mainly from election statistics for women and men running in local, state, and national legislative races, in both primary and general elections. Taken together, these books conclude that though voter prejudice, antiwoman party elites, and lack of access to campaign funding may have been problems for women candidates in the past, those problems in actuality

have receded even if perceptions of discrimination still linger. The more women run for office, the more they win and will win—up to a point. That point is the electoral system itself. Both books point to the political opportunity structure, with its high rates of predominantly male incumbency and slow turnover, as a strong deterrent to women's gaining representational parity.

In addition to the negative effects of such incumbency and turnover, Darcy, Welch, and Clark also provide evidence that multimember districts enhance, and single-member districts retard, women's chances to gain office. On the basis of this analysis they call for more research into what kind of electoral system might best serve women's representation without negating the gains made by minority men in single-member district systems. We should also ask if the same kind of system that enhances white women's representation would do the same for women of color. In fact, one question that goes begging in this book is what explains the grosser underrepresentation of women of color in comparison to that of white women. Comparing the barriers to representation of "women and minorities" will not answer this question, since women are differentially privileged or denied because of race while minorities include women as well as men.

Darcy, Welch, and Clark approach the "problem" of women's underrepresentation in public office by trying to solve the "puzzle" of why so few women still seek and gain office. Implicit in the question, why so few? is the assumption that in the democratic system of the United States the intention has been that women should serve, and serve equally with men, in public offices at all levels of government. And yet by the authors' own assiduous sifting of the historical literature on the colonial and U.S. Revolutionary periods, they are brought to conclude that "the American Revolution resulted in a denial of political rights to all women" (p. 3). This denial was intentional, as their examination of early state constitutions demonstrates. One could argue that this was no covert "male conspiracy" (one of the hypotheses they test) but rather overt public policy making by white men of property according to the rules they set up for themselves. Curiously the authors do not discuss the U.S. Constitution and its first ten amend-

ments. This would have provided the federal context for reinforcing the authority of the states to exclude women from the political process. Nor do they go into how the institution of slavery in many of the states, reinforced by that Constitution, served to exclude African-American women and men not just from the political process but from human status itself.

There is, then, an inherent tension throughout this book between philosophical assumptions about the democratic nature of the U.S. political system and empirical evidence that leads to the inference that the "democratic" electoral system itself is the problem. This is not to say that the authors have not provided us a valuable synthesis of much of the literature relevant to women in electoral politics. They have done that and more with their own original analyses of women's gains in local, state, and national legislatures, whether those gains be actual, as in the local and state legislatures, or projected, as in their computer model of the U.S. House of Representatives. Perhaps unintentionally, they have shown us *why so many*. They have shown us what women have been able to accomplish in two hundred years in making an undemocratic political system move toward some form of democratic ideal.

DIANE L. FOWLKES

*Georgia State University*

**Citizen Participation in Public Decision Making.** Edited by Jack DeSario and Stuart Langton (Westport, CT: Greenwood, 1987. xii, 237p. \$35.00).

The premise for this book is that power over government policies has been delegated to technocrats, at the cost of democratic control. The editors believe we need to find workable means of democratic control by examining new or unusual citizen participation programs that mix elements of technical expertise about what is possible and citizen views about what is desirable. The purpose of the book is to evaluate current programs and make recommendations (p. x). The 10 essays report on specific programs, summarize prior evaluations of programs in specific policy areas, offer conceptual schemata, and make suggestions for action.

Michael D. Reagan and Victoria Lynn Fedor-Thurman provide an excellent analysis of the California attempt, through a public advisory and an energy commission, to increase public participation. Despite the openness, the authors find an "insignificant quantity of public participation" (p. 91). The key is intensity: when a specific outcome closely touches personal life, individuals are stimulated to participate in hearings, signaling to decision makers a depth of concern but not providing a reliable indicator of broad public preferences. By contrast, "public interest" groups "are increasingly likely to be involved the more the matter involves general policy" (p. 95), though the input of these "staff groups" on policy development is largely divorced from broad membership preferences. The difference between individual citizens and public interest groups lies in what they become intense about. Intensity is crucial because participation costs—time, effort, information—are high and benefits are, at best, unclear.

Marcus E. Ethridge's "Procedures for Citizen Involvement in Environmental Policy" identifies the explanations for the existence of public participation procedures as "a conservative legal movement to constrain . . . regulatory authority"; "a legislative concern with controlling" the use of "delegated legislative power"; and an "administrative interest in developing and maintaining independent political power bases" (p. 117). The procedures do not democratize administrative decision making and what little participation occurs is "systematically unrepresentative of the public interest" (p. 122). Absent "broad-based mobilization" (p. 130), the processes at most keep a subgovernment, or iron triangle, within bounds.

The remaining articles make less distinctive contributions. Rachel Bratt's review of federal attempts to spread citizen-initiated housing programs ends with a suggested marketing role for the national government (stimulate experiments, market the best to local governments with funds and guidelines, allow local adaptations). Mary G. and Robert W. Kweit note that citizens' views are facts crucial to standard policy analytic techniques such as cost-benefit analysis. The final essay by the editors makes five suggestions: that citizens have a clear role and power, that citizens get the information and resources they need, that citizens provide

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normative contributions and experts technical contributions, that joint review of contributions occur at a later stage (p. 219).

Overall, the book is difficult to place in the context of political science. The proffered definitions (p. 5) of *technocracy* and *democracy* make no reference to influence, power, authority, or control. Perhaps because of the editors' premise that past and current policy-making simply represents failure, the essays do not discuss long-standing means of democratic participation and control—elections, parties, interest groups, juries. Excepting the articles by Reagan and Fedor-Thurman and by Ethridge, there is little awareness of the politics of policy-making, the stages of decision making, or the role of information in opinion formation.

This book fails to integrate intense concern and value judgments about citizen participation with professional expertise about political participation and public decision making. The political science literature is partly at fault. Aside from voting in national elections, political and governmental activities of citizens are not covered in mainstream political science. However, the editors cannot be excused for ignorance of the few studies that do exist, for example, John C. Pierce and Harvey R. Doerksen, *Water Politics and Public Involvement* (1976). Another weakness is that while citizens are considered a source of value judgments, public preferences about citizen participation itself are not mentioned in the book. Michael P. Smith and I found some surprises when we studied them ("Citizen Participation and Urban Water Resource Uses," in *The Delivery of Urban Services*, ed. Elinor Ostrom [1976]).

DOUGLAS D. ROSE

*Tulane University*

**Redefining the Supreme Court's Role: A Theory of Managing the Federal Judicial Process.** By Samuel Estreicher and John Sexton (New Haven: Yale University Press, 1986. x, 201p. \$20.00).

Common wisdom has it that there is a workload crisis in the U.S. Supreme Court. The perception of a crisis is common; the wisdom of

the perception is debatable. More contentious than the existence of the problem are the proposed solutions. One solution has been to create a new court between the U.S. courts of appeals and the Supreme Court. A phoenix, the proposal for such a court continues to be rejected, but it keeps coming back in different forms, currently as the "Intercircuit Tribunal" (ICT). Samuel Estreicher and John Sexton, former U.S. Supreme Court law clerks for justices Powell and Burger, now professors of law at New York University, have entered the debate, and as a result of their research, argument can be conducted on a much more informed basis. "We observed that the claims . . . for a new national appellate court rested on empirical premises that . . . had [not been] established. . . . We were struck by the fact that the case for the new court was being made at a level of abstraction and bald assertion—with advocates simply declaring that the Court was now not able to hear all of the cases that it should hear, but without identifying precisely which cases those were (that is without specifying the role of the Supreme Court within our judicial system)" (pp. ix, 3).

The authors redefine the Supreme Court's role in terms of a "managerial model," and establish criteria for case selection accordingly. Then they, along with the editors of the *New York University Law Review*, examine all cases petitioned during the 1982 term in light of the criteria. They find that nearly one-fourth of the cases granted had no legitimate claim on the Court's time and resources and that significantly less than 1% of the cases denied review were cases that should have been heard. They conclude that "the problem is not one of workload, however, but one of role definition" (p. 7). Accepting their conclusion depends upon accepting their criteria for case selection and accepting their coding. The criteria eliminate cases taken solely for error correction, and they prevent premature Supreme Court intervention (for example, by having the Supreme Court refuse to resolve a circuit split until at least three circuits have ruled).

The value of this research does not turn on acceptance of the authors' criteria for case selection. More important is their demonstration that control of the "problem" lies clearly within the hands of the justices. There is no need for structural change; indeed, they

demonstrate persuasively that a new structure would probably exacerbate the problem. Their arguments against the ICT, and structural change generally, are not new, though they state them succinctly and persuasively. What makes their effort particularly worthwhile is the systematic examination of all potential cases for a given term so that the arguments can be grounded in data, not anecdotes. The analysis is not statistical, but it appears to be rigorous and thorough. The book only provides a summary of the more extensive work reported in 1,253 pages of volume 59 of the *New York University Law Review*. It is here that scholars will ultimately turn rather than to the book.

Though the primary focus of the book is to rebut the need for the ICT, the authors see this book as doing something more. They claim their role redefinition "has a radicalizing potential for altering our basic conceptions about the Court's relationship to the federal lawmaking process, the nature of practice before the Court, and the roles of other actors in the system" (p. 128). The book achieves this goal less well.

"The Court's principal objectives in selecting cases for plenary consideration," they write, "should be to establish clearly and definitively the contours of national legal doctrine once the issues have fully 'percolated' in the lower courts, to settle fundamental inter-branch and state-federal conflicts, and to encourage the state and federal appellate courts to engage in thoughtful decision making, mindful of their own responsibility in the national lawmaking process" (pp. 4-5). Would any justice disagree? Either the authors knock down a straw man, or they do not demonstrate adequately why their proposals are so radical. The authors posit that the concept of the Supreme Court as the court of last resort is a myth. But sophisticated observers have no such illusions. Indeed, justices themselves state that the Court is not there to correct errors. Justices admit that they do it, but they rarely justify it as a proper role. Since justices continue to do it, however, one must ask why they do not resist. Is it simply discipline, or something else? Perhaps keeping lower courts off balance might serve an enforcement function. Or perpetuating the myth may serve a crucial need at those times when the Court must act in an antimajoritarian fashion. In short, the authors do not pose hard

questions to their own arguments. They treat issues intelligently but not thoroughly; and in this vein they tend to argue by case anecdote. It is unfortunate, because one suspects that they could make their "role" argument quite intelligently and thoroughly as they have done with other issues in the book.

The book makes an important contribution. It is very good at providing an overview of the Supreme Court's function in the federal appellate structure, especially for the person who is not familiar with law and knows the role of the Court only by the big cases. For those with a more detailed interest in the Court, the book provides an excellent summary of their very rich data set. The authors certainly have added wisdom.

H. W. PERRY, JR.

*Harvard University*

**The Role of State Supreme Courts in the New Judicial Federalism.** Susan P. Fino (Westport, CT: Greenwood, 1987). xii, 154p. \$29.95).

One concern raised frequently in the assessments of judicial politics and behavior research is that far too little attention has been devoted to examining courts in a comparative context, be it cross-institutional within the same political system or cross-national among and between political systems. Despite encouragements to engage in such designs, precious little comparative research has appeared. Occasionally scholars have seized the opportunity to use the fifty U.S. state systems as a source of theory building and hypothesis testing, recognizing that the similarities and differences across the settings lend themselves to comparative analysis, but no focused research agenda has emerged. Given this trend, Fino's study of U.S. state supreme courts represents a welcome addition to the literature.

At the outset Fino makes it clear, however, that her interest in state courts of last resort goes beyond a dispassionate study of variation across judicial systems. Instead, she views state supreme courts as participants within a broader pluralist system that contributes to the making of public policy in the United States. In this context she is avowedly activist, arguing that state supreme courts should act as a bul-

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wark against the apparent retrenchment in the level of protection afforded to civil liberties and civil rights by the U.S. Supreme Court. Much of Fino's argument in the opening chapters is thus prescriptive in tone, seeking to define a role for state supreme courts within a larger "new judicial federalism." Although there is much promise in conceptualizing state courts of last resort as participants within a U.S. judicial pluralism, the normative component of her model, which envisions the courts pursuing particular political goals, seems to be less than adequately integrated with the empirical component designed to assess dimensions of judicial performance.

One of the author's central concerns is to identify the characteristics of a "'good' state supreme court" (p. 14), that is "one which is committed to the development of an independent body of state law through the rendition of principal decisions" (p. 5). In this scheme, a "good" state supreme court is committed to pursuing an activist role. Activism, in turn, is denoted by three criteria that serve as indicators for state supreme court performance, the dependent variable for much of the empirical analysis that follows. These criteria include, in the author's words, "the reliance on independent and adequate state grounds; the citation of precedent borrowed from sister states; and the citation of law review or other scholarly articles" (p. 5). A detailed model is provided that views state supreme court performance as a function of a series of relationships involving socioeconomic diversity, political-legal culture, quantity of litigation, institutional characteristics of state judiciaries and the social background and role characteristics of the justices. The chapters which follow offer a wealth of information about the recruitment of judges, the judges' social background characteristics, and a variety of institutional characteristics about state supreme courts and their respective judicial systems.

Despite, and perhaps because of, the large amount of data, the study does not fulfill its potential. At times we are offered segmented analysis that all but teases us with the rich potential of the subject matter. Other analysis covers familiar, albeit interesting, issues, such as the section on formal recruitment methods and U.S. regional politics. In yet other places, Fino wrestles the concepts of political culture in trying to explain variations in supreme court

performance, a less than wholly successful endeavor not so much because her analysis is wanting as because of the vagaries inherent in Elazar's original typology. Much of the book offers detailed examination of six courts—Arizona, Kentucky, California, Michigan, Nebraska, and New Jersey—each representative of a class, or type, of state court of last resort. Although these brief case studies suggest trends within the larger universe of courts, the author offers a number of disclaimers about generalizing to all supreme courts. In her words, they are intended to be "suggestive" (p. 65) only, and she is candid about the extent to which her arguments can be supported by the types of data she offers (p. 87).

In the final analysis much slips between the theory of judicial performance offered at the outset and the analysis presented through the book. Still, there is much here that should encourage others to probe the interactions between courts and their respective sociopolitical systems within the context of the fifty U.S. states.

BURTON ATKINS

*Florida State University*

**American Medicine: The Power Shift.** By Eli Ginzberg (Totowa, NJ: Rowman and Allan, 1985. ix, 224p. \$32.50).

**The Health Economy.** By Victor Fuchs (Cambridge: Harvard University Press, 1986. 401p. \$28.00).

The history of U.S. medicine over the past 15 years has been one of great change and considerable confusion. In 1971, Senator Edward Kennedy had his aides write a book about the world of U.S. medical care, entitled *In Critical Condition*. This sense of trouble was so widespread that Republicans and Democrats, liberals and conservatives, competed over which form of national health insurance to offer. In 1974, for instance, the now-forgotten Kennedy-Mills proposal received extended consideration in the finance committees of the Congress, as did Nixon's proposed Comprehensive Health Insurance Plan and the catastrophic health insurance bill of Senators Long and Ribicoff. It all seems very long ago, looking back from the Reagan era, this flurry of

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30 to 40 million citizens who lack anything resembling decent health insurance. So 15 years after talk of a medical world in critical condition, the problems of access continue to be serious, the relative rate of inflation has slowed slightly, and truly extraordinary changes in the rules of the professional game are taking place as U.S. capitalism flexes its muscles on the \$400-billion industry one used to call medicine.

In this context one turns to the books under review with hopes of illumination. Both are collections of essays by widely acclaimed health economists. Both take very wide views of their subjects, how and why U.S. medicine has taken the shape it has. Both, alas, suffer from excruciatingly obvious flaws. For a political scientist to take them on, a test of relevance is required. We all know that collections of essays risk repetition, looseness of argument, and the absence of sustained analysis. Stipulating that, is there anything useful to add when advising one's colleagues about what another discipline has to offer scholars of U.S. medicine and that industry's place in the politics of the U.S. welfare state?

My answer is that there are very good reasons for critically reviewing the perspective of widely read health economists. Among social scientists, economists have generally had the most influence on the formulation and evaluation of U.S. public policy. The prominence of the Council of Economic Advisors is but the most obvious symbol of this vaunted status. Health economists have been at the forefront of this disciplinary imperialism, displaying their wares to students, staff, and policy-makers. They have impressively shaped both the debate over U.S. medicine and the conventional answers given to what are, as Fuchs put it, the "exceedingly difficult questions about efficiency and equity in health." (p. 1) Moreover, health economists with the broad interests of Fuchs and Ginzberg show no hesitation in discussing the behavior of governments, evaluating their actions, explaining their decisions, and predicting their futures. Understanding the economist's viewpoint, then, is clearly useful, protecting us from unwarranted generalizations and helping us select what is valuable in the discipline's distinctive approach and claims. Let me begin with Fuchs's work.

Although the book's scope is broad and its

themes inviting, *The Health Economy's* substance is ultimately disappointing. There are at least three reasons for this. First, the work has all the problems of a collection of essays, in this instance articles written during the 1970s and 1980s. Repetition, loose links among parts, the absence of sustained argument and analysis—all of these are familiar. One solution would have been to rewrite the essays completely, using the earlier work as a first draft; this would have been another book. Another would have been to begin the collection with a substantial introduction that outlines the argument fully and illustrates how each of the chapters support it; this would have both clarified and made more palatable the resulting repetition and predicted omissions. Fuchs does provide a brief introduction, but it is the loosest guide to how the individual parts hang together. Infuriatingly, he leaves out, both in the introduction and the individual chapters, precisely where and when each essay was written or published. Given the profound changes in U.S. medicine in recent years, a collection of essays on "the health economy" that ignores most of them by patching together disparate papers between two covers is bound to be disappointing.

The second problem has to do with Fuchs's notion of how economics can help in understanding the place of medical care and health in so-called postindustrial societies. He assumes his readers will be familiar with health care institutions but not with economics. So he devotes the opening part to Health Economics 101, lecturing us about the most elementary conceptions of centrist neoclassical economic theory. One wonders where he first delivered this homiletic effort, but it fits uneasily in a collection of scholarship ranging over the many topics of the health economy. Moreover, Fuchs's real focus is on health economists, what they can teach the rest of the world and "what exciting opportunities and challenges" the changing world of medicine and health offers to his profession. One hoped the book would illuminate the broad world of health; one gets bits—often very fine bits—on this and that subject matter, grouped together under the vague rubrics of "the economic approach," empirical studies (first, of medical care and then of health), and finally a fourth part on "health" itself.

The substantive chapters do, nonetheless,



have partial truths to offer. The empirical chapters take up a number of quite interesting topics. Fuchs provides good reasons to think that physicians can create part of the demand for their services; he calls attention to data that support the conclusion—much associated with Fuchs's scholarship but widely shared—that rich, modern societies with substantial health care resources cannot expect to produce large gains in health status from more of the same medical care. And there are other chapters of equal interest and cogency: a pioneering paper on surgery, detailed investigations of the benefits of motor vehicle inspection, the effects of low-level radiation exposure, and the complicated causal relations between education and smoking.

As a guide to public policy, however, the work is quite disappointing. Looking over his shoulder at fellow economists, Fuchs is at pains to try to explain why modern citizens should want first-dollar health insurance, why national health insurance should appeal to such a wide variety of nations, and why the role of government should have expanded so greatly in this field during the twentieth century. Here Fuchs alternates between the banal and the dubious, interrupted intermittently by common sense. For instance, he claims that "the conditions of modern life seem to compel a more equal sharing of material goods and political power" (p. 290). Maybe, maybe not. But no serious analysis is presented to substantiate this high-level claim. In Fuchs's view, "national health insurance and other interventions in health are best viewed as political acts undertaken for political and social objectives that are minimally related to the health of the population. . . . a conclusion," we are told, "inescapable from the evidence now available" (p. 292). Yet, though Fuchs does review the evidence on health status and public policy, he hardly touches the rich literature—in political science and elsewhere—on how and why national health insurance and other public policies politically emerged. His chapter on the "irrationality" of national health insurance is not about collective madness but about the presumptions of his U.S. colleagues in health economics. He is at pains to show them why what they regard as "irrational" is commonly taken to be both a civilized way to finance medical care and a necessary public feature of every other industrial democracy but the

United States. Who, we should ask, merits the "irrational" adjective?

There is, then, something quite odd about this collection of the essential Fuchs. High-minded and vacuous on the largest political questions in health, Fuchs is rigorous in reviewing the statistical work in epidemiology. The result is a book that irritates in its mix of levels of analysis while contributing a reprinting of some of Fuchs's most interesting empirical studies of selected topics in the world of medical care and health. This is not a substantial work on its announced subject, *The Health Economy*. And from the standpoint of political sciences scholarship, the book is galling in its ignorance. Fuchs wonders about the origins of governmental action in health but ignores Robert Alford's seminal *Health Care Politics*. He writes broadly about national health insurance as a political topic, but one looks in vain for references to Paul Starr's magisterial synthesis, *The Social Transformation of American Medicine*, or to Rudolf Klein's account of the politics of the British National Health Services' origins and postwar development. Nowhere is there mention of U.S. political studies: my own work on Medicare or National Health Insurance, the scholarship of Frank Thompson, Deborah Stone, Judith Feder, or Harvey Sapolsky, let alone the philosophical reflections of a Norman Daniels or a James Childress on what Fuchs lumps together as "equity" issues. It is churlish, in one sense, to recite a book's omissions. But Fuchs advertises his scholarship as an inquiry into health and medical care in a "broad social, political, and economic context." It is no such thing as regards the political context of health policymaking in the United States. And where there is a large gap between promise and performance, scholarly norms call for blunt words. Political studies all too often concede the world of economic concerns to economists; this humility (or fright) may be quite harmful. But equally harmful is the slothful scholarship of the imperialistic economist, mixing the detailed analysis of costs and production functions with the most casually constructed accounts of why governments should, or should not, act and why they act as they do.

Ginzberg's *American Medicine: The Power Shift* is somewhat less ambitious in scope but hardly less parochial. Yet the parochialism is quite different in character. Whereas Fuchs

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professes the virtues of proper economic analysis, Ginzberg ponders about the present value of the "most important efforts [he] made in the early 1980s to understand the unsolved problems facing the U.S. health care system and to identify the preferred solutions" (p. ix). This is no modest aim; but Ginzberg manages to group 18 essays into four parts, each fetchingly titled to entice the reader into a sense of delving in high political economy: "A Tale of Four Decades" (part 1); "The Mirage of Competition" (part 2); "Physicians, Nurses, and Allied Health Personnel" (part 3); and the quintessential grab bag, "Health Agenda" (part 4).

Ginzberg's book, as with Fuchs's, raises questions of aesthetic taste and scholarly norms. He has the grace to report at the start of each chapter whence it came. The foreword explains that the various essays "explore the complex forces that played themselves out" in U.S. medical politics and "point up the anticipated, as well as the unanticipated interactions among them" (p. xv). One would have thought the scholarship on federal and state health politics would have informed this inquiry. Instead, it appears that Ginzberg's chief scholarly authorities are other works by the selfsame Ginzberg. I counted, in reviewing the first chapter on the past four decades, 10 references to Eli Ginzberg out of a footnote total of 19.

Leaving scholarly style aside, what is one to say about the addition to our understanding of the "power shift" in U.S. medicine this book of essays offers? Ginzberg's first chapter makes the unexceptional claim that our system is a thoroughly mixed one, with for-profit, non-profit, and governmental actors all sharing power in medical care. "We have no option," he tells us, "but to continue along the path that we have been going, seeking to introduce feasible changes while preserving the many virtues of the mixed system that has evolved" (p. 12). This is a good example of giving advice without much guidance. He is rightly concerned that "significant improvement in health care delivery can never be achieved by the passage of laws such as procompetition or by sole recourse to an administrative device such as prospective budgeting." But it is quite unclear what he is suggesting when he concludes that "significant improvement requires continuing adjustments in the use of resources informed by the lessons of experience," adjust-

ments that will be forthcoming when the "leadership in each community [recognizes] its obligation to become involved in improving health care delivery for all persons in their area" (p. 52). Well, yes, but how will that come about? Ginzberg is relentless in questioning the simplistic search for panaceas to U.S. medicine's troubles. He punctures the "grand illusion" of competition (chap. 7), cautions that competitive forces are now firmly at work, and illustrates, in a fifth chapter on competition and cost containment, how easily one can confuse cost containment with sensible restraint of wasteful expenditure. He predicts the "coming struggle for the health care dollar," though many would say we are already there, and urges, in chapter 17, "a plea for caution" in "the reform of Medicare." In all of these ways, Ginzberg's essays raise real difficulties and make plausible recommendations. But as with so many of his economicalist colleagues, Ginzberg is much better at telling us what to do than how to do it. He brings decades of experience to this counsel and if he annoys the colleague who wishes for more detail of design and more generous acknowledgement of the work of others, these short essays offer at least modest value for the money.

Both books, however, reveal how much the study of an important policy field is an interdisciplinary exercise. The economist brings a crucial set of questions that no political scientist can afford to ignore. But the work of economists—typically, so self-contained both about their expertise and, alas, their ignorance—reminds us that political and other social scientists will have to do most of the scholarly integration. Something in the training of dismal scientists—the Jim Tobins and the Robert Heilbroners and the James Buchanans notwithstanding—directs their attention inward. It's a shame, as both these works reveal. Yet there is an alternative available within the economics' profession; Bob Evans's brilliant treatise, *Strained Mercy: The Economics of Canadian Health Care*. Less easy to buy from its Canadian publisher, Evans's book is substantially more illuminating, the best work I know of in English on the economic issues of medicine.

THEODORE R. MARMOR

Yale University

**Local Health Policy in Action: The Municipal Health Services Program.** By Eli Ginzberg, Edith M. Davis, and Miriam Ostow (Totowa, NJ: Rowman & Allanheld, 1985. xiv, 136p. \$28.50).

*Local Health Policy in Action* reports the results of a five-year five-city evaluation study of the Municipal Health Services Program (MHSP), a demonstration project designed to improve the delivery of primary health care to the inner city poor. Between 1978 and 1983, evaluators from the Conservation of Human Resources at Columbia University monitored the implementation of MHSP in Baltimore, Cincinnati, Milwaukee, St. Louis, and San Jose. This report describes changes that took place during the course of program implementation and seeks to explain them in terms of a number of political, community, and professional organizational variables. Based on findings from their evaluation research, the authors make recommendations relative to the future use of urban community health care centers. The assessment of program utilization and cost outcomes is reserved for another report.

Lacking an operational definition of the dependent variable in this study (implementation), the reader is forced to infer from the detailed analysis in chapters 2 and 3 that implementation is conceptualized as a series of on-site decisions and actions about facilities, services, and public health versus hospital resources devoted to delivering comprehensive health services to the poor. Arguing from an ecological perspective, in chapters 4 and 5 Eli Ginzberg and his coauthors attempt to show that the ability of implementors to carry out the MHSP mandate was constrained by national and, especially, local "political-institutional" environmental factors; for example, the structure of local government, the influence of area medical schools, the personality of the mayor, turn-over rates among political leaders and program managers, and circumstances unique to each site.

While the report can be recommended for what it has to say about the local politics of ambulatory health services to the inner city poor, the findings make familiar reading for implementation scholars; namely, that implementation is complex and variable and that variability in implementation behavior can

often be explained in terms of individual-level differences as well as differences in the implementation milieu. A major shortcoming is that the authors have chosen to write a report rather than a book: the sponsor-dictated research problem is atheoretical and policy-specific; the study is noncumulative in that it is not set in the context of an existing literature on implementation, urban services delivery, or evaluation research; and the findings are not generalizable: for instance, rather than purposefully or randomly selecting sites, cities were chosen for "their potential for realizing the primary goal of the demonstration" (p. 2). Thus, the report misses an opportunity to speak to a larger audience of policy generalists as well as specialists in implementation and evaluation research.

One aspect of *Local Health Policy in Action* that makes it potentially valuable to a larger audience is the creative approach that project consultant Richard Nathan developed for field research. Because it is both comparative and longitudinal and because it has the advantage of minimizing measurement error associated with conventional ex post facto evaluation studies, I personally like the approach—a standardized set of participant observations of events as they occur periodically at the five sites during the course of five years of program implementation. Unfortunately, beyond a cursory overview in the introduction and a vague reference to the MHSP demonstration fitting an "experimental construct" or presenting "the opportunity for natural experiments" (p. 79), nowhere in the volume is there a detailed explanation of how the experiment was actually conducted—how and at what intervals the qualitative data were collected and how the data were pooled and analyzed.

What is sorely needed is a discussion of problems related to conducting a study of this sort, that is, how key concepts are defined, what measurement strategies are used, how alternative explanations are treated, and how the researchers confront data reliability problems as well as internal and external threats to validity, for example, the "degrees-of-freedom" problem. Absent such a discussion, one does not know how to interpret the findings and the "lessons and implications" that are outlined in chapter 6.

*Local Health Policy in Action* should make satisfying reading for specialists in health

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a continuation of what is basically normal partisan competition.

The author's contribution is the view that McCarthy and McCarthyism are best understood as a combination of these two contradictory explanations. Republican partisan elites did indeed consider McCarthy an instrumental asset for deposing Democrats from their 20-year rule. Yet as the author contends, it is also true that McCarthy's appeal—perhaps his very strength—stemmed from an individual political style identified by pluralists as “the style of mass politics” (p. 19). Thus the author argues that “it was the injection of Joseph McCarthy's [impulsive] political style into elite partisan struggles” of the 1950–54 time period that produced the anti-Communist hysteria known as McCarthyism (p. 19).

In developing the notion of the “impulsive style” or “impulsive type” in U.S. politics, the author draws upon a number of major theorists, including Maslow (need hierarchies), Piaget (cognitive development), Kohlberg (moral development), Connell (child's construction of the political world), Barber (leadership typologies), Merelman (political ideology), Lasswell (political man) and Converse (belief systems). However, in reference to these theories, the author is unusually selective, dealing with only a very limited range of the material available. In fact one could say he is superficial and at times even outdated. For example, Kohlberg's six-stage theory of moral development is discussed on the basis of works written as long ago as 1968 and 1969. Yet not only have Kohlberg's theories been extensively critiqued in the last 15 years, but Kohlberg modified his own presentation of the “six”-stage theory since publication of the works cited by the author. What this points to is a research orientation in this book that is disappointing. It is only tantalizing, not illuminating, to refer to a number of important theoretical perspectives without employing the concomitant rigor necessary for using them in a thorough manner.

In addition, the author's application of his new political construct—the impulsive style—to an analysis of McCarthy's behavior would have been strengthened by a more systematic use of evidence. There is anecdotal invocation of snippets of information—usually quotes from McCarthy, journalists, and scholars—as illustrations of how McCarthy typifies the im-

pulsive type. However, such “evidence” is extremely fragmentary, as well as highly selective. What might have been more convincing would have been the analysis of larger blocs of McCarthy's communications—perhaps campaign speeches, the *Congressional Record*, congressional committee reports, his correspondence—to ascertain whether in fact the particular psychological outlook identifiable as the “impulsive style” predominates McCarthy's discourse and conduct.

There is merit in the book's argument that two competing explanations of McCarthy and McCarthyism—pluralist and elitist—can be reconciled by viewing McCarthy as an elite operating with the style usually associated with that of the mass public. The author presents the case for how this combination clicked in U.S. politics for a short term, leaving us with the legacy of “McCarthyism” for the long term. Whatever might be said about the methods used to develop this thesis, Landis's book does offer an interesting point of view.

EILEEN L. McDONAGH

*Northeastern University*

### **Encyclopedia of the American Constitution.**

4 vol. Edited by Leonard W. Levy and Kenneth L. Karst with Dennis J. Mahoney (New York: Macmillan, 1986. 2,196p. \$320.00).

There are at least two wrong ways to read an encyclopedia. The first is to consult it only when you want to know something specific, go directly to the entry most likely to help you, and then withdraw. This technique is excessively instrumental and rarely leads to the germination of an interesting idea. The second wrong way is the way I have just attempted in order conscientiously to review this encyclopedia, namely, to begin at the beginning and try to slog straight through, actually reading all the entries as they come. The reader of the *Encyclopedia of the American Constitution* will be suitably rewarded by what so far as I know is as close as anyone has come to the right approach to encyclopedia reading, that is, leisurely browsing. This approach maximizes pleasure as well as intellectual nutrition.

The work at hand consists of four generously sized volumes of large type, much in the style of this same publisher's *International En-*

of the volume and reduces its utility for students of southern politics.

An additional problem is that the chapters are somewhat uneven. Thus while the essay analyzing the public addresses of southern governors and gubernatorial candidates during the 1970s is firmly grounded in a carefully collected data base, some of the other essays (e.g., "Response of the Main-Line Southern White Protestant Pulpit to *Brown v. Board of Education*, 1954-1965") are based on fragmentary data. Similarly, the chapters presenting case studies of the rhetoric of selected individuals are not uniformly successful in relating these figures to their southern roots or to the events of the period.

On balance, the book is less than the sum of its parts. There is little obvious connection among the chapters beyond their common temporal and regional settings and their concern for rhetorical analysis, and there is little sense that the volume develops any clear theme except that "a variety of voices were heard as the region responded to radical social change" (p. 13). Still, most of the essays are interesting and informative, and that in itself qualifies this book as a useful addition to the literature on contemporary southern politics.

ROBERT P. STEED

*The Citadel*

**The Politics of Energy Conservation.** By Pietro S. Nivola (Washington: Brookings Institution, 1986. viii, 285p. \$31.95, cloth; \$11.95, paper).

Nivola's book is valuable as an addition to the energy policy literature for five distinct reasons: it provides case studies concerning policy development in four energy arenas—natural gas, oil, electricity, and gasoline; it illustrates the frustrations of policy-making at the national level; it examines difficulties inherent in making cohesive policies in a federal system; it examines the problems associated with the ebb and flow of executive leadership in Washington; and it provides a well-formulated argument in favor of deregulation as a policy goal to attain the conservation of energy. Because it has all of these themes, the title of the book is somewhat limiting; Nivola provides an analysis of national policy-making

in the energy arena over a period of years, only one of the purposes of which is the conservation of energy.

Particularly useful are the case studies concerning national policy-making, which are studies that could be used to illustrate the frustrations of making policies in a pluralistic nation on items of complexity. Very often the purposes of the legislation passed are confusing and the outcomes of the policies stand in stark contrast to their original intentions. Certainly the cases of oil and natural gas policy fall into that category. In his examination of the various struggles in Congress and in the states, Nivola also illustrates another point well—that the various arenas of energy policy defy aggregation. Indeed, the politics of oil, natural gas, electricity, and gasoline are separate issues, with connotations that render analysis that fails to distinguish among them of limited value.

His analysis of the various struggles within Congress include a sensitive understanding of the interactions among members of Congress, between Congress and the president, and among competing interest groups. He also illustrates the difficulty of using public opinion as a cue for national policy-making. Particularly noteworthy are his logit analyses of voting behavior in Congress, which reveal the overwhelming impact of party affiliation as a predictor of energy votes, especially in the arena of deregulation of oil and natural gas.

The analysis also is a good case study of the difficulties of comprehensive policy-making in a federal system. The regulation of electricity, particularly, remains a state function, and states take various approaches to such regulation. The result is that often national goals cannot be accomplished through state programs.

Presidential leadership is another theme of this book. The fact that presidents come and go but policies endure makes for interesting difficulties in accomplishing policy goals. When the nation has a changeover in presidents, the entire focus of policy in a given arena may change. Thus it was that the changeover from President Carter to President Reagan changed the whole debate with regard to energy policy. Under such circumstances, the directions of policy often get changed in midstream.

Finally, this book provides a well-developed

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rationale for using deregulation and market forces to accomplish the nation's energy conservation goals. For this reason, the book, while valuable, must be read in concert with the work of other scholars who argue for the conversion of the nation's energy system through solar and other renewable energy resources and those who argue for an increased governmental role in the development of the nation's energy needs in the long run.

JAMES W. RIDDLESPIERGER, JR.

*Texas Christian University*

**Comparing Presidential Behavior: Carter, Reagan, and the Macho Presidential Style.**  
By John Orman (New York: Greenwood, 1987. viii, 190p. \$35.00).

John Orman has compiled a group of individual comparative studies of Carter and Reagan and attempted to unify them into a thematically consistent whole. The result of this effort is an uneven book containing some very interesting discussions of presidential behavior.

He begins his discussion with an explanation of the macho presidential style. In brief, the style entails a president being competitive, sports-minded and athletic, decisive, unemotional, strong and aggressive, powerful, and a "real man." Carter was unfairly, according to Orman, criticized for not living up to this macho stereotype that all presidents to some degree have exhibited. Conversely, Reagan personified this macho style except for his tendency to show controlled emotions in his speeches. Reagan gained popular support by fitting the stereotype that the U.S. public expected of their president while Carter violated this myth and lost support. The ensuing chapters evaluate the Carter and Reagan presidency mainly using data from four original studies.

Using the *Public Papers* of presidents Carter and Reagan the author attempts empirically to verify access to the president. Since participants at formal meetings are listed in the *Public Papers*, Orman counts each name listed as one interaction and concludes with the total number of interactions per individual. He correctly acknowledges the fact that access is not

influence and that all members in a meeting do not contribute equally. One fruitful approach, using this data base, would have been to code individual meetings with the president as a sign of a higher advisory profile. Identifying formal access in reported meetings would not provide a complete description of presidential access but it is available information. When available, use of the daily diaries of the presidents would provide additional information on informal meetings and telephone conversations. Many presidential studies have suffered from using the best available data, even if not ideal. The conclusion of this study is that Carter had over half of his interactions with only three people: Brzezinski, Mondale, and Moore, with Rosalyn Carter attending one hundred official meetings. Carter met most often with national security advisers while staff meetings were most common in the Reagan presidency. Additional comparisons are limited by the author's method of presenting the data. While Carter's interactions are presented in nine clear tables, Reagan's interactions are summarized in only three tables. The Reagan discussion is regrettably less well documented than the Carter chapter.

A second study deals with an examination of presidential activities cataloged in the *Public Papers*. Orman cites the real problem that all activities (letter versus speech) are not equally involving but does not clearly define his categories of analysis. Therefore, the conclusion that while Carter was active in official transactions Reagan was more active in political activities becomes less significant. Why, for example, is a meeting with a "foreign head of state" viewed as political while a "communication with Congress" is seen as official?

The third study looks at interest group interactions with the president as listed in the *Public Papers*. As expected, Carter and Reagan defined their "public" in very different ways. Carter met most often with representatives of unions, the military, veterans groups, public interest groups, and religious groups. Reagan emphasized corporate leaders, conservatives, right-wing groups, and religious groups. This study gives empirical support for the argument of individual discretion for the presidents within their institutional constraints.

A 10-point list of criteria for great presidential performance focusing on justice, peace, civil rights, economic and public interest con-

cerns is described but unfortunately not directly applied in the concluding section on the press.

A longitudinal study of valenced (positive or negative) reporting on the president from 1900 to 1983 categorized all articles on presidents or presidential candidates cited in the *Reader's Guide to Periodical Literature*. The result is a fascinating body of information for students of media and politics and of presidential behavior. Orman presents the data in a useful fashion that could be used for numerous analytical discussions. However, positive valenced stories that say good things about the president are not necessarily macho stories, nor are negative stories solely "feminine"-type portrayals. So while Orman describes interesting temporal and partisan patterns in media coverage of the president he does not strongly support his macho thesis.

Empirically based studies of the president placed in a comparative perspective provide an important and expanding focus for presidential scholars. This book contributes to this tradition and presents original data that are well worth studying.

KATHY B. SMITH

Wake Forest University

**Elections in America.** Edited by Kay Lehman Schlozman (Boston: Allyn & Unwin, 1987. xvi, 336p. \$14.95).

In its conception, this book is something between a general treatment of elections and public opinion and one of the "quickie" volumes rushed to press after each quadrennial presidential election. The conception has worked out pretty well: the essays have a contemporary feel, dealing with the two Reagan elections and the Reagan presidency. On the other hand, the authors seem to have benefitted from extra time in which to reflect on events and (in a couple of cases) get hold of some timely data.

The various authors, participants in an October 1985 symposium at Boston College, are well-known and respected scholars. Many of the essays find the authors reiterating positions with which they have long been identified. Even these contributions are useful as updates of established themes (Crotty, Ranney, Jacob-

son, Warren Miller, Frankovic) or as a succinct summary of an important view of U.S. politics (Walter Dean Burnham).

Sometimes one is disappointed at what is not reiterated. On the subject of presidential nominating rules, Austin Ranney and William Crotty are old sparring partners. Here they contribute essays that are judicious, scrupulous, and balanced. All this is admirable, I suppose, but I would have liked to see some sparks fly. Ranney does a little bit better, getting in a few digs at the reformers and proposing one very radical, utterly impossible reform himself.

Sometimes, on the other hand, the reiteration seems somewhat tired and not a little anachronistic. Burnham, for example, accurately observes that parties mobilize the citizenry and especially mobilize people of low status. From this he correctly deduces and demonstrates that the decline of parties implies an increasing class bias in participation. He then concludes that this class bias is a conservative bias since "there seems little reason to doubt" that the sizable "party of nonvoters" would be Democratic voters or, if the option were available, socialist voters. There is, in fact, plenty of reason to doubt. With the rise of the elite new class, the link between social status and attitudes has become exceedingly weak. On "social issues," the well-off are more liberal than the poor. Thus it is far from clear that Burnham would get the sort of policy outcomes he wants from a more *genuinely* egalitarian political system. But of course that is not the same thing as a political system dominated by elites using the rhetoric of equality.

Burnham's suppositions run afoul of John Petrocik's careful analysis of the "party of nonvoters" from 1956 through 1984. Consistent with traditional "surge and decline" theories, nonvoters (like peripheral voters) have been heavily influenced by the short-term forces that dominated the election and have preferred the winner (be he a Democrat or Republican). Nineteen eighty, however, was an anomaly, with nonvoters preferring Carter by a substantial margin. Petrocik suggests that the 1980s may have seen the rise of a new kind of nonvoter, but in spite of a careful canvass of the data, he is puzzled about the nature of the beast.

Warren Miller's paper, a continuation of work he has done in collaboration with Merrill

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Shanks, bypasses some of the questionable features of their earlier efforts (for example, a complex, entirely hierarchical causal structure) to provide a very instructive comparison between the 1980 and 1984 elections. While most political scientists were insisting that the 1980 election had not the slightest element of a policy mandate for Ronald Reagan, Miller and Shanks showed that the voters saw government policy as being, on net, to the left of their preferred position and that this perception accounted for a substantial part of the Reagan margin. In 1984, the public perceived policy as having shifted to the right, generating a large increase in expressed satisfaction. Limiting further rightward movement, however, is the fact that among the dissatisfied, those who see policy as too conservative now outnumber those who see the status quo as being too liberal.

A pair of papers by Doris Graber and Michael Robinson debate the role of the media in the 1984 presidential election. The intellectual challenge addressed by both essays is the need to explain the fact that while network news coverage of Reagan was strongly negative, the president won by a landslide. Graber suggests that while the *words* used were highly unfavorable toward the incumbent, the pictures conveyed a much more favorable image. Robinson, in contrast, suggests that "good news" during the election year offset any (minimal) effects of "bad press." Both essays quickly go beyond their data into the realm of punditry and speculation; but this is not surprising, given the vast difficulty in actually measuring media effects.

Aaron Wildavsky's essay, "President Reagan As a Political Strategist" is brilliant and provocative. He labels the president "a superb political strategist" who is "providing about as much direction as the existing American antileadership system can support." This piece will probably be widely reprinted as a counterweight to liberal analyses that portray the president as either an ignorant dolt or (contradictorily) a nefariously effective manipulator of the masses; but one suspects that it is almost as far off the mark as they are. The deficit is one example. Wildavsky sees a cunning strategy to deprive the Democratic party of political support by depriving it of the opportunity to spend money on its favored constituencies. I see merely a president staunchly

committed to cutting taxes and increasing defense spending.

Among the other authors represented here: Gary Jacobson attacks common-cause-style campaign finance reform; Frank Sorauf provides a provocative comparison of campaign financing in the House and the Senate; Kathleen Frankovic gives a blow-by-blow account of the 1984 primary season, as seen through the *New York Times*-CBS polls; and Schlozman (with Sidney Verba) provides a fine analytic outline of ways of viewing the behavior of voters.

JOHN MCADAMS

*Marquette University*

**Chief Justice: Leadership and the Supreme Court.** By Robert J. Steamer (Columbia: University of South Carolina Press, 1986. xiv, 334p. \$24.95).

In the midst of a seemingly endless literature on the U.S. Supreme Court—legalistic, institutional, historical, behavioral—there is a relative dearth of material on the office of chief justice. Well-known, often-cited articles by Alpheus Mason, David Danelski, Walter Murphy, and Sidney Ulmer have provided an important beginning. And biographies of John Marshall, William Howard Taft, Harlan Fiske Stone, and, recently, Earl Warren have added greatly to our understanding of the office and its potential for leadership.

In 1982 the White Burkett Miller Center of Public Affairs at the University of Virginia hosted a conference on the chief-justiceship; from its proceedings came Peter Fish's monograph, *The Office of Chief Justice of the United States: Into the Federal Judiciary's Bicentennial Decade*. This may be regarded as the first book-length study of the chief-justiceship (discounting *Our Eleven Chief Justices* by Kenneth B. Umbreit [1938], which is largely biographical).

Robert J. Steamer's *Chief Justice* is more extensive than the Fish monograph, for it examines the 15 chief justices through Warren Burger in much greater detail, categorizing all largely on the basis of style and contribution. Three—Marshall, Hughes, and Warren—are classified as "great."

Aware of the difficulties of conceptualiza-



tion, Steamer nonetheless uses leadership in his analysis. What is a leader? Why are some chief justices leaders both on and off the Court? Justice Abe Fortas, Steamer tells us, called Earl Warren " 'a great and powerful leader,' declaring: 'We know this; we feel it,' but he could not say why. He finally took refuge in the phrase: 'By some process short of the occult,' no more descriptive perhaps than Frankfurter's 'intrinsic authority' or as others would say, an x-factor, an unknown quantity" (p. 62). In order to reach empirically based conclusions regarding successful leadership on the Supreme Court—focused in the position of chief justice—political scientists have relied on small group analyses, employing the behavioral distinctions of Robert Bales. Bales's task and social dimensions have been further refined to include intellectual and managerial components.

Even with such "group-based" definitions of leadership, categorizing chief justices of the United States as "great" leaders, near-greats and unsuccessfuls remains a formidable task. Steamer concludes his treatise in a manner not significantly more precise than Justice Fortas: "In asserting the performance of the men who have occupied the head chair of the nation's highest tribunal, it is evident that the leadership they provided—social, intellectual and managerial—has been qualitatively uneven, that the results for exceptional leadership are complex, enigmatic, and in large measure dependent upon imprecise, intrinsic qualities" (p. 293).

In reaching conclusions, Steamer blends—I would not say confuses—the trait and situational approaches to leadership. "Personality . . . is a major determinant of effective leadership," he argues, "but the fact that a chief justice must work his will with eight independent souls not chosen by him is a formidable barrier to his success" (p. 294).

Throughout the book, Steamer either refers to or uses situational measures—opinion assignment, conference management, court cohesion, dissent rates, procedural innovation—to operationalize "successful leadership." In this regard, *Chief Justice* builds effectively on earlier research, especially what with a more quantitative or social-psychological bent. Yet an uncertainty about how to treat, how to integrate qualitative concerns pervades the analysis: "In sum, judicial craftsmanship, while not essential to leadership, cannot be dis-

regarded" (p. 88).

*Chief Justice* is an ambitious effort, all the more admirable for its attempt to integrate traditional (often impressionistic) and empirical scholarship. While we still await the definitive, single-volume work on the office of chief justice of the United States, this work adds measurably to our understanding of this position.

ROBERT G. SEDDIG

*Allegheny College*

**Nuclear Power Transformation.** By Joseph P. Tomain (Bloomington: Indiana University Press, 1987. x, 212p. \$25.00).

"In the event of the cancellation of a nuclear power plant under construction, who pays?" Should the costs be borne by a utility's customers, its stockholders, or taxpayers generally? This is the timely policy question posed by Joseph P. Tomain in *Nuclear Power Transformation*.

In the early 1970s, the future for nuclear power in the United States looked promising indeed. New plants were coming "on line" every few months, and power companies were rushing to place orders for new reactors. It was predicted as late as 1975 that a thousand nuclear reactors would dot the North American landscape by the end of the century. Since the late seventies, however, there has been little good news for the U.S. nuclear industry. In spite of industrial investment in nuclear power in excess of federal spending on the space program or the Vietnam War, no domestic power company has placed an order for a reactor since 1977; safety concerns and outright fears about nuclear accidents are widespread; the price of nuclearly produced electricity is no longer competitive with coal; and, most critical to Tomain's analysis, nuclear power plants under construction are being scuttled, leaving staggering price tags that must be paid.

What happened? Clearly the accident at Three Mile Island (TMI) in 1979 had something to do with the loss of public confidence in nuclear power. However, the financial problems facing the nuclear industry had emerged several years before TMI grabbed the headlines. The skyrocketing cost of labor and nuclear plant materials, increases in interest

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rates for borrowing funds, and most importantly, managerial incompetence led to the actual price tag of some nuclear construction ending up being as much as 10 times more than projected.

This book, Tomain writes, could have been subtitled *From Three Mile Island to Chernobyl*. The actual title aptly summarizes one of the themes of the post-TMI history of nuclear power—transition. Since the late 1970s, nuclear power has been moving away from what Tomain calls a “traditional” pattern, typified by capital-expansive plant construction, through a transitional one, characterized by stalled and canceled construction, to a “post-industrial” pattern, predicted by Tomain to include some new scaled-down nuclear construction.

The heart of *Nuclear Power Transformation* is an excellent chapter detailing case studies of nuclear mismanagement in the transitional period. The case of the Zimmer nuclear plant in Ohio is illustrative of the current problems in the nuclear industry. The Zimmer plant was years behind schedule and more than a billion dollars over the original projected cost of \$240 million when it was canceled. The investors will probably lose a good portion of the \$1.7 billion spent on the project because the state supreme court has ruled that an Ohio utility may not raise the rates to customers to recover the losses resulting from the abandonment of nuclear construction. Only if the Zimmer plant can be converted to a coal-fired facility will the investors recoup any of their original outlays. By contrast, courts in other states have ruled that abandonment costs can be absorbed by utility ratepayers.

Tomain's *Nuclear Power Transformation* is built upon careful research in a range of government and industry materials. In addressing the question of who pays for abandonment costs, Tomain offers many thoughtful observations. However, the reader has to wade through some tedious and fuzzy exposition of “models” and “dualities” before finally arriving at Tomain's recommendations.

This lack of clarity is, unfortunately, not confined to the concluding sections. Although logically organized, *Nuclear Power Transformation* is frequently repetitious. Moreover, the author misses a number of opportunities to humanize his technical subject and provide glimpses of the personalities of the individual

regulators, citizen critics, or utility executives whose names appear in his pages. For example, it would have been intriguing to find out more about the mysterious agent hired by Cincinnati Gas and Electric to investigate problems in the Zimmer project. Part of the story of the post-TMI era is that of the people who, wittingly or unwittingly, have figured into the transition. Notwithstanding these criticisms, *Nuclear Power Transformation* is an important book and a welcome contribution to the growing list of scholarly titles on nuclear power.

JOHN W. JOHNSON

Clemson University

**Power and Pluralism in American Cities: Researching the Urban Laboratory.** Robert J. Waste (New York: Greenwood, 1987. xvi, 176p. \$32.55).

The resurgence of the study of political power, reflected in recent publications by Clarence Stone, David Ricci, William Domhoff, Thomas Dye, and most recently, Robert Waste, is long overdue. Professor Waste's book provides a detailed trip backward to the time when we were still debating whether democracy was alive and well in U.S. cities; and therein lies both its major strength and weakness.

For Waste, the pluralist-elitist debate is still unresolved and still urgent. His book restates Robert Dahl's polyarchy model, emphasizing what Dahl said (and really meant) and how his critics misinterpreted him. This attention to democratic theory should be a component of the new literature on power, for the study of power without attention to its normative implications is shortsighted. Waste makes an important contribution by reexamining the concept of polyarchy, showing that Dahl's apparently inconsistent discussions of the concept can be reconciled when we realize that in fact Dahl had two polyarchy models: one, describing an ideal (or egalitarian) polyarchy, represents Dahl's normative views on democracy; the other, describing polyarchy-in-action, represents Dahl's empirical conclusions about U.S. democracy.

Waste defends Dahl against criticism of his polyarchy model, suggesting that critics mis-

understood the dual nature of his model. But this single-minded defense of Dahl is a major weakness of Waste's book. In refighting the battles of the 1960s, Waste ignores recent advances in the study of power and their potential impact on the concept of polyarchy. Only 20% of the material referenced postdates 1975 (with fewer than 20 post-1980 references). Almost completely ignored is recent work by a number of sociologists who use network analysis procedures to assess social structure and structural impediments to action; these include Edward Laumann (*The Bonds of Pluralism* [1973]) and Ronald Burt (*Toward a Structural Theory of Action* [1982]). Also missing are references to recent studies of citizen participation, particularly at the community and neighborhood levels (e.g., Harry Boyte, *The Backyard Revolution* [1980]). Waste missed a worthwhile opportunity by not considering how these and other recent studies (1) are consistent (or inconsistent) with Dahl's earlier conclusions about polyarchy-in-action; (2) are consistent (or inconsistent) with Dahl's formulations of an ideal polyarchy; and (3) allow an expansion of either polyarchy model.

To his credit, Waste develops his own empirical procedure to assess polyarchy in urban settings. Unfortunately, he remains committed to Dahl's well-known structural criteria for polyarchy (e.g., freedom of speech, right to vote) in his choice of measures, with predictable results: he found that two Rhode Island cities, chosen to maximize differences in adherence to the principles of polyarchy, were almost identical on his polyarchy scale. This no doubt shows that U.S. cities practice a generally comparable and generally acceptable brand of democracy (although Waste's conclusions may be confounded by insensitivity in his measures). But I expect that cities nonetheless *do* differ in meaningful ways in their adherence to democratic principles. Further, we can assess those differences through a careful examination of Dahl's eighth criterion for polyarchy: governmental policies reflect citizen preferences. The community power studies conducted during the 1950s and the 1960s attempted to assess the strength of this linkage, and the more recent studies of political networks and participation were largely motivated by the same consideration. With more work in this area, a bridge between polyarchy-in-action and ideal polyarchy could be

constructed. It is a pity that Waste did not take us further in this direction.

JOHN M. BOLLAND

*University of Alabama*

**Money, Media, and the Grass Roots: State Ballot Issues and the Electoral Process.** By Betty H. Zisk (Newbury Park, CA: Sage, 1987. 279p, \$29.95, cloth; \$14.95, paper).

This is a book about selected ballot proposition campaigns and elections in California, Massachusetts, Michigan, and Oregon between 1976 and 1982. It is based primarily on secondary sources, elite interviews, and participant observation. The book seeks to explain electoral success and assess the educational impact of direct legislation (p. 158). The book differs from recent books and articles on this subject by focusing on elite behavior, issue activists, and the conduct of specific campaigns.

Zisk's summaries of the issue campaigns in these states shed light on a form of electioneering that has been largely overlooked by political scientists. The book addresses several salient questions: Who gets involved in these campaigns? Why? To what extent are issue networks established? Do supporters and opponents seem to learn from the experiences of proponents and opponents in other states? How? What strategies exist to get issues on the ballot, to persuade voters, to dominate the media? Zisk's examination of voter decision making is based not only on her own research but on the work of others as well.

While the book is insightful in several respects, several methodological and research design mistakes limit its contribution. By the author's own admission the selection of states and issues for the study was highly subjective (p. 23). This resulted understandably from both time and budget limitations and the desire to focus more on the elite level. But the book poses several questions for which a larger sample of data exists and is more appropriate. Ranges of participation on ballot measures, for instance, are readily available from most states and the Inter-University Consortium for Political and Social Research. The book not only fails to pull from this broader and more appropriate sample but does not even look at all of the measures in the four states for the period

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studied. Using these incomplete data, the author dismisses the fatigue and negativism hypotheses found in some of the literature she examines (John Mueller, "Voting on the Propositions" *American Political Science Review* 63[1969]: 1197-1213). In addition, the book excludes from analysis the 10 states that permit voters to decide constitutional amendments and revisions but not popular initiatives or legislative statutes. This peculiar division of states potentially affects the book's analysis of issue agendas, numbers of issues presented to voters, and so on.

Zisk also mistakenly defines *turnout* as the percentage of registered voters who vote, rather than as the percentage of the voting age population who vote. Although she knows that rates of registration vary across these states, she repeatedly uses this less accurate measure of participation (p. 112).

In terms of the book's stated objectives—to help readers understand electoral success (or, more commonly, failure) in ballot propositions and to clarify the level of public education resulting from the process—Zisk is critical of both dimensions of the process. She finds dis-

couraging the power of money in proposition voting; the confusing, distorted, and overly simplified nature of the campaigns; the scanty coverage by newspapers on the substance of the ballot questions; and the low degree of voter sophistication (p. 246). In short, Zisk claims that ballot propositions are often not referendums on the petitioner's question but instead are measures of how ably sponsors can distort and confuse questions to the voter through 30-second advertisements and through the media's focus on charges and countercharges rather than on substance. Contradicting these conclusions, the author ends the book by saying, "While these mechanisms for direct democracy are hardly the universal panacea that some past reformers have claimed, they have helped to open up new pathways for participation, for political communication and even for policy innovation" (p. 268). In this treatment of direct democracy, as in so many others, the author ends up pulling her punch.

DAVID B. MAGLEBY

Brigham Young University

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## COMPARATIVE POLITICS

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**Congress, Parliament, and Defense: The Impact of Legislative Reform on Defense Accountability in Britain and America.** By Andrew Cox and Stephen Kirby (New York: St. Martin's, 1986. ix, 315p. \$29.95).

Cox and Kirby, both of Hull University, have selected a promising topic for study. They seek to use the notion of defense accountability as a device for probing into the dark side of the national security state, or in this case national security states of the United Kingdom and the United States.

Vital issues are addressed. Questions of titillating weapons procurement scandals are as current as this morning's newspaper, lending immediacy to their study. Further, they have linked case studies to important concerns—executive-legislative relationships, the costs of secrecy in democracies, and the

possibility and utility of institutional reform—by means of the concept of accountability, which they define in essence as "the obligation on those who have been delegated to carry out public functions to reveal, to explain and to account financially for their actions and decisions" (p. 9).

Crucial defense decisions involve the allocation of resources for defense as opposed to other programs and the allocation and management of resources within the area of defense. The notions of *public accountability*, where governments are required to justify defense in relation to domestic programs, *program accountability*, by which officials demonstrate that programs are achieving their objectives; and *financial accountability*, relating both to legality and efficiency, are introduced to guide the reader.

Cox and Kirby had the right idea. They

undertook comparative research in Great Britain and the United States in the late 1970s and early 1980s to see if it was possible for legislatures to reform themselves in order to be better able to scrutinize and control defense policy-making. While there are many differences between Parliament and Congress, they find that the defense field is particularly resistant to direct accountability for several important reasons, including the unwillingness of legislators to discharge their functions properly, the lack of expertise outside the defense establishment and the dearth of independent agencies to provide information that is not first filtered through defense channels. Even if these problems could be overcome, the executive can usually rely on secrecy or the tendency of defense-related committees to be made up of prodefense legislators. In fact it turns out that to be effective, the legislature needs the executive branch to carry out its own functions competently, itself a problematic precondition for meaningful oversight.

The authors wisely point out that accountability in setting defense priorities relates to the commitments expressed in a state's foreign policy. In addition, assessments must be made of the intentions, strategies, and capabilities of potential adversaries. These political questions also must be followed by a political evaluation of the priority of defense in relation to other national goals. In practice, as the authors point out, deficiencies in the legislative budget process, despite reforms, have kept Congress and the Parliament from systematically setting priorities.

In the realm of defense procurement, the authors detail the story of the success of Congress in overturning the "unsound" decision to exclude funding for the Marine Corps AV-8B V/STOL aircraft. Yet even this case "is explained more by a rare combination of political factors than it is by the effect of a reformed budget process" (p. 205). The British reforms contributed to the illumination of the defense procurement process but did not have significant policy impact. Studies of British and U.S. manpower policy reinforce the general conclusion of the study.

The authors seem displeased with their conclusions. The creation of special budget and oversight committees in the two legislatures has had relatively little impact on the traditional committees and on the traditional means

of getting things done. They praise the British efforts to release more information while pointing out that the Congress is actually suffering from information overload. Ultimately, they call for the development of proportional representation in Britain and the adoption of a new freedom of information act there.

Though they are not optimistic about the prospects for defense accountability being achieved in the future, due to the partisan and parochial attitudes of legislators and those who control the executive branch, they hold out the hope that somehow circumstances might conspire for a forward-looking legislature to create a role for itself in the coming budget-crunch era. The failure to face up to the findings of the research, which, unfortunately, are excessively repeated, weakens an otherwise worthwhile addition to the literature of legislative-executive relations, public administration, and national security policy studies.

FREDERIC A. BERGERSON

*Whittier College*

**Comparative Policy Research: Learning from Experience.** Edited by Meinolf Dierkes, Hans Weiler, and Ariane Berthoin Antal (New York: St. Martin's, 1987. 531p. \$40.00).

During 1982 and 1983, the Berlin Wissenschaftszentrum, in conjunction with Stanford University, attempted to assess the last two decades of comparative policy research. This bulky volume is the result. It is less than a fully integrated stocktaking but it is also more than the usual pseudobook produced by collecting conference papers.

After the introductory preliminaries (where, among other things, Karl Deutsch reviews two thousand years of comparative research), the first section of the book deals with problems of methodology and the uses of comparative research. Adam Przeworski offers a very practical overview assessing methods of cross-national research since 1970. Alex Inkeles examines the difficulty of relating such research to the needs of policymakers. In a third chapter Aaron Wildavsky applies his four-fold classification of political cultures to the problem of chemical regulation and uses of research in six developed nations.

The heart of the book consists of eight chap-

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ters reviewing the relevant literature in four substantive fields: environmental policy, education policy, economic policy, and social policy. In each of these areas, the project organizers have tried to have the authors assess the "state of the art" by addressing a common set of questions. Some of these issues are theoretical (What theories have implicitly or explicitly been developing in the field and how have they fared in research findings?) some are methodological (What are the particular problems associated with cross-national policy research?) and some are organizational (What have we learned about how to manage such research realistically?). A brief final section seeks to draw conclusions and chart future directions.

As is normal in such projects (and this one has 34 authors or coauthors), certain people appear to have taken their assignment seriously, read each other's papers, brought citations up to date, and tried to think through their position. Others seem to have been along for the ride. To me, Przeworski's chapter is in the former category, offering a clear-headed and frank discussion of methodological issues. David Vogel's review of the literature on comparative environmental policy sets a comparably high standard of thoroughness and thoughtfulness, as does the chapter on economic policy by Burkhard Strümpel and Joachim Scholz. There are other good chapters, as well as some real clunkers. Enough said.

Most readers interested in this field will probably find this book's greatest value to lie in the literature reviews of the four substantive fields. Unfortunately, only Vogel's chapter appears to cover works appearing after 1984. Much of the rest of the volume has a canonical quality that, however valid the injunctions, is unlikely to alter the way other researchers carry out their work.

What have we learned about doing comparative policy research? The results surveyed in Ariane Berthoin Antal's thoughtful last chapter are not likely to surprise many readers. We have learned that comparative studies need to be more attentive to theory building so that cumulative results are achieved. Studies need to be more historically grounded and use more longitudinal data. We should worry more about the parochialism in our choice of countries and the true equivalency of issues in dif-

ferent national settings. We should do a better job of parsing the implicit models that lie hidden in our varying research designs. For more scholarly and socially useful comparative policy studies we do not need some new methodological breakthrough. We need to use more carefully and rigorously the methodologies we already possess. To cope with the sheer complexity and volume of information needed in large-scale comparative studies, we need to become more adept at organizing scholarship through multinational and multidisciplinary teams of researchers. Most of us are ill-prepared to do that and pass the incapacity on to the next generation by the way we teach students to work.

To all of which one can only say *amen*. However it is curious that the editors come down very hard on the last point without ever mentioning recent strides being made in truly cross-national team research. For years now people have been making assertions about the distributional consequences of different nations' social and economic policies. What we haven't had are good data to test such assertions. The so-called Luxembourg Income Study—a robust international team project—is now giving us the first really comparable data on individual financial resources in a series of developed nations and it is but the forerunner of an even more far-reaching program of comparative research at the European Center for Population Studies in Luxembourg. It is an elementary point, but I think it is true. Students of comparative public policy have enough theories, hypotheses, methodological tools, and conferencing opportunities to keep them going into the next century. What they do not have are decent data.

HUGH HECLIO

*George Mason University*

**The Politics of Colombia.** By Robert H. Dix (New York: Praeger, 1987. xv, 247p. \$37.95).

Colombia is one of the most significant countries in Latin America, yet it is also one of the most seriously neglected by both scholars and journalists. Although it is the third or fourth most populous nation in the region and

one of the very few countries in the Americas that has maintained a stable civilian-ruled political system since the late 1950s, Colombia is largely ignored in the United States. Except for occasional articles about the lucrative and violent cocaine traffic in the popular press, there is a great silence. General volumes on Latin America normally omit discussion of this important South American nation almost entirely.

Our collective ignorance about Colombia makes Robert Dix's outstanding new book an especially welcome event. His carefully researched and gracefully written analysis provides an excellent introduction to this complex nation, which will benefit any interested student or scholar. As he did in his classic 1967 volume, *Colombia: Political Dimensions of Change*, Dix carefully integrates the relevant scholarly literature on Colombian politics (which now includes many of his own specialized studies on populist movements and other topics) into a coherent and illuminating synthesis. Although there are other very useful single-volume treatments of Colombian politics, such as Harvey Kline's insightful *Colombia: Portrait of Unity and Diversity* (1983), Dix's book, which covers events as late as the 1986 elections, is the most important comprehensive and up-to-date work available.

Dix's book begins by placing Colombia in comparative perspective emphasizing major similarities to other Latin American nations (e.g., economic dependency and Iberian colonial origins) and highlighting key differences (e.g., noninterventionist military tradition and strong traditional political parties). The author also provides a brief but thorough historical overview that does not slight the critical nineteenth century, during which many of the enduring patterns of Colombian politics developed. Later chapters analyze in detail, with extensive references to the literature, the socioeconomic and geographical context of politics, parties and electoral behavior, interest group activity, governmental structure, and characteristics of contemporary public policy formation.

After 30 years of study, Dix still finds Colombia to be a puzzling and somewhat paradoxical country. In spite of its liberal democratic formal institutions and procedures, the Colombian polity is revealed by Dix to be a highly elitist system, in which power is

narrowly concentrated. He also observes that despite some of the most glaring socioeconomic inequalities in Latin America, the radical Left remains quite weak, notwithstanding several active but ineffectual Marxist guerrilla groups. In addition, the author continues to be intrigued by the fact that although rapid social mobilization has completely transformed Colombian society in recent decades, it has left traditional political institutions remarkably intact.

In his conclusion Dix examines some possible explanations for these and other Colombian puzzles. Ultimately, in contrast to more radical observers, he reaches the conclusion that Colombia's paradoxical political system has its own logic and can be expected to "muddle through" for a good deal longer. His argument, like his entire volume, is persuasive.

J. MARK RUHL

*Dickinson College*

**Centre Formation, Protest Movements, and Class Structure in Europe and the United States.** Edited by S. N. Eisenstadt, L. Roniger, and A. Seligman (New York: New York University Press, 1987. 187p. \$30.00).

The stated purpose of this book is to present a new approach to the comparative study of societies and politics. The new approach, which the authors occasionally call the *civilizational approach*, focuses on the normative beliefs of "elites"—a term that, unfortunately, is never systematically defined in the book but that seems to refer to all individuals and groups enjoying unusually high amounts of prestige within a given society or territory.

Allegedly, the political behavior of elites is shaped to a great extent by the "chasm between the transcendental and mundane order," or, stated differently, an inevitable gap between what is and what ought to be (p. 9). The authors concede that many other social circumstances influence elites' political behavior. But they single out elites' moral and religious beliefs for special attention.

According to the authors, elites consciously design and redesign political institutions and processes according to their religious beliefs and moral principles. Depending on how they define their moral and religious standards and

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conceptualize the gap between what is and what ought to be, elites will shape their existing political systems so as to include or exclude other groups in society, and so as to tolerate, suppress or encourage certain forms of protest activity.

Eisenstadt develops the analytic framework in two theoretical chapters. The remaining chapters use this thematic focus on elites and their moral and religious beliefs to answer certain perennial issues in Western social science, such as U.S. exceptionalism (Why is there no socialism in the United States?) and southern European political turbulence (Why are democratic regimes in southern Europe so fragile?). Other historical topics include French state building and student protest in England and Italy. The chapters are primarily syntheses of currently available historical literature. The authors do not present fresh data; rather, they present alternative interpretations of history, in which the ideals of elites are given considerable causal weight.

There is an interesting tension throughout this book between (1) a reductionist interpretation of history, in which the theological and moral beliefs of elites are an omnipresent first cause behind all sorts of trends, events and actions, and (2) a more richly contextual view of history, in which the beliefs of elites are only part of an immense set of significant factors that jointly shape human history. The ambiguity is due in part to the authors' incomplete development of their theoretical framework, a situation that the authors openly acknowledge. They are confident that the theological-moral beliefs of elites matter. But the authors are not yet certain how *much* the theological-moral beliefs of the elites matter or how elites' beliefs concretely interact with other possibly important variables, such as sudden breakthroughs in science and technology, gradual changes in divisions of labor, and differences in material interests among people of different classes.

In addition, the civilizational approach tends to minimize the ideologies of lower classes or to treat their ideological evolution in a surprisingly superficial manner. Lower-class moral and political ideologies, to the extent that they are discussed at all, are treated primarily either as mere repetitions of elites' beliefs or as simple repudiations of elites' values and beliefs. However, as many

historians—such as E. P. Thompson, Christopher Hill, and Lawrence Goodwyn—have pointed out, the beliefs of nonelites often have a substance and a developmental logic of their own, largely independent of the moral beliefs of elites. Moreover, Thompson, Hill, and Goodwyn insist that the beliefs of nonelites often have important consequences for the organization and transformation of society at large. They therefore encourage intellectual historians to look at ideas “from the bottom up.” The advocates of the civilizational approach, however, choose to look “from the top down” and presume that elites have an unparalleled say in how social and political institutions develop.

*Centre Formation, Protest Movements, and Class Structure in Europe and the United States*, in short, is a politically and intellectually provocative book, given its thesis about the decisive political and social importance of elites' religious assumptions and moral aspirations. The book's central argument is often vaguely stated, and not infrequently it is overstated. Certainly much more can be (and has been) said about how ideas—in particular the ideas of poorer classes—impinge on social and political history. The book is nonetheless important, because it compels us to reconsider how and why moral and religious ideals matter and because it provokes us to reconsider the origins, nature, and extent of elites' powers.

CYRUS ERNESTO ZIRAKZADEH

*University of Connecticut*

**Growth to Limits: The Western European Welfare States since World War II.** Edited by Peter Flora. Vol. 1, *Sweden, Norway, Finland, Denmark*. Vol. 2, *Germany, United Kingdom, Ireland, Italy* (New York: Walter de Gruyter, 1986. 383p. and 499p. \$95.75 and \$122.40, cloth; \$29.95 each, paper).

These volumes analyze welfare state development primarily for the period since 1950 for 8 of the 12 countries included in a project directed by the inveterate research maestro in this field. With the exception of France, these are the same nations covered in Peter Flora's earlier endeavors. Once again he has lead and directed a group of younger political



sociologists and political scientists, this time to produce in-depth monographs mainly written by natives of each country. The comparative analysis based on their country studies will be presented by Flora in one of three additional volumes soon to be published.

Each national study opens with a historical synopsis of developments in the first half of the century. Then follow a descriptive section concentrating on the development of welfare state resources and clientele; an evaluation of the distribution of services and transfers in terms of their impact on the distribution of life chances; and analyses of the demographic, economic, and political correlates of welfare program development over the 30-year period. In the final sections public opinion data are drawn on to help assess future options particularly in the light of experiences during the decade of resource constraints since the mid-1970s.

In view of the extensive research particularly on this part of Europe, one test for the volume on Scandinavia is that for redundancy. It meets this test very well, for the wealth of data tapped by the authors presents much more fine, detailed analysis than is found in most of the earlier English language literature. While it is not clear why the countries are presented in reverse alphabetical order, Sven Olson makes an excellent start with the Swedish chapter. In his evaluation he distinguishes between the more successful—income maintenance and health—and the less successful policy experiences, as in education and housing. But he also presents data from successive levels of living surveys that permit other conclusions to be drawn, since these show that individuals “with problems” were reduced much more in housing than in health between 1968 and 1981 (p. 63).

Sampling the other authors’ analyses of how their countries handled the problems of the past decade, one finds variations that suggest that reference to a single underlying Scandinavian model may be losing utility. For Denmark Lars Norby Johansen ascertains that the reason why the Glistrup backlash evaporated later in the seventies was partly due to increased mobilization of welfare state clients and public employees but that this led to increased mobilization and declining consensus. He identifies a trend toward a dual welfare state, with entitlements and universalism remaining dominant in some areas, but with a market-based counter-

process becoming stronger, particularly in housing and pension policies. Denmark was slower to react to the mid-seventies stagnation than Finland, where Matti Alestalo and Hannu Usitalo credit earlier pruning with helping to engender increasing consensus during the latter 1970s. They are confident that the major welfare state achievement will not be threatened during the 1990s. Oil helped Norway to postpone hard choices, and Stein Kuhnle is a little more guarded in assessing prospects there. A stronger conservative party has pushed for privatization with more effect there, and he predicts that some changes in tax and benefit structures may have to be made. But partly because the welfare state is of particular importance to the Norwegian political culture, he prognosticates that “a radical turn toward a privatized residual welfare state is unlikely.”

As these examples show, the authors are not coy or cautious about interpreting the trends that are extracted from their very diverse data. Thus their prognoses can be matched against others employing more formal models. The aim of not losing a potential audience among policy-makers is evident also in the title. Flora sees growth over the three decades explained politically mainly by the structural elimination of the self-employed “natural enemies” of the welfare state. Future limits are seen as the result of recent rapid growth and (following Allardt) in the shift in focus from *having* to *being* and *loving*.

In the second volume both the countries and the authors are more heterogeneous. The fine hand of the most experienced of the contributors is evident in the German section, while those on Italy and Ireland were developed from doctoral dissertations at the European University Institute, under whose sponsorship these volumes appear. Peculiarities of the less well known Irish and Italian situations are skillfully analyzed, as in Maria Maguire’s presentation of how the Irish attempt to close the welfare gap that separated it from the rest of Europe was made possible in the 1960s through the Catholic church’s policy reversal on key aspects of social services reform. Her description of the mixed success of the endeavors to link higher education to employment markets is borne out by the recent exodus of Irish young people.

Everywhere there is superb documentation, which will be still further extended in a subse-

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quent volume. While the contributions are similarly structured, they vary significantly so as to allow the authors to explore particularly unique problems. Where called for, general rules are deviated from in order to analyze a particular problem dimension. Thus, whereas intranational regional differences are generally not treated, this rule is broken for Italy. Mauricio Ferraro illustrates vividly the striking North-South differences in the contributions and expenditures mix of welfare programs and how these are explained by linkages to party networks and especially to the quasi-partisan *patronati*, which regulate individual access to welfare.

The German and British chapters help clarify why the coming to power of conservative governments in 1979 and 1982 have had such different consequences. The analysis of the party control-welfare relationship is much more abbreviated for Britain, and the claim that "legislative changes are not a fully adequate indicator of the political input to social policy" (p. 223) cannot by itself justify this. One wonders whether the publishers expressed their annoyance by misspelling the names of both the country and author, Richard Parry, in the table of contents, as well as that of his home town elsewhere. But generally the degree of gremlin control is quite high; and the volumes are well produced.

For Germany, Jens Alber's analysis of post-war core laws shows that of those passed through 1974, 81% initiated extensions, only 8% cutbacks; in the subsequent decade 56% instituted cutbacks, only 27% extensions. Public opinion support of spending levels weakened, but in 1984 those favoring curtailment in the crucial social transfer system were less than a fifth in all social groups. By contrast, support for the universalistic basis of many British programs was eroding, and Parry ascertains that even in the Labour movement "an essentially individualistic model of consumption is displacing faith in social solidarity and collective allocation" (p. 235).

Overall, these volumes present unmatched analyses of developments in Europe, and they will be essential to U.S. social scientists whose access to the underlying European publications and sources is more restricted.

ARNOLD J. HEIDENHEIMER

Washington University

**The Philippine State and the Marcos Regime: The Politics of Export.** By Gary Hawes (Ithaca: Cornell University Press, 1987. 196p. \$25.00).

Hawes's study of the interaction of the agricultural export economy with the international economy and its effect on economics and politics in the Philippines, based on 1980 research but updated, focuses on three case studies of agricultural production in coconut, sugar, and fruit products. Each study is presented in separate chapters. There is also an introductory chapter, "Creation of the Philippine Political Economy" and a concluding one, "State and Regime in the Philippine Context." A bibliography is not included, although there are five appendixes detailing agricultural production and identifying principal producers.

Hawes's work makes many of the same points as Frank H. Golay's earlier study, *The Philippines: Public Policy and National Economic Development* (1961). Whereas Golay wrote that "the Philippine economy has many attributes of a fluid 'frontier' economy in which the entrepreneurial class as a competing elite is displacing the heretofore dominant agricultural aristocracy. . . . Filipino entrepreneurship is distinctly Filipino in its strong predilection to seek economic advantage through political institutions and processes" (pp. 408-9), Hawes observes how "elite cohesion" broke down with the development of competing economic strategies, one emphasizing import substitution industrialization and the other, agricultural exports (pp. 125-26, 352).

Hawes claims that one reason for Ferdinand Marcos's declaration of martial law in 1972 was the diversification of economic investment into industrial production that threatened the elite's "privileged positions" (p. 38). Then he argues that the development of "state autonomy" during martial law helped to rationalize development policy around an export-oriented agriculture, a rationalization now threatened by the return to a democratic political system (p. 53). His case studies are chosen to illustrate how an "export-oriented model of development" was a means to sustain Marcos in power and to reconsolidate "class domination" by a traditional elite (p. 54).

His primary intent of demonstrating how domestic economic interests and international

economic pressures interact with domestic politics to affect the political structure is a laudable one and central to understanding Philippine politics. Yet his case studies do not fully explain this interactive process.

His analysis of the evolution of state policy toward these agricultural sectors has an artificial flavor. Various government orders are recited in copious detail and corporate directorships outlined without fundamentally explaining why and how decisions occurred. For example, we are told that a Filipino company, House of Investments, was a minority shareholder with Dole in an agribusiness concern that created an inequitable system of "grower's contracts" with small farmers to produce and market bananas, but we are never told who owned House of Investments (it is not even cited in the index) nor why Dole would have collaborated with it. As Hawes has argued that Marcos-crony capitalism was an important determinant of economic strategy, one wonders if that was true in this case.

His study would have been better served with a more thorough updating. While published after the fall of Marcos, references are made as if he were still president (p. 78). In addition, several important developments that occurred during the latter part of the Marcos period but that give insight into economic policy and elite attitudes go unnoticed. For example, in coconuts, no mention is made of the attempted assassination of Emmanuel Pelaez, now ambassador to the United States, reputedly because of his efforts to protect coconut farmers from Marcos cronies. In sugar, efforts by another crony, Antonio Florendo, to gain control of the world sugar trade, the failure of which contributed to Marcos's overthrow by eroding economic growth levels even further, is also not noted.

In his conclusions Hawes disaggregates elements of the bourgeoisie to demonstrate how the impact of the Marcos regime on their economic interests contributed to his fall (pp. 135-42). Workers do not figure prominently in his analysis. The National Federation of Sugar Workers is mentioned once (p. 100). However, as his case studies have not clearly demonstrated competing elite interests—how they interacted and how they were affected by Marcos's policies—one is left with a hypothesis but not a proof. Yet the hypothesis is an extremely important one, especially if, as Hawes con-

cludes, the Philippine State under President Aquino "has changed, but very little" (p. 164).

RICHARD J. KESSLER

*Carnegie Endowment for International Peace*

**Intellectual Foundations of the Nicaraguan Revolution.** By Donald C. Hodges (Austin: University of Texas Press, 1986. xiii, 378p. \$27.50, cloth; \$11.50, paper).

The exact nature of the ideological orientation of the Sandinista government in Nicaragua is an issue much argued and little understood. Popular misconceptions notwithstanding, most students of the Nicaraguan revolution realize that Sandinista ideology is a far cry from orthodox Marxism-Leninism. Yet the precise composition of the new amalgam is more difficult to determine. This task has been greatly facilitated by Donald Hodges's new work. *Intellectual Foundations of the Nicaraguan Revolution* carefully examines the diverse influences in the revolutionary process. It is clearly the seminal work on Sandinista ideology and arguably the best treatment of Augusto César Sandino's (1895-1934) thought. It goes far beyond David Nolan's more modest *The Ideology of the Sandinistas and the Nicaraguan Revolution* and adds much new information on Sandino not found in Neil Macaulay's excellent *The Sandino Affair* or Gregorio Selser's pioneering study, *Sandino* (1957).

Hodges begins by carefully tracing Augusto César Sandino's life and intellectual development. Particular attention is paid to the Sandinist namesake's exposure to radical Mexican nationalism, *anarquismo*, communism, and theosophy (which Hodges sees as a major influence in Sandino's thought). Part 2 examines the intellectual development of the current Sandinistas and is distinguished by its careful scholarship, ample commentary on the genesis of each possible influence, and comparative analysis of its appearance in religious or revolutionary movements elsewhere. Like most of us who would understand this complex phenomenon, Hodges examines the role of nationalism and Nicaraguan history but pays particular attention to the import of the new Marxism and the new Christianity (an outgrowth of liberation theology in Nicaragua). A

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chapter on military strategy is broad in scope and among the best ever written on the subject in the Latin American context.

The work argues that although Sandino began his political career as a Nicaraguan liberal, "his ideology evolved from a rudimentary anarcho-communism acquired during his first stay in Mexico into the comparatively sophisticated political theosophy developed during his second visit there from 1929 to 1930" (p. 19). Although a few elements of Sandino's strategic thinking were circuitously incorporated into Guevara's thinking and the revolutionary struggle in Cuba, most lay dormant until after the Sandinistas were forced to rethink their positions in light of several costly military defeats in the first part of the sixties. Carlos Fonseca Amador then encouraged the careful study of Sandino's ideas and began to circulate his writings and syntheses of his political and military thought. Only after reflecting on the politicoorganizational thrust of Sandino's original message (and their previous overemphasis on armed struggle without political organization) did the Sandinistas begin to grasp the importance of mass organization and broad-based people's warfare. Alternately working with peasants, workers, the masses generally, sectors of the church, and even bourgeois politicians, the young revolutionaries gradually built the politicoorganizational base that enabled them to topple the hated Somoza regime. Their openness to all the most dynamic (and nondogmatic) currents in the new Marxism that grew up in Latin America and Europe in the sixties and seventies, to the new Christianity that was developing in Latin America, and—most importantly—to the increasingly strong insurrectionalist tendencies in the Nicaraguan people themselves, enabled them to guide (but not always lead) the overthrow of the hated dictatorship. The resultant ideology was strongly influenced not only by dynamic new currents in Marxism and Christianity but by the revolutionary fervor of the Nicaraguan people.

The most controversial aspect of the work is Hodges's thesis that the well-documented heavy incorporation of liberation theology and Ernesto Cardinal's radical Christianity into revolutionary ideology is but a modern-day substitution of liberation theology for the radical rationalist theosophy that—he argues—so heavily influenced the original Sandino.

Careful reading of the footnotes reveals that one of the most frequently cited sources to document the strength of Sandino's belief in theosophy and the heavy influence of the Mexican Communist party that Hodges alleges is none other than the highly propagandistic *El verdadero Sandino, o el calvario de las Segovias* by Anastacio Somoza himself. Thus one is placed in the strange position of needing to accept much of what the author of Sandino's death said about him in order to accept the full validity of Hodges's argument. Problems in these areas or with Hodges's reticence about acknowledging just how different the use of popular people's war was in Nicaragua as compared to Cuba do not, however, diminish the overall importance of the work. Rather they suggest that—as is the case with most ground-breaking scholarship—there are other possible interpretations of some of the rich data presented.

The great value of *Intellectual Foundations of the Nicaraguan Revolution* lies in Hodges's discussion of how Sandinista ideology evolved from Sandino, the confluence of historical factors that facilitated this process, the brilliant analyses of what the author correctly labels the new Marxism and the new Christianity in Nicaragua and in the aforementioned analysis of revolutionary strategy. This work is by far the most comprehensive effort in these areas and will become the standard work for understanding the ideology of revolutionary Nicaragua.

HARRY E. VARDEN

*University of South Florida*

**Working Class Formation: Nineteenth-Century Patterns in Western Europe and the United States.** Edited by Ira Katznelson and Aristide R. Zolberg (Princeton: Princeton University Press, 1986. viii, 470p. \$55.00, cloth; \$14.50, paper).

In the past two decades, inspired in large part by E. P. Thompson's *The Making of the English Working Class*, historians, sociologists, and political scientists have produced a large number of studies of the development of the working class in the nineteenth century in various of the now industrialized countries. These studies have generally focused on a

given community or occupation or a relatively short time period in one country or both. The aims of this edited volume are (1) to build on these studies to produce overviews of the process of working class formation in eight essays focusing on different time periods in three countries (the United States, France, and Germany) and (2) to use this material along with studies of Britain to develop in two essays some conclusions about the common features of working class formation and the causes of variation among the countries. The value of this volume is enhanced by its cohesion. It is not the ordinary compilation of essays. The authors met twice: once to react to a discussion paper on class formation by Katznelson (which formed the basis for chapter 1) and to work out a shared agenda for the individual essays and a second time to discuss drafts of the essays. Thus there is considerable theoretical continuity in how each of the essays deals with the material.

This volume's importance lies not only in its presentation of the historical evidence and comparative synthesis, it also makes an important contribution to a great lacuna in class theory. In the past decade and a half, there has been a proliferation of sociological works discussing the criteria for the placement of positions, occupations, or groups into objectively defined categories of class. By contrast, there has been little theoretical and empirical work on the process of class formation; that is, the process by which objectively defined categories of class do (or do not) lead to the development of cohesive social groups, which, in turn, become the basis for common consciousness and collective action.

Katznelson's initial essay outlines a framework for the study of class formation, suggesting "that class in capitalist society be thought of as a concept with four connected layers [or levels] of theory and history: those of structure, ways of life, dispositions, and collective action" (p. 14). To elaborate briefly, *structure* is class at the objective level; and variations in structure are those due to level of economic development, capital intensity, average enterprise size, product specialization, and so on. The level *ways of life* includes such factors as work relations, labor market relations, and patterns of residence. *Dispositions* are shared attitudes, including class subcultural definitions of society and social relationships. *Col-*

*lective action* includes not only riots, strikes, and demonstrations but also movements and organizations. Diversity, through time and across countries, at levels 3 and 4 are the main objects of analysis of the essays.

It is impossible to do justice to the individual essays in such a short review. Suffice it to say that aside from the main themes that emerge again in Zolberg's concluding comparative essay, almost all of them contain observations that help one to understand facets of the development of labor in the respective countries. To take just a few examples, Bridges argues, convincingly I think, that artisans in the earlier nineteenth century United States were not culturally different from their European counterparts (contra Hartz). Rather, their different reactions can be accounted for by the U.S. setting: widespread suffrage and the urban locale of industrialization in a growing agricultural country. Cottareau throws new light on the reasons for the low degree of formal organization of the French unions, showing its interconnection with industrial structure and foreign competition, low population growth and tight labor markets, and high degree of informal workers' control in the workplace.

Zolberg begins his comparative historical synthesis with a devastating attack on previous attempts to account for variation in working class ideology and organization by Lorwin, Rokkan, and Lipset. He rightly argues that World War I and the Russian revolution so massively changed the political scenery as far as labor was concerned that it is impossible to connect prewar social and historical factors directly to postwar characteristics of labor movements, as these authors do. His analysis, which focuses on the situation at the outbreak of the war, emphasizes two broad sets of factors accounting for variations in the four cases: variations in capitalist industrial structure (the pace and timing of industrialization and the structure of the economy) and the character of the political regime (legacies of absolutism and extent of political rights for the working class). The latter is particularly important for distinguishing the polar cases—both in terms of regime type and working class movement—of the United States and Germany. Variations in capitalist industrial structure elucidate variations in all four cases but are particularly important in explaining the peculiar nature of French unions and union-party relationships.

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Though explaining differences is Zolberg's main task, he takes pains to point out that there was in fact a considerable degree of convergence between the four cases as of the immediate prewar period.

As in all books that break new ground, there is also much that one will quarrel with in this volume. Two points struck me with particular force. First, despite the title and Katznelson's theoretical outline, the essays focus much more on labor movement formation than on working class formation. The consciousness and (in)action of unorganized workers are hardly discussed, especially in the sections of the book that cover the turn of the century, despite the fact that only a minority of workers in any of the countries were even union members at this time. Moreover, nonorganizational measures of class cohesion other than residential segregation (i.e., those that would tell us something about unorganized workers, such as intergenerational reproduction, intermarriage, etc.) are analyzed only in Kocka's essay. This lacuna is all the more serious in the light of the importance this group has in the analysis of the ultimate time period under discussion in the one place they are mentioned, Zolberg's analysis of prewar France. There he argues that the usual view of the French workers as radical syndicalists is false since only a small minority of workers were members of the General Confederation of Labor (CGT), many if not most CGT members voted, and a large number of French workers voted for parties of the Center and Right.

Not only are a limited number of cases considered in the volume, the cases chosen are systematically (and probably purposely) biased to the large countries. This affects not only comparative conclusions but also the analysis of many other essays since their insights are often based on comparative observations. To cite just one example, the Norwegian case completely contradicts Zolberg's political regime hypothesis, since a parliamentary regime with broad suffrage was in place before the development of the labor movement and male suffrage was adopted only a decade after the founding of the Social Democratic party; yet the pattern of labor movement development was most similar to Germany: close union-party cooperation and a radical (initially reformist Social Democratic Marxist and later syndicalist) leadership. In my view, the inclusion of the experi-

ence of the smaller democracies of Europe as well as that of Italy would change a large number of the hypotheses advanced in the book.

There is a sense, of course, in which attacking a work for what it does not do is a bit unfair, and there is no doubt that the publication of the book is a milestone in the study of working class politics. It will be essential reading for Europeanists, students of comparative labor movements and working class politics, and, more generally, of macrocomparative history for some time to come.

JOHN D. STEPHENS

*Northwestern University*

**The Politics of Oil in Indonesia: Foreign Company-Host Government Relations.** By Khong Cho-Oon (Cambridge: Cambridge University Press, 1986. x, 253p. \$39.50).

This is a study of the problems of the relationship between multinational companies and host governments arising from mutual perceptions of the imbalance in their respective bargaining strengths. It is the book's argument that this problem of "perceptions" of a sense of imbalance is essentially political in nature, with economic concerns and market forces peripheral. In order for a satisfactory relationship to be established between the foreign company and the host government there must be an objective recognition by both sides of each other's interests and negotiations in order to strike a balance.

Mineral industries are a special case. The negotiating concerns of both sides are sharpened by the finite nature of the resources. The issue of the distribution of gains from exploitation must balance the factors of foreign capital, technology, and expertise against sovereign control. Furthermore, there are different time perspectives with respect to the rate of exploitation. In this general context, the author examines the Indonesian petroleum industry as an appropriate subject, illustrating the wide range of issues raised by Indonesian aims and attitudes toward host government-foreign investor relations. The book, therefore, is a contribution to the study of global resource policy as well as the Indonesian political economy.

Looking at the pattern of relationships in the

Indonesian oil industry, governed by production-sharing agreements between Pertamina, the state oil company, and its foreign contractors, the author identifies three central areas of concern in the negotiating process. First there is question of the division of revenue. The author details the issues of royalties, taxation, depreciation, and the other financial conditions. Related to this is the determination of the absolute level of total revenues and matters affecting it, such as level of output and export prices. Disappointingly, the author does not really discuss the impact of Indonesian OPEC membership on this. Finally, there is the domestic impact of the foreign company's operations in economic and political terms. The author does not find that the oil industry has made substantial backward and forward linkages that would integrate resource activities to Indonesian national development. The author concludes that although there appears to be a basic shift in favor of Indonesia in terms of the content of the agreements for oil extraction, the examination of the operational conditions of the agreements leads to doubts whether such a shift has in fact occurred. The author attributes Indonesia's failure to make real what it has obtained on paper to deficiencies in the technical and managerial resources available to Pertamina to implement the production-sharing contract clause vesting Pertamina with sole responsibility for managing the extractive operation. In the long term, however, as the Indonesian side builds up its own capabilities, it already holds the legal means to effect the transfer of petroleum extraction from the foreign companies to indigenous management.

Readers interested in the domestic political linkages of Pertamina will be disappointed. The author does not examine Pertamina as an important political institution in Indonesia. Although he does note the problems of accountability and corruption, he warns that "attempts to investigate such matters lead to a nebulous realm because of their controversial and politically sensitive nature" (p. 165). Certainly, however, Pertamina's position as a state within a state and its off-budget financial inputs to other institutions and individuals had impact on Indonesian perceptions of its interests in its relations with the foreign companies. It is, in fact, the special nature of Pertamina in the politics of Indonesia that somewhat quali-

fies its identification by the author as an appropriate case study of his general propositions about relationships between foreign companies and host governments.

DONALD E. WEATHERBEE

*University of South Carolina*

**Religion and Political Conflict in Latin America.** Edited by Daniel H. Levine (Chapel Hill: University of North Carolina Press, 1986. 266p. \$24.95).

**The Catholic Church and Politics in Brazil, 1916-85.** By Scott Mainwaring (Stanford, CA: Stanford University Press, 1986. 328p. \$37.50).

These two books contribute to a well-delineated, and well-worthwhile subfield of Latin American studies, that which deals with Catholicism. (Although *religion* is used in the titles, on the whole Catholicism is what is meant.) This body of literature is conveniently and necessarily manageable in size—*necessarily* because the kind of immersion in the topic necessary to produce quality work requires a degree of intellectual, almost emotional, commitment to the topic that not many can muster, certainly not over the long run. Levine has in fact stayed with the topic for over 10 years; I very much hope that Mainwaring—whose volume is a revision of his dissertation—will do so, because it is an excellent piece of work.

The issue with an essentially historical account such as Mainwaring's is generally not—and certainly not in this case—whether the main facts are accurate. The academic (and literary) issue is whether the author shows sensitivity to the forces that produced the facts and whether enough empathy is shown to make them come alive. The history of the evolution of Catholicism—in this case, Brazil's—is essentially the history of a series of internal movements and of reactions to these movements, with both action and reaction propelled by persons of one or another kind of conviction. The structure of the church, its hierarchical nature, its international links, and its doctrine are not just "there." They are interpreted, vigorously accepted, severely questioned, or taken up to be modified by the groups that form these movements. The

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author's job is to portray the perceptions of the structure and ideology of the church and of the surrounding society that the leaders and followers of these movements and countermovements maintain. Only then can the reader understand why actors behave the way they do.

Of all this, Mainwaring does an excellent job, all the more praiseworthy because the period he covers and the movements and geographical regions he covers are extensive. Beginning (in essence, and the title notwithstanding) in the late fifties with the various progressive movements, many involving Catholic students and lower-level priests but always with the essential protection of at least a segment of the senior hierarchy, Mainwaring then portrays the church's reaction to the military coup of 1964 and its aftermath. While both in 1964 and again more recently a notable conservative strain asserts itself as a reaction to what are perceived as left-radical excesses in society at large (1964) or in the internal movements of structural and doctrinal reform (liberation theology), the long-term historical change is clearly "progressive." The essential concern of the church with secular matters is now accepted, and the structure of the church has changed: the laity, including "popular sectors," have assumed a role that would have been totally inconceivable a mere 30 years ago. The frictions to which this gives rise—as well as the frequent *lack* of friction compared with what might have been expected—are not only well described, but their causes are sensitively analyzed. The book is somewhat overwritten (actual conflicts and their bases repeat themselves in the last few chapters), and in the first chapter the four analytical alternatives don't come through as clearly as they might. But these blemishes are minor and certainly not unique.

The overall quality of Levine's book is good for an edited volume and there is a good deal of unity here, provided naturally by the material itself. The underlying issues are very roughly similar in many of the Latin American countries: what about the role of the lay poor and the strong preference of some of the lower clergy for them? What about repressive military regimes that attack the poor and the priests who defend them and thereby require a response from the hierarchy whether it initially wishes to take a critical stand toward repres-

sion or not. When dramatic political events (large-scale violence) take place and dramatic socioeconomic situations (mass poverty, deepened over time) present themselves, both reaction and no reaction are political, as Levine again points out. The contributors to this volume are among those who have stayed longest with the field: Phillip Berryman, Michael Dodson, Thomas Bruneau, and Brian H. Smith; but there are also some welcome newcomers: Susan Nelson, Charles Reilly, Mainwaring, and Thomas Kelman. Their case studies of Brazil, El Salvador, Nicaragua, Chile, Colombia, and Bolivia—as well as Levine's more general essays—all focus on the multiple implications for church structure of the increased activism of the poorer laity, led by sympathetic priests. They are of high quality.

In order not to be accused of trying to join a mutual admiration society, let me make the following point, which is meant seriously, and applies to much of Latin America writing: We were certainly weary of the mechanical application to the facts of life of excessively abstract schemes such as those of Parsonian functionalism. We—at least I—subsequently became weary very quickly of the equally mechanical application to the facts of life of excessively abstract schemes of a neo-Marxist and dependency kind. But there is a place for middle-level generalizations—if not about "institutions such as the church and others similar," then at least about "the church." Ivan Valliers's attempted stage-type conceptualization aroused a great deal of (understandable) ambivalence after its appearance in the late 1960s; though many used it, if only as a foil. But we do need new, courageous—probably flawed—attempts of this kind. These essays (and Mainwaring's monograph) reach the level of thoroughly sound but nevertheless essentially ad hoc reflections on current events. There is absolutely nothing "better" than such first-class journalism. But there is something different. And some of our thought, as academics, should be devoted to that difference.

HENRY A. LANDSBERGER

*University of North Carolina*



**African Politics: Crises and Challenges.** By J. Gus Liebenow (Bloomington: Indiana University Press, 1986. xii, 305p. \$32.50, cloth; \$12.50, paper).

**Studies in Power and Class in Africa.** Edited by Irvine Leonard Markovitz (New York: Oxford University Press, 1987. xv, 400p. \$32.00, cloth; \$14.95, paper.)

If anyone doubts that the study of African politics remains sharply divided between work influenced by modernization theory and work influenced by neo-Marxist theory, he or she needs only to examine these two important books briefly and such doubts will vanish. Both books are quite general in their coverage, and both are intended for use in the classroom, but they differ remarkably in substance. Liebenow deals with the impact of colonial rule, the challenges of nation building, the crisis of enduring poverty, political parties, military coups and military regimes, and U.S. policy toward Africa, but in all of this he makes only passing reference to class. In contrast, the latter concept and its relation to power in and over the state are central to the analysis presented in all chapters of the Markovitz collection, chapters that also cover many of the topics discussed by Liebenow.

Modernization theory is reflected at many points in Liebenow's analysis but does not explicitly dictate all aspects of that analysis. He defines modernization rather loosely as an attitude favorable to change in the direction of a broader scale of social interactions and a belief that such change can be brought about by human beings of all kinds working together. Nation building is an important component of modernization because it involves overcoming barriers of ethnicity, race, language, religion, and urban-rural differences to establish a larger national community. Class barriers are not mentioned, however, in this otherwise excellent discussion.

The crisis of enduring poverty, although acknowledged to stem from a variety of factors including the residual effects of colonialism and a sometimes hostile international environment, is blamed primarily on the policy failures of African governments. In the absence of a class explanation for these failures, they are blamed on unrealistic ideological commitments to industrialization, socialism, and self-reliance. What should replace these

commitments, in Liebenow's opinion, is not an equally unrealistic commitment to capitalism but rather a pragmatic mixture of institutions involving popular participation and an emphasis on agriculture in the immediate future, combined with a gradual move toward modest industrialization later on. These are extremely sound recommendations, but the reasons they have not been implemented in many African countries and are in constant danger of being abandoned in those countries in which they have been implemented have to do with social structures and international constraints as well as ideological commitments. The latter cannot be understood apart from their interaction with the former. In Africa, class interests have often transformed the ideology of socialism into the reality of state capitalism, which fails to follow Liebenow's recommendations for alleviating poverty for reasons very different from those identified by examining socialist ideology alone.

The Markovitz book contains chapters of varying quality, although the average quality is high. The editor's introduction contains an extension of the theoretical approach that he presented in *Power and Class in Africa* (1977), particularly with regard to the combination of the public and private sectors by the ruling class as the new base of their power, the limits of dependency theory, and the consequences of policies such as indigenization for class formation. The ways in which each chapter ties into one or more of these themes or those covered in Markovitz's earlier book are briefly explicated.

The most interesting chapters to me—those by Beverly Grier, Martin Kilson, Thomas Callaghy, Ndiva Kofele-Kale, Ronald Libby, Stanley Greenberg, and Hermann Giliomee—illustrate the wide variety of methods by which, and contexts in which, relations of class and power are analyzed. The first two authors write about the colonial Gold Coast, utilize an historical approach, and are very informative. In addition, Kilson has a fascinating theoretical discussion of the limits of African populism as an instrument of change. Callaghy's chapter essentially summarizes parts of his book, *The State-Society Struggle: Zaire in Comparative Perspective* (1984), which uses the approach of comparative historical sociology. Kofele-Kale's chapter presents quantitative data on the regional dis-

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tribution of public offices and other resources in the Cameroon, which are effectively integrated into his argument about the use of regional cleavages by the Anglophone bourgeoisie in that country to further their own class formation.

A commendable point in both of these books is their effort to integrate the analysis of South Africa into their respective frameworks for analyzing African politics. For Liebenow, the clash of nationalisms in South Africa presents unusually severe problems of nation building; although he also emphasizes the degrading and exploitative character of apartheid as it is currently practiced. Libby's chapter in the Markovitz book demonstrates that there is a strong class cleavage among white Afrikaner business people that may lead to party realignment, as those owning small businesses join workers in the Conservative party. Greenberg and Giliomee show that the Bantustans do not contain effective labor markets, reinforcing the case that they are places permanently to keep people who are totally devoid of privileges in order to reinforce class differences between them and those who are given limited privileges through obtaining jobs in the "white" areas.

JAMES R. SCARRITT

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**Liberia: The Quest for Democracy.** By J. Gus Liebenow (Bloomington: Indiana University Press, 1987. xiii, 336. \$35.00, cloth; \$12.95, paper).

This book is likely to be *the* one-volume current information and background source on Liberia, Africa's oldest republic and a country with special historic ties to the black America, for some time to come. Liebenow is one of the pioneers of United States-based African studies in general and of Liberian studies in particular, with several landmark works to his credit (such as his 1969 book, *Liberia: The Evolution of Privilege*). As was the case with his earlier book, this one will not endear him to the current ruling elite in Liberia, as Liebenow throughout proclaims his desire to encourage democratic changes in Liberia and has taken a leading role in this country in attempting to change U.S. government policies toward the

Doe regime—by testifying before congressional committees, for example.

The book is divided into three parts: an excellent initial part—nearly half of the book—covering the "First Republic" and Liberian political culture; a second part giving a "prelude and perspective" on the 1980 coup, which ended the First Republic and the dominance of the traditional settler elite in a hail of bullets; and a five-chapter final part entitled "Second Chance for Democracy: Promise versus Performance," which details the initial high hopes engendered by the coming of the "new order" in 1980 and the gradual disillusionment of those who hoped for representative government, as Samuel K. Doe's government reverted to many of the practices of the discredited True Whig administrations of the past.

*Liberia* is well written, with an intimate knowledge of the dramatic events of the past decade in Liberia tempered by an appreciation of the complexities of Liberia's past. Liebenow makes appropriate comparisons with other African situations on occasion, which help place the work in the broader African context. The national elections of October 1985 and the subsequent attempted coup the following month are closely analyzed, with Liebenow taking the Reagan administration to task for failing to take more vigorous actions against the (then military) Doe government at the time. (Doe was installed as the first president of the Second Republic in January of 1986.) Events may soon substantiate Liebenow's suggestion that President Doe's regime may not survive much longer, but the reasons for Liberia's ongoing crisis are clearly spelled out here.

This is an excellent book, and belongs in general (including public) and specialized libraries alike.

CHARLES HARTWIG

*Arkansas State University*

**The Political Economy of Latin American Defense Expenditures: Case Studies of Venezuela and Argentina.** By Robert E. Looney (Lexington, MA: Lexington Books, 1986. xxiii, 325p. \$39.00).

The subtitle of this volume is misleading. General titles with specific subtitles often constitute transparent attempts to attract a wider audience than case studies of a particular country or two can muster. But this book does not concentrate exclusively on Venezuela and Argentina. It begins with a statistical analysis of determinants of defense expenditures in the entire third world, then presents comparisons between Latin American and non-Latin American countries, and then focuses on Latin American countries exclusively. Only then does the author turn to a discussion of Venezuela and Argentina.

Looney reports that for third-world countries in general, as well as Latin American countries in particular, resource availability is the most important factor in an explanation of defense expenditures and in determining whether a country will become an important arms producer. For arms producers in Latin America (but not for nonproducers), the overall impact of military expenditures on economic growth is positive. An examination of budget trade-offs for Latin American countries reveals that arms producers experience negative trade-offs between defense expenditures and social welfare expenditures, while non-arms producers enjoy "positive trade-offs" between those two budget categories.

Regarding Venezuela, Looney reports that increases in defense expenditures are associated with decreases in other major areas of government expenditures and that in the future Venezuela's defense expenditures are likely to maintain the stability they have shown in recent years. With respect to Argentina, Looney's major finding is that its military allocations are most importantly a function of long-term economic trends, affected only secondarily by changes in political regimes. Even so, he finds that military regimes in Argentina have consistently outspent civilian governments on defense. For the future, Looney anticipates that Argentina's military expenditures should increase steadily, growing at 2.3% annually from 1984 to 1990.

Ultimately, however, I wonder whether any-

thing is really established by this book except the author's ability to computer-analyze mountains of data and his dogged persistence in attempts to interpret reams of computer printout. My doubts stem not from a general skepticism about quantitative analyses; on the contrary, I am quite sympathetic to such efforts in principle. What I find lacking here to a disturbing degree is theoretical discipline.

The reader will find in this book almost a hundred tables in a little over three hundred pages. However, the variables in the tables and related formulas seem to be selected entirely according to the size of their contribution to a high *r*-squared. For example, at one point Looney tests a hypothesis about the impact of military expenditures on economic growth in 10 arms-producing Latin American countries. The regression equation produces an *r*-squared of 96.7. But the data in the equation pertain to military expenditures in 1981, while the economic growth figures cover the years from 1970 to 1982 (p. 54). In other words, the "theory" on which this analysis rests fails to take into account the rudimentary logical requirement that the alleged cause precede the reputed effect.

The author reports that one of the factors that best discriminates between arms producers and nonproducers among Latin American countries in 1979 to 1980 is growth in exports from 1960 to 1970. He makes an attempt to "explain" this finding (p. 43), an effort that is especially valiant in light of the fact that (predictably) the form of the relationship changes entirely (from negative to positive) if export figures from 1970 to 1982 are substituted for those observed in earlier years. But his valor is best described as quixotic in its confrontation with the inherent implausibility of such *ad hoc*, *post hoc* "explanations" of relationships that emerge from analyses based on so many tentative candidates for independent variables (roughly 34, in this case).

Finally, it is ironic, in reading a book ostensibly devoted to the *political* economy of defense expenditures, to discover in chapter 2 (p. 38) that "a number of political variables were added to the data set. . . . The results were not, however, an improvement over those obtained from the eight socioeconomic variables initially selected." So much for the political variables. We are not even told what they were. All we can infer with confidence is

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that they failed to contribute to a high  $r$ -squared, and therefore, as far as Looney is concerned, they are of no interest.

Area specialists are not typically sympathetic to quantitative approaches. A book like this one is not likely to overcome their resistance, notwithstanding its laudatory preface by Martin Needler.

JAMES LEE RAY

*Florida State University*

### **Land Reform and Democratic Development.**

By Roy L. Prosterman and Jeffrey M. Riedinger (Baltimore: Johns Hopkins University Press, 1987. xii, 314p. \$29.50).

This is a book of advocacy. Its authors wish to alert us to the necessity of carrying out land reform in the third world before the year 2000. If such reform is carried out, they argue, then it will be possible to avoid "at least half a dozen great civil conflicts . . . which might well carry a combined death toll in the millions" (p. 1). In addition, some 100 million hunger-related deaths and 200 million births will be avoided. Finally, land reform will save some 1 billion people from Marxist despotism. The authors assert that these four benefits of land reform are not merely "fanciful predictions" but are "to be expected with considerable confidence" (p. 2).

Prosterman and Riedinger arrive at their predictions after many years of study of land tenure problems around the world. Their book is broadly comparative, with illustrations drawn from throughout the world. Their most detailed analyses, however, come from an examination of South Vietnam and El Salvador. The book begins by arguing that relative deprivation is the cause of revolution and civil violence. It is not poverty in general, however, that causes these upheavals; rather, it is that kind of poverty whose cause can be directly attributed to human agents. Since most people in the third world earn their livelihood from tilling the soil, their grievances are largely related to land tenure issues. Hence, instability is linked to land tenure, specifically landlessness. The authors then show that when the percentage of landless families to total families in the population is high, there is a direct link to instances of political instability

and to poor performance of the agricultural sector. Four different models of development are reviewed (termed the *family farm model*, the *collective model*, the *populist model* and the *nonagricultural incomes model*), with the family farm receiving the highest marks. The book then shows how the family farm model, often dismissed as incompatible with the conditions found in the third world, can be tailored to fit these conditions.

The work can be evaluated on three levels. First, as a study of land reform it provides a great deal of useful information, some of it statistical and some descriptive. The descriptive information is by far the more useful since it is based largely upon the hands-on experience that the authors had in Vietnam and El Salvador. Second, as a prescriptive work it fails because of the overzealousness of the tone. The wild claims made in the introduction merely serve to warn serious scholars and policymakers that the authors are so deeply committed to land reform that they likely lack objectivity.

It is the third level of analysis, the one dealing with the importance of land reform in third world stability and development, that is likely to be of greatest interest to the broadest range of scholars in the social sciences, particularly those concerned with the field of political economy. The volume places singular emphasis on landlessness as the most important variable in predicting mass political violence. This emphasis is at odds with two other approaches in the literature. First, empirical studies, beginning with the pioneering work of Bruce Russett, have not concentrated on landlessness but have centered instead on land distribution, often measured by the Gini index. Prosterman and Riedinger convincingly argue that measures of land distribution usually ignore the size of the agricultural sector and therefore are unable to weigh the importance of unequal land distribution to the society as a whole. But the importance of this contribution is mitigated by the widespread awareness of it, beginning with Huntington's landmark work of 1968. Second, the focus on a single variable is at odds with the multivariate tradition that defines modern empirical social science. We tend to shy away from single-variable explanatory models, since few stand up under rigorous statistical testing. A test of the landlessness thesis across 60 countries carried out by

Edward N. Muller and myself (*American Political Science Review*, 81 [1987]: 425-52) found a significant correlation of trivial magnitude with the 1973-77 death rate from political violence. When income inequality is added to the equation, landlessness is reduced to nonsignificance. The multivariate causal model developed in that study found that the landlessness variable was totally irrelevant, while agrarian inequality was relevant only when mediated through income inequality.

The volume is highly recommended for upper division and graduate courses on development, as it can serve to provoke likely classroom discussion. It will also be of considerable use to those who seek an insider's view of land reform in Vietnam and El Salvador.

MITCHELL A. SELIGSON

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**Paper Stones: A History of Electoral Socialism.** By Adam Przeworski and John Sprague (Chicago: University of Chicago Press, 1986. vi, 224p. \$24.95).

Throughout much of the twentieth century democratic socialist parties have been major actors on the political stage in most Western countries. In the past decade, however, many of these parties have suffered serious electoral reversals; and debates about their future prospects have engaged party activists and scholarly observers alike. Przeworski and Sprague's *Paper Stones* makes a valuable contribution to these debates and by so doing facilitates understanding of the dynamics of party support in liberal democratic political systems.

The authors' point of departure is a variation on the familiar "dilemma of democratic socialism." Historically, as debated by socialists themselves, this dilemma concerned the consequences of participation in "bourgeois elections" and, more particularly, the consequences of electoral success. The latter was an expected consequence of the former because it was assumed that the working class would inevitably become an "overwhelming majority" and would support socialist parties. Once in power these parties would face the decision of whether to institute "reforms" to alleviate the misery of the working class. The presumed dilemma was that such reforms would lessen

the radicalism of the working class, thereby undermining electoral support for Socialist parties and thwarting their long-term goal of instituting the socialist commonwealth.

For Przeworski and Sprague the dilemma is somewhat different. They emphasize that socialist parties entered the electoral arena when the working class was not an "overwhelming," or indeed even a "bare," majority. Thus the immediate problem for these parties was not what to do with power but how to get it. Moreover, since the working class (defined by the authors as "manual workers in mining, manufacturing, construction, transport, and agriculture" [p. 172]) has not become a majority in capitalist societies, this problem has persisted. "Leaders of socialist parties must repeatedly decide whether or not to seek electoral success at the cost, or at least the risk, of diluting class lines and consequently diminishing the salience of class as a motive for the political behavior of workers themselves" (p. 3). Przeworski and Sprague hypothesize that historically this risk has been both real and substantial.

To determine whether Socialist parties have encountered a persistent vote trade-off attendant upon pursuing "pure class-only" versus "supraclass" electoral strategies, Przeworski and Sprague analyze party support patterns in seven countries (Belgium, Denmark, Finland, France, Germany, Norway, and Sweden). Using longitudinal data on vote shares and class composition they estimate the magnitude of the opportunity costs socialist parties have incurred by adopting one of these strategies. The modeling techniques employed are elegant; and the results thereof, thought-provoking.

A principal finding is that the short- and long-term opportunity costs borne by Socialist parties for pursuing supraclass strategies are almost always positive (the very long-term results for France being the sole exception); that is, votes among the working class are lost when these parties gain support among other classes. In cross-national perspective, however, the equilibrium value of the trade-off varies sharply. In some cases, supraclass strategies produce increments in vote shares, but in others pure class-only strategies are advantageous. Comparing political contexts leads the authors to conclude that opportunity costs covary with the strength of corporatist institu-

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tions (principally the strength, organization, and party affiliation of trade unions) and the existence of communist party competition for the working class vote.

Using their opportunity-cost model Przeworski and Sprague also consider why the vote for socialist parties eventually stabilized at a plurality level and why, now, these parties face the prospect of secular decline. They conclude that the class structure, that is, the minority position of the working class and the nature of the trade-off between working and nonworking class support have powerfully influenced the historical development of electoral support for these parties, and will continue to do so in the future. Other relevant factors include relationships between socialist parties and trade unions and the strategic choices (sometimes mistakes) made by socialist party leaders. Within the authors' frame of theoretical reference, the argument about the past is powerful; that about the future, less so.

When forecasting the likely decline of socialist parties Przeworski and Sprague place great weight on the observation that the working class (as they define it) constitutes a diminishing proportion of the electorate in all Western countries. It is also true, however, that workers remain a sizable segment of these electorates; and it is by no means certain that Socialist party leaders, particularly in countries where the trade-off between working and nonworking class support is not especially sharp, will be unable to construct supraclass coalitions that are sufficiently large to win elections. The possibility for doing so would seem inherent in the authors' emphasis on the importance of party strategies: "the assumption that public opinion is exogeneous [*sic*] to party strategies is absurd. . . . Parties mould 'public opinion': they present the public with images of society, evoke collective identifications, instill political commitments" (pp. 125-26). Given this and the additional consideration (which the authors admit) that there is a set of significant "valence" issues that cut across class lines, the future of major Socialist parties would not seem to be necessarily bleak, particularly in the near and medium term. The authors may be correct to conclude that "the era of electoral socialism may be over" (p. 185), but their analyses do not provide "clinching evidence" on the point.

Another weak point concerns the use of sur-

vey data. The authors rightly recognize the utility of such data for bolstering their argument; but some readers will be disappointed with what they have done or, better, have not done in assessing the dynamics of individual-level behavior. This is perhaps particularly the case with regard to the hypothesis that "once workers vote socialist they are not very likely to change, even if party strategies do change" (pp. 60-61). Evidence on this point is very relevant to the central argument about vote trade-offs. Except in the long run, where considerations of voter replacement come into play, the existence of such trade-offs requires that the hypothesis be *false*. Testing it with appropriate panel data would have been an interesting and valuable exercise.

The conclusion that Przeworski and Sprague's analyses are neither complete nor convincing in every respect should not blind one to the many strengths of their book. *Paper Stones* is a theoretically rich and methodologically sophisticated contribution. Perhaps most importantly, it brings a much-needed long-term dynamic perspective to a field too long dominated by static analyses and a focus on essentially stochastic influences on short-term trends. That the authors may not be able to persuade all of their readers about the likely future of democratic socialism is not a major failing. That they have helped us to understand its past and present is quite sufficient.

HAROLD D. CLARKE

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**Women and Political Conflict: Portraits of Struggle in Times of Crisis.** Edited by Rosemary Ridd and Helen Callaway (New York: New York University Press, 1987. xi, 246p. \$38.00).

The editors of this collection, both social anthropologists, use the theme of public conflict to explore the conditions that render women powerless and those that provide opportunity for women to enhance their power. The case studies contribute to the growing body of theory on gender and politics by distinguishing between *women's powers* and *men's power*: "women's powers as being

more diffuse and individualized outside the bureaucratic structures of society; men's power as being more co-ordinated and structured within an institutionalized framework" (p. 3). The focus is on a relatively narrow spectrum of political conflict, and this is a source of both strength and weakness in the book: *strength* because some of the conflicts in question have been understudied but *weakness* because of the thin body of theoretical material available for integration into the studies.

The conflicts that are studied, with the exception of Homa Nategh's chapter on Iran, lie between the evolutionary and the revolutionary that are the substance of most studies of women and political change. Several of the conflicts are interesting precisely because they illuminate gender roles in circumstances seldom understood outside the immediately affected communities—Cyprus in the 1970s, the 1984–85 coalminers strike in Britain, and Brittany in western France, for example. In one way or another all of the authors were personally involved in the conflicts about which they write; and although this personal involvement provides the richness that often comes with participant observation, it also carries with it the drawbacks of political bias. Homa Nategh's anger at the course the 1979 Iranian revolution took, for example, raises the question of both the sources and the perspective available to an Iranian academic forced into exile in the wake of the revolution.

The book contains eight case studies: in addition to those on Cyprus, Britain, Brittany, and Iran noted above, the chapters cover Northern Ireland in the 1970s, Palestinian women in Lebanon, gender relations in the Israeli kibbutzim, and the Turkish community in West Berlin. The distinction between *women's powers* and *men's power* is not explored in any systematic fashion, but the narratives do suggest clearly that while women's powers emerge in unexpected and often creative ways in time of strife, these powers remain peripheral to male power, which is institutionalized. Moreover, women's powers are only recognized and appreciated when they are exercised in the name of, and on behalf of, domestic roles and responsibilities. As Callaway herself concludes, these brief ethnographies "give little evidence . . . that through their efforts women have gained any greater share in the making of public policy or even of

decisions in areas which affect their own lives as women" (p. 228).

In their introductory and concluding chapters, the editors suggest some reasons for the inability to translate private and even public struggles into public power: the vulnerability of women to sexual violence, conformity to cultural, religious, and class expectations; and competition from household obligations. The most provocative hypothesis to emerge is that women are most effective when their political activity serves to reaffirm the cultural identity of their communities and that probably as a consequence little in the way of traditional gender relations is altered in either the private or the public sphere. In Northern Ireland, Lynda Edgerton observes, "the collective power and social identity that women acquired through their public activity have not been reflected in an altered position within the home" (p. 80). "The status and respect awarded by the community to mothers of Palestinian martyrs," note Rosemary Sayigh and Julie Peteet, "epitomises the politisation of women's reproductive role" (p. 122). Women active in the British coalminers strike were fighting to save coal communities, which are "close-knit, heavily male-dominated and traditional" (p. 97). Under certain conditions, assertion of traditional culture becomes a radical political statement. This is somewhat true in the Palestinian camps in Lebanon and, as Clare Kroyzl stresses, especially true for Turkish women in West Berlin: "If Germans see the veiling of Turkish women as a sign of outmoded tradition and women's subordination [*sic*], Turkish women use it as a deliberate political act oriented toward the future and demonstrating control over their lives. . . . As a political act, the practice of veiling can be seen as conservative in reinforcing traditional gender roles and radical in its conscious rejection of the ascribed status of Turks in Berlin" (p. 206).

*Women and Political Conflict* offers no new methodological approaches to studying gender and politics. The case studies are descriptive and often anecdotal, and limited—geographically and culturally—to Western Europe and the Middle East. The studies do, however, provide new insights into the opportunities, and particularly the limits, of noninstitutionalized power. Women may be mobilized in times of political conflict, but just as often they are

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restrained by violence, culture, state, or religion. "Women's powers" are furthest from being realized when all four of these factors reinforce each other, as in Cyprus after the Turkish invasion of 1974, which left many Greek Cypriot women without either status or protection, or in Iran after the 1979 revolution and the war with Iraq that quickly followed.

SUE ELLEN M. CHARLTON

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**Political Organizations in Socialist Yugoslavia.** By Jim Seroka and Rados Smiljkovic (Durham: Duke University Press, 1986. xxvi, 332p. \$49.50).

The literature on Yugoslav political institutions falls roughly into three categories: hostile; sympathetic but critical; and laudatory and uncritical. This volume, one of the few books in recent years to address the question of the structure and functioning of political institutions in the Yugoslav self-management system, attempts to find a niche in the second category but all too often falls into the third. The volume is at its best when describing the political institutions which are at the heart of the self-management system; even the expert will find much that is useful here. But the work falls down when it comes to describing how these institutions function. All too often the authors confuse the ideals of the system with its reality. Shortcomings are identified but treated as unfortunate deviations that can be overcome with patience and goodwill. The electoral system in place prior to the 1974 constitutional revisions is subjected to repeated criticism, even though it permitted more than one candidate to run for election and was far more democratic than the method of indirect elections adopted in 1974. The conclusions on the Party, which are the heart of the book, read like an annual report issued by a corporation or university:

Our fundamental conclusion is that the Yugoslav political organizations have usually been effective in fulfilling their roles within the legislative system and process. The political organizations have neither abused nor neglected the use of their powers and capabilities within the legislative domain. They have maintained a careful balance of activity that has permitted the Yugoslav legis-

latures on the federal and republic levels to survive, to develop, and to prosper. (p. 236)

This conclusion, and others like it, seem out of place in light of the deterioration of the Yugoslav system (in all its particulars) after 1980.

Three factors help contribute to this lack of accurate and incisive analysis. One is the collaborative nature of the work. At times the two authors—one a U.S. and the other a Yugoslav political scientist—seem to be taking diametrically opposed views. On other occasions they seem to have settled for blandness and empty phraseology, as in the example just cited.

A second reason for the lack of incisive analysis lies in the reliance of the authors on quantitative empirical data. These data, drawn largely from sample surveys and other aggregative statistics, are not without interest. Unfortunately they tell us very little about how the political institutions in the self-management system actually function. At times the data are presented selectively; for example, figures on the number of strikes are given for the period 1964–69 but not for later years. The limits of the method are revealed in the authors' refusal to comment on the degree to which the Party has become federalized on the grounds that "the federalization of the Party is a very difficult issue to measure with the use of empirical evidence" (p. 75). (In this connection it should be noted that the lengthy discussion of democratic centralism in the League of Communists of Yugoslavia [LCY] nowhere indicates whether lower bodies are required to carry out the decisions of higher bodies, a central tenet of the concept of democratic centralism as it has been applied in Communist systems.)

A third reason for the low level of critical analysis lies in the assumption of the authors that political institutions are necessary to guide the self-management system and adjudicate among interests. Thus the period up to 1974 is pictured as one of growing confusion and weakness; while the post-1974 system, with strengthened party controls, is seen as a step forward in the development of self-management. This puts the book at opposite polls from the only other in-depth analysis of the party in recent years, April Carter's *Democratic Reform in Yugoslavia* (Princeton, 1982). While the Party does play a crucially important role in making the system function (and,



vainly for the most part, in trying to adjudicate among interests), it does not follow that by strengthening the Party one has strengthened self-management. Judging from the corruption and abuses of power uncovered in the LCY in recent years, the system needs less, not more, party control.

The volume is not lacking in recommendations for change or in comments critical of this or that aspect of the self-management system in Yugoslavia. But what is desperately needed, at present, is a better understanding of how the Yugoslav political system could have fallen so far so fast since Tito's death. The reader will seek in vain for the answer in the Seroka and Smiljkovic volume, which appears to bend over backwards not to offend Yugoslav sensibilities when dealing with the shortcomings of the self-management system.

PAUL SHOUP

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**Public Violence in Canada, 1867-1982.** By Judy M. Torrance (Montreal: McGill-Queens University Press, 1986. 270p. \$32.50).

This book is an ambitious attempt to examine the sources, character, and responses to, public violence in Canada. Torrance has assembled a great deal of historical, sociological and psychological information and produced several suggestive interpretations of both Canadian politics and Canadian culture in general.

Torrance begins her work by defining *violence* as "the infliction of physical harm on people or their property" (p. 9). Having eliminated nonphysical harm, she further narrows the scope of her study by noting she will be mostly concerned with "public" violence, which she describes as "those incidents that are widely regarded as having a significant impact on society or an important part of it" (p. 14). It is possible, of course, to take issue with these definitions, but they are presented along with discussion of other possibilities and certainly her choices constitute a reasonable basis for scholarly exploration.

Discussion of the literature regarding violence permeates the book, but little if any of this information will be new to those interested

in the field. In any event, Torrance is concerned with applying these theoretical approaches to analyzing violence to various Canadian cases; and it seems appropriate, therefore, to focus this review on that effort.

Perhaps the greatest difficulty with an attempt to examine public violence in Canada is that there has been so little of it. For example, only one incident of public violence in Canadian history, that is, the Northwest Rebellion of 1885, resulted in more than nine deaths. For this reason much of Torrance's work is aimed at an explanation of why Canada has been a comparatively nonviolent society. Not surprisingly, she finds that Canada's history, its political culture, and the actions of its governments offer partial explanations.

Torrance's treatment of what she calls the myth of the "Peaceable Kingdom" is particularly interesting. She argues that the perception of Canada as being, and needing to be, a peaceful society, especially when compared to the United States, has had a powerful effect in shaping the attitudes of both political leaders and the Canadian people as a whole. This perception has tended to stigmatize both public violence and the nongovernmental organizations promoting it as being "un-Canadian" and therefore illegitimate. In consequence, these beliefs have also provided the authorities with strong justifications for applying stern measures to suppress public violence. Torrance further notes that in many instances the occurrence of violence in Canada has been precipitated by governmental actions rather than those of discontented groups. These governmental actions, however, have been popularly approved as being necessary for the preservation of "peace, order and good government." Indeed, her discussion of the role of government in the management of violence in Canada is among the most valuable portions of the book.

I believe that the portrait of Canada that Torrance paints is generally accurate and that her book will be an important contribution to understanding public violence and Canada. Having said this, I also think the author has used far too much space in attempting to relate all potentially relevant theories of violence to the Canadian experience. Very often this diligence results in what might be characterized as an "on-the-one-hand-but-on-the-other-hand"

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type of evaluation. These assessments are no doubt judicious, but they result in tedious reading and the impression that almost everything is explanatory, but not very. In the conclusion of her book Torrance acknowledges the often limited value of much of the theorizing about public violence and proceeds to offer a sensible listing of "eight factors that ought to be taken into account when seeking to explain an incident or pattern of violence."

*Public Violence in Canaan* is, I believe, a thoughtful book and can be read profitably not only by those interested in Canada but by anyone who is interested in the fields of political stability, collective violence, and internal war.

WAYNE G. REILLY

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**Open Borders, Nonalignment, and the Political Evolution of Yugoslavia.** By William Zimmerman (Princeton, NJ: Princeton University Press, 1987. ix, 158p. \$22.00).

William Zimmerman's book is intended to be a "stocktaking of important trends in Yugoslav political development" (p. 5). Yugoslavia's change from a closed, Soviet-type system to an open one after the 1948 Cominform expulsion is studied from three perspectives: (1) national-international linkages; (2) connections between the external environment and the evolution of Yugoslav institutions and policy processes; and (3) linkage between Yugoslav and international economies and Yugoslav mass-elite relations.

In 1948 the Yugoslav leadership was forced to reformulate the direction of their domestic and foreign policies. The Yugoslavs aspired to play an important international role despite their exile from the Soviet bloc by promoting an ideology which differentiated them from both superpowers. Zimmerman contests the supposition, widely held by Yugoslav scholars, that nonalignment resulted from Yugoslav adherence to workers' control through self-management, the linchpin of its domestic policy. His argument is that self-management was "the domestic expression" of Yugoslav aspirations for increased international influence. Chronologically, of course, self-management forms were instituted long

before nonalignment became a policy option. Nevertheless, Yugoslavia's turn to the United Nations in the aftermath of the Cominform expulsion was a forerunner of its use of international mechanisms to preserve the domestic legitimacy of the Communist party and, one suspects, the integrity of ethnically divided postwar Yugoslavia. The fact that the Yugoslavs persist in tinkering with self-management speaks volumes for its importance to the elite, particularly since economists warn that persistence of some of its forms threatens deepening economic chaos for Yugoslavia. The major concern of post-Tito Yugoslavia is Soviet "hegemonism." Perhaps the major hope is that Yugoslav self-management may be deemed worth saving as a unique construct and that the West will continue to allow that Yugoslavia's nonaligned status must be maintained. In that case, Zimmerman's national-international linkage is brought up to date.

Zimmerman details Yugoslavia's problem of governing its workers abroad, whose large number qualifies them to be considered as Yugoslavia's "seventh republic." However, the "seventh republic" is able to influence the Yugoslav government in ways that the six traditional Yugoslav republics cannot; that is, the regime must avoid policy-making that interrupts the flow of remittances from the migrant workers. Furthermore, Yugoslav workers have greater exposure to Western ideas and culture (including conspicuous consumption) limiting even more Yugoslavia's ability for heavy-handed socialization. Zimmerman thus concludes about Yugoslav economic migration that "just as exit precludes Yugoslav totalitarianism," it also opens an escape valve that decreases the likelihood of antiregime and of pluralist mobilization (p. 143). Because the subject of migrant workers is such a difficult one both from the point of view of the sending and the receiving countries and because Zimmerman presents such a wealth of material on Yugoslavia's approach to the problem, this publication might have profited from some discussion and comparison of the problems that other sources of migrant labor, such as Turkey, share with Yugoslavia and how (or if) they are able to solve them. The reader with interests outside of Yugoslavia might also have benefited from learning more about how the EEC countries have reacted to the influx of Yugoslav labor and to what extent this has

altered relations between Yugoslavia and those countries.

While there are books aplenty about various aspects of Yugoslavia, Zimmerman's work has its greatest value in its focus on the effect of the international environment on internal political development. This study persuasively argues via the Yugoslav example that "plural penetration [by elements of the international environment] can increase a state's freedom to maneuver internationally and also allow its elites to maximize the resources available with which to pursue its internal development strategy" (pp. 145-46). It would be interesting to see if this theory corresponds as well to a country that is securely within a bloc as it does to a country that professes nonalignment.

The author's respect and concern for the reader are evidenced by his attention to organization and word economy. However, detracting from the quality of the author's presentation is the format of the footnotes. Inclusion of a shortened version of each footnote within the text was disconcerting as it broke the flow of reading and forced the incorporation of explanatory notes into the text. Happily, this is only a minor imperfection in a work of real value for both policymakers and analysts of Communist systems, toward whom this book is aimed.

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## INTERNATIONAL RELATIONS

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**Strategic Nuclear Targeting.** Edited by Desmond Ball and Jeffrey Richelson (Ithaca: Cornell University Press, 1986. 367p. \$29.95).

To nuclear strategy and in turn to all our defensive dispositions, nothing is more important than targeting—a point amply made by the editors and authors of this instructive and expertly written and compiled work. It is a collection of 13 chapters (including an introductory essay), by 13 authors, including the editors, Desmond Ball and Jeffrey Richelson. The other authors are David T. Cattell, Lawrence Freedman, Colin S. Gray, Michael Kennedy, William T. Lee, Kevin N. Lewis, F. S. Nyland, George H. Quester, Bennett Ramberg, David Alan Rosenberg, and David Yost. All are well known in the general field or subfields of nuclear strategy.

The format of this volume, with almost half of the chapters previously published in virtually the same versions, breeds some overlapping. The book is not a text or the structured and unfolding argument of a single author or study group or even a panel or conference, yet the volume has coherence and great current utility despite the somewhat ancient origin of some of the chapters. In fact, the editors have done a

considerable service to the strategic-intellectual community and, beyond that, to the defense policymaking community and the attentive public, in presenting in one place a comprehensive set of essays that focus sustained attention on the significant dimensions of the nuclear targeting question: historical, analytic, critical, and prescriptive.

Though targeting is "central" to nuclear strategy and national strategy, it is poorly understood—indeed in some ways perversely misunderstood—by the public. This is true even—perhaps especially—of those in the peace movement, who describe themselves as particularly "concerned" about the possible effects and the putative imminence of nuclear war, who base a large part of their discontent with present U.S. (and Soviet) nuclear strategy on the misperception that it (still) embodies an immediate, total, and mutual countercity salvo. (Actually, U.S. nuclear targeting doctrine did call for something approximating this, but only in the first primitive years of the United States possession of nuclear weapons, and even then, the lists included a much wider spectrum of military targets.) Thus, peace activists seize the "best" of both sides of the critical argument, assailing the instability that is more properly associated with counterforce

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targeting, and condemning the terminal destruction that is properly associated with counterpopulation targeting. But also, in a curious mirror image, the hawkish Right carelessly assumes that present nuclear targeting doctrine consists of the notorious "MAD" (mutual assured destruction). A careful reading of the essays in this book should disabuse anyone of such simplistic misapprehensions of targeting doctrine—though it may create other reasons for apprehension.

It would be invidious—as well as impossible in this space—to conduct a comparative evaluation or a detailed review of the contributions of each of the 13 authors. All the contributors bring to their essays the benefit of years of cool, exacting attention to weapons, strategies, characteristics of target systems, and the history of nuclear decision making in the United States and elsewhere. The general plan of the book is a history of targeting doctrine; a comparative discussion of the doctrines of the United States, Britain, France, and the Soviet Union; a critique of various types of targeting doctrines (countervalue, including deliberate killing of population; counterforce; attacks on political elites; countereconomic targeting, including destruction of nuclear power plants; concentration on the sinews of Soviet control of ethnic minorities); and a discussion of the problem of nuclear war termination.

A reviewer, casting his eye over this material, might ask, at the outset, why study targeting at such close range (that is, if one is not a "weaponeer" by vocation)? To give a sense of where targeting fits within its larger field, I divide nuclear strategy into two aspects: first, *posture* (the numbers and kinds of weapons; their characteristics of accuracy, range, and destructive yields of radiation and heat and blast; the delivery vehicles and platforms; their locations; their time urgency, vulnerability and speed to target; and the mechanisms of command and control) and second, *doctrine*, consisting of (1) precedence or occasion of use, that is, when they are to be used (in a first or second strike, in response to what provocation, in what sequence and numbers) and (2) *targeting*, that is, at what we aim our nuclear forces: whether countervalue, counterforce, or some variant of either.

It is essential to realize that targeting is "logically" connected with precedence of use (most poignantly, the implication of counter-

force with first strike) and also with the evolution of the tangible elements of nuclear posture. An example of the latter connection is the observation, made by Ball in the introduction, that not until five or six Single Integrated Operational Plans (SIOPs) from the first one in 1960 did the United States even have the technology, particularly that of accuracy, that would make it relevant to entertain a menu of sharply distinguishable targeting options.

Nuclear strategy is not merely an underdeveloped area; it is also a peculiarly perverse subject. What shines through the entire collection of essays is a paradox—or at least an irony: that rationality—of target selection, among other aspects—is not just hard to attain but self-contradictory. Precisely because rationality contributes to the limitation of destruction and the chance of reimposing intrawar deterrence, that is, stopping the destruction, it conflicts with the very irrationality whose prospect constitutes the most effective deterrent of nuclear war in the first place. This book invites us to ponder the blend of rationality and irrationality inherent in nuclear deterrence and nuclear war. In particular, the exacting critique of targeting *choices* should dispel the widely accepted and almost comfortable notion that nuclear threats and nuclear war proceed from "madness." That judgment leads not only to misplaced ascriptions of causality but to wrong, as well as contentious and distracting, remedies—such as those in George Kennan's article entitled "Cease This Madness" (*Atlantic*, January 1981).

This book incorporates the judgment that over long stretches of time, actual targeting doctrines had little to do with rhetorical policy; that targeting doctrines have been existential, in the sense that they were determined and limited by capabilities. This is not quite the more far-reaching claim of liberal critics that targeting doctrine has evolved through sheer mindlessness. There is also considerable ground for skepticism of tailoring effects to accomplish exotic strategic purposes and Machiavellian political purposes. First, there is the sheer degradation of command and communication, including the "hot line," that would occur in the first few minutes of a nuclear war. Second, even though U.S. doctrine since 1973 has explicitly excluded attacks against cities "per se," there are some residual-

ly limitless nuclear effects. And especially there is the high degree of collocation of military or leadership or political targets, or just about any other category of supposedly discrete targets with large bodies of population, particularly in the Soviet Union. Not to put too fine a point on this, the enemy might not understand—or might not choose to understand—that we didn't "mean" to destroy its cities; and, of course, then it would be all over for us, too. In fact, all 200 of the 200 largest Soviet cities, and 80% of the 886 Soviet cities with populations over 25 thousand would be attacked anyway, and thus various "noncity" attacks would kill an estimated 50 to 100 million Soviet people. Among other "unintended"—or, more grisly, intended—effects of attacks against Soviet nuclear power plants would be the release of radioactive substances from some 40 reactors, which would render 50,000 square miles of territory in the European USSR uninhabitable for at least 50 years. All of this calls into serious question the feasibility of "escalation control," on which limited nuclear options partially rest. And it illustrates that nuclear weapons are not the clear and precise signaling devices sometimes suggested in the more abstract literature on deterrence and compellence. The encompassing context is that in an initially deliberate, or even an expanding and ultimately all-consuming, nuclear attack on populations, about half a billion people would be killed "promptly" in the United States, the Soviet Union, and Europe, and other hundreds of millions would be extinguished by more slowly developing global effects.

One thing I miss, in the discussion of the derivation of U.S. targeting doctrine (though it does appear in the discussions of British, French, and Soviet doctrines), is an ultimate inquiry into *why* (in the sense of a reason, not just a blank history or a mindless technological drift) our current strategy has moved to an emphasis on counterforce, with such disabilities as the open-endedness of weapons and cost and the heightened crisis instability. There is a connection that runs very deep in our overall national strategy. Counterforce targeting is virtually dictated by our extension of nuclear deterrence over important allies, such as those of NATO. The thread runs through "damage limitation" (of our society, not merely of our offensive nuclear forces) as a function of our

offensive nuclear weapons; damage limitation is important to the credibility of our extended deterrence. Damage limitation, in turn, is facilitated by counterforce (and by strategic defense). Thus, in this one respect, this book fails to indicate what—and how much—would have to change, beyond the weapons systems and the deployments, if we should want to change our current targeting doctrine.

Does any common prescriptive advice emerge from this book? Perhaps this would be too much to expect, given that the book is an assemblage of views from some at least obliquely antagonistic parts of the defense intellectual community (for example, Colin Gray and Jeffrey Richelson). One summary conclusion, endorsed by almost all, is that any nuclear war might be so catastrophic that no combatant would "prevail," in the gross sense of this word. Even Colin Gray, an avowed proponent of not the desirability (!) but at least the relative feasibility of fighting a nuclear war, states that if prior attention is not devoted to damage limitation and intrawar deterrence, the initiation of even limited nuclear strikes would lead to "suicide on the installment plan."

Coeditor Ball, in the introduction, counsels that "it would be prudent to give further consideration to targeting strategies that avoid attacks on military, leadership, and economic-industrial targets in urban areas, since these attacks create a greater risk of climatic effects." Also, for other reasons, the various authors advise avoiding the targeting of such sophisticated subsets as the networks that control ethnic minorities and the bunkers of military and civilian leadership, as well as discouraging counterforce in its specific sense of targeting (presumably in a first, or early, strike) the enemy's nuclear missiles. But such severe limitation of target categories (and I, for one agree with the sense and thrust of such limitation) carries the implication that it might degrade deterrence. And since deterrence is the irreducible function of nuclear weapons, a function acknowledged even by those who assert that they have no "military" function, then we are back where we started. In the business of nuclear strategy, as in most other businesses, there is no free lunch—or, in this case, no free launch.

EARL C. RAVENAL

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**America's Changing Role in the World System.** Edited by Terry Boswell and Albert Bergesen (New York: Praeger, 1987. xi, 300p. \$37.95).

This book is concerned with whether the United States can continue as the dominant power in the contemporary international system, and the possible consequences that may emerge from a change in the pattern of U.S. hegemony. The book is divided into three parts. The first examines the U.S. decline in the context of continuity and change in world system structure. The essays in this section are devoted to the discussion of such topics as the U.S. response to the world economic crisis that has supposedly resulted from a downward phase in the long wave of international economic expansion and contraction, the political ironies that have emerged as U.S. power has waned, the degree to which the world system has undergone structural transformations since the Second World War, and the prospects for the sort of basic changes in the international system that would lead to the demise of the nation-state.

Put simply, the central thesis in the first section is that the ability of the United States to retain its hegemonic position is in decline. It is explained that this is a function of twin processes: (1) long waves of economic prosperity and recession, and (2) the inability of dominant countries to use their policy selections to guarantee their ascendancy over time. Both processes are attributed to the inherent functioning of the capitalist world system, in which constant competition for market domination and profits create tendencies toward such things as overproduction, a constant search for cheaper labor, and a continual jostling by lesser countries to upset the domination of hegemonic actors. It is argued that these historical shifts are inevitable and that the policy choices of actors will do little to arrest these processes. It also is noted ominously that in the past, war has been a key mechanism for facilitating changes from one hegemonic state to another. Therefore, the apparently imminent demise of the United States is discussed as being a reason for apprehension.

The second part of the book investigates some of the specific problems that have emerged as U.S. difficulties have multiplied. The problems examined include (1) the erosion

of innovative approaches to production and management in the United States and the attempt to borrow potential Japanese solutions; (2) the degree to which the international arena remains hierarchical even as U.S. hegemony recedes; (3) the nature of the international competition that confronts the United States and how it has changed over time; (4) the changing conflicts between the United States and the third world; and (5) how U.S. aid to third world countries has shifted over time and whether it is related to violent changes in governments.

On the whole, the book's greatest strengths are found in this part. As is stated at one point, "while the process of hegemonic rise and decline in world production is much discussed in world system and international relations circles, the specifics of the process are little understood" (p. 157). Enhancing understanding requires careful research. The work reported by the authors in this part of the book reflects this sort of research, with efforts being made to test systematically some of the contentions that often are associated with world systems theory. The discussion in the first part of the book would have benefited from similar attempts to substantiate the views that are expressed.

The book's final section considers how declining hegemony affects U.S. culture and society. The decline of U.S. art, shifts in the perceptions that U.S. citizens have of themselves and of the world, and the changing roles played by women in the world and in U.S. society are the topics that these essays address. With the exception of the essay on U.S. perceptions, these essays are only tenuously tied to the themes presented earlier. A concluding chapter designed to summarize and tie together the points made by the contributors would have been useful for overcoming this problem.

JOHN M. ROTHGEB, JR.

*Miami University*

**The Causes and Prevention of War.** By Seyom Brown (New York: St. Martin's, 1987. xiv, 274p. \$32.50).

Seyom Brown admits to casting his net "widely across ideological orientations and fields of knowledge" (p. vii) in his search for

the causes of war and strategies for peace. He reaps a rich harvest in the end, one whose final bounty is worth the attention and the patience needed to fish with him in what are sometimes familiar waters, at least for academic specialists in peace studies. The net is a large one, corraling relevant work out of all the social sciences and the views of strategists and statesmen, biologists, philosophers, moralists, and theologians.

At its best this work provides clear, well-reasoned summaries of the views of a Ghandi, Galtung, Kissinger, or Harold Brown, of the rationale for the Kellogg-Briand Pact, for UN peacekeeping, or for the NPT and SALT. Abstract issues in alliance building, deployments, diplomatic maneuvering, and much else that may cause or prevent warfare are often neatly illustrated with apt historical references, so that even the familiar may look invitingly fresh.

Yet the effort to gather everything in may not always discriminate clearly among what is more or less important to an understanding of this most critical of human issues. For example, chapter 7 presents a considerable sample of "peace plans"—from the seventeenth century to the present—for transforming the global political structure to minimize the amount and scope of large-scale violence. While one might argue that some of these are demeaned by the tiny space allotted them, more disconcerting is the effect of a section toward the end of the chapter "The Superpower Disarmament Charade." Its placement and the space given it suggest that the earlier proposals will always be subject to the cynical manipulation of those who speak for nation-states. The leave-no-fish-unchanged approach is the main culprit here, for it tends to induce an agnosticism with regard to value choices that may perplex, more than lead, those who need direction.

The book's real importance is in its long concluding chapter, "Toward an Integrated Strategy of War Prevention and Control." Here the great net is hauled in to show how an integrated peace strategy must operate in five interlocked spheres—morality, social structure, diplomacy, military capability, and military strategy—with Brown's conclusions as to what will enhance peace then laid out for each sphere. It is an impressive work of synthesis, departing from the accumulated findings of the previous nine chapters to winnow and com-

bine them into a convincing sketch of a strategy that is original in conception and highly provocative. For one hint of the symbiotic strength that results, Brown proposes that in the moral sphere, the number of people killed or tortured as a result of individual or group action generally determines the degree of moral evil of the action (p. 221). That premise informs his strong emphasis in the sphere of military strategy on the concept of firebreaks, of which he adduces four types—all intended to prevent escalation of combat to genocidal levels (pp. 252-57). For these reasons, when discussing the social structure, he advocates dismantling the major East-West alliances on grounds that "the principal risk borne by all coalition members—the prospect of local wars turning into global holocaust—has become a terrible . . . liability to the protectors and the protected alike" (p. 230).

The author does have a tendency to assume an inevitable disjunction between efforts to transform the system so that conflicts may be resolved peaceably, and "muddling through" in time-honored ways to avoid a holocaust. Yet as shown above, he also concludes that actors responsible to the current state system need to adopt radical changes in policy. It is more than a semantic quibble to note that, as these are implemented, they will begin to produce structural changes of the sort that will enhance the safety of the species over the longer term. The wisdom in this book is that the author never makes the best in any way the enemy of what is better. Nor should he—or we—forget that if the unacceptable risk of catastrophic violence can be transformed into one that we can live with, that risk, in turn, can be progressively diminished and the world made safer for us all.

LYNN H. MILLER

*Temple University*

**Third World Coups d'Etat and International Security.** By Steven R. David (Baltimore: Johns Hopkins University Press, 1987. xi, 191p. \$22.50).

During the past two decades the literature on military coups in the third world has mushroomed—at least in some fields of regional studies—at virtually an exponential rate. Yet

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few scholars have explored systematically and from a global perspective the outer parameters of such domestic upheavals. Too often ignored are the international dimensions and implications of military coups; the linkages between domestic coup plotters and external powers; and the praxiological policy dictates stemming from a foreign power's desire to either overthrow a third-world regime or to shield it from an internal upheaval.

Such is the main focus of Steven David's *Third World Coups d'Etat*. His six compact chapters commence with a summary outline of generic causes of military coups (utilizing an adaptation of Huntington's not very satisfactory typology) and their repercussions especially on the United States. Claiming that significant external involvement is visible in 38 instances out of some 357 successful coups in the post-1945 era, he then analyzes the experience of the United States, the Soviet Union and (in a very cursory manner) several other powers in supporting, deterring or crushing third-world coups. He concludes with a survey of several possible U.S. responses to the various international challenges posed by attempts at violent regime changes.

David's assessment is that the United States has promoted its global interests far more effectively and with greater success, through protecting Third World regimes from coups than through backing the overthrow of unfriendly regimes. Even when U.S.-supported or sponsored upheavals have met with success "the long-term cost . . . has often worked to the detriment of U.S. interests" (p. 68). The Soviet Union has more clearly evolved a contemporary strategy "designed to preserve Soviet gains rather than to expand Soviet influence" (p. 102) with the authoritarian, elitist single party, closed-society (often military-led) system prevailing in allied third-world states making its task much easier.

The author's recommendations to U.S. policymakers seeking to provide a bulwark for friendly foreign regimes against internal military upheavals are the inevitably shopworn ones of yesteryear, so uniformly ignored by most third-world leaders: encourage a drift towards democracy, popular structures, economic and political reforms, bridging the gap between rich and poor; limit societal corruption, inculcate civic values in the military; encourage regional pacts; provide military

matériel that will assure that direct U.S. involvement in the actual suppression of an upheaval is unnecessary; and encourage other powers more inclined to intervene (e.g., France, UK) to do the dirty work instead.

In general the specialist assessing David's work will here and there grumble over an occasional slight factual error, misinterpretation, cold warrior bias, or sloppy attention to detail in the notes section. (No bibliography is provided.) But all this would be in the nature of petty nitpicking for the author has masterfully, if at times somewhat superficially, synthesized a huge amount of data from a wide array of sources and weaved it with sufficiently convincing detail into his six chapters. There is thus in essence little that is wrong with what is in the book; however, far too many questions are raised by what is *excluded* from analysis under a variety of pretexts.

Specifically, foreign involvement in coup contexts is very narrowly defined as the author himself reminds us at the very outset, a fact that somewhat undermines the value of David's study. The work moreover relies exclusively on secondary sources (on the grounds that the global breadth of the study realistically precludes field research), not one of which is in the foreign languages. Insisting on "public, credible evidence that foreign involvement played a major role" in coup contexts (p. 2), David automatically precludes from analysis upheavals poorly understood or reported abroad, those in countries permanently shrouded by a veil of secrecy (e.g., in the Soviet orbit) or where foreign involvement is for a variety of reasons suppressed or glossed over by the host country. Similarly, excluding from consideration instances of heavy ongoing foreign support (e.g., France) for third-world regimes faced with internal threats *short* of an actual military coup may appear methodologically rigorous; but it fails to give adequate cognizance to the fact that in some countries the existing civil-military stability may be *solely* a function of the certainty in military circles that any coup would be immediately reversed by foreign force of arms.

As a result of these coup exclusions, Africa, for example—with some hundred coups and attempted coups to her credit—merits only casual attention and then only with reference to a few key upheavals. Despite the insights David strives to develop from his global per-



spective and notwithstanding numerous *references* to a variety of military coups in a host of countries, in reality the study rests excessively on data drawn from a limited number of upheavals in key countries. Hence the very uneasy feeling the reader is likely to have as he or she puts down the book, aware of its solid merits but also that it really presents only part of the picture. Clearly a wider net needs to be thrown out by the next scholar to approach this theme (with the "French connection" in client states specifically bearing investigation) if the very pertinent issue of the external parameters of third-world coups is to be more definitely analyzed.

SAMUEL DECALO

*University of Natal*

**Strategy and Force Planning: The Case of the Persian Gulf.** By Joshua M. Epstein (Washington: Brookings Institution, 1987. xiii, 169p. \$28.95, cloth; \$10.95, paper).

Joshua Epstein's book deals with a very important defense policy problem of immediate concern to the United States: What strategy and what force structure would provide the most credible deterrent to large-scale Soviet aggression in the Persian Gulf? To assess these questions, Epstein analyzes a case study of U.S. defensive options to Soviet attack options in the oil fields of Khuzestan in southwestern Iran. Three basic strategies are addressed: one is symmetrical (i.e., involves a reaction to threats "at the same location, time and level of the original provocation"), while the other two are asymmetrical (i.e., involve "shifting the location or nature of one's reaction onto terrain better suited to the application of one's strengths against adversary weaknesses") (p. 1). The *conventional tripwire-vertical escalation* (asymmetrical) strategy is discussed in chapter 2. Here "a small nonnuclear force would be quickly emplaced as a signal of U.S. commitment and as a tripwire that if violated, would ostensibly trigger nuclear employment against targets in the Soviet Union or Iran. The prospect of nuclear reprisal, rather than that of outright conventional denial, would carry the disuasive burden" (p. 6).

The *horizontal escalation* (asymmetrical)

strategy (discussed in chapter 3) involves "launching counter offensives with conventional forces against Soviet targets *outside* the Persian Gulf theater" (p. 6). According to Epstein, the major weakness of the asymmetrical strategies as a defensive response to Soviet aggression in the gulf, is their lack of credibility. He therefore suggests another alternative: the *conventional deterrence* (symmetrical) strategy (which is addressed in chapter 4). This strategy advocates the use of a U.S. rapid deployment force considerably smaller than that planned by the Reagan administration to deter Soviet aggression. According to Epstein, this is the most credible and the preferred defense strategy available to the United States in the Persian Gulf.

*Strategy and Force Planning* is a well-written and well-organized book and is easily read. It compares and evaluates different Soviet scenarios (e.g., the airlifted and overland attack options) and U.S. defensive options while employing threat assessment and force planning methods (detailed in a series of appendixes). Its use of dynamic simulation methods coupled with sensitivity analysis while offering an alternative formulation to Lanchester's equations is particularly innovative. The author's knowledge of Iran's geography and of Soviet and U.S. military options and vulnerabilities in the gulf is also impressive. Based on his analysis, Epstein recommends that in case of Soviet aggression in the gulf, the United States threaten to exploit Soviet military vulnerabilities while relying on a five-division, speedy, rapid deployment force (see chapter 5).

As is the case with some studies based on scenarios, however, Epstein's discussion of a number of issues seems speculative and leaves the impression that other attack options and counteractions should also have been considered. Moreover, Epstein's quantitative analysis is directed not at the comparison of the three basic strategies (a comparison that, although well presented, is entirely subjective) but rather at the issue of force structure and requirements. Furthermore, as the author himself has noted (p. 5), the generalizability of his analysis to other contingencies in the Persian Gulf is questionable. Despite these few limitations, however, the strengths of *Strategy and Force Planning* clearly outweigh its weaknesses. It is a very interesting and timely book,

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which deserves the attention of all serious students of national security policy.

ALEX MINTZ

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**The Political Economy of International Relations.** By Robert Gilpin with the assistance of Jean M. Gilpin (Princeton: Princeton University Press, 1987. xvi, 449p. \$45.00, cloth; \$9.95, paper).

The study of international relations in the United States has been dominated by two alternative paradigms, liberalism and realism. Robert Gilpin has been one of the most sensitive and illuminating exponents of the latter perspective. In *The Political Economy of International Relations* he distills and extends much of his earlier work. This study is both a shrewd analysis of the contemporary international environment and a summary of developments in the field of international political economy over the last two decades.

Gilpin does not believe that the prosperity and stability that characterized the early postwar period, when U.S. hegemonic leadership was at its height, can be restored. There have been fundamental structural changes in the international system, most importantly a decline in the power of the United States resulting from postwar recovery in the rest of the world, the astonishing performance of several East Asian countries, and domestic mismanagement of the U.S. economy, especially by the Reagan administration. Consumer spending, economic growth, and the defense buildup were made possible only because the Japanese were prepared to underwrite the massive federal budget deficit. The competitiveness of U.S. firms was eroded by overvaluation of the dollar, which altered consumer tastes and strengthened foreign producers. The United States went from being the world's major creditor to being its largest debtor. Never before had such a dramatic shift in international financial markets taken place in so short a time.

Gilpin sees little prospect for the emergence of pluralistic leadership that could replace U.S. hegemony and sustain a liberal order. The United States has increasingly turned toward

the pursuit of more specific and narrowly defined interests, which Gilpin terms *mini-lateralism*. U.S. policymakers destroyed the Bretton Woods system in August 1971. Protectionism has increased. The macroeconomic policies of the Reagan administration undermined stability by leading to exchange rate misalignment, massive bilateral trade deficits, and exchange rate instability.

The Europeans are not prepared to assume a global role. Their economies cannot easily adjust because of wage rigidities and welfare state commitments; and they have, in any event, never been as wedded to liberalism as the United States. Planned trade is more attractive for the European community.

The Japanese have made rhetorical commitments to openness, but the dense social networks that characterize all Japanese relationships make it very hard for foreign manufacturers to penetrate the Japanese market. The Japanese concentration on interindustry rather than intraindustry trade increases the likelihood of interest group-generated political conflicts. The Japanese cannot act as a market of last resort, one of the critical functions for any hegemonic leader in the world economy.

Because pluralistic leadership is unlikely, Gilpin argues that the best that can be hoped for is what he terms *benign mercantilism*—a policy of state intervention designed to promote domestic values and order rather than to prey on the vulnerabilities of other states. He already sees manifestations of such a strategy. There has been an increasing amount of sectoral protectionism, a policy that can accommodate variations in the ability and willingness of different countries to adjust to external pressures. Regional blocs are emerging with North America, West Europe, and the Pacific basin becoming more internally integrated.

Gilpin notes that his view is less sanguine than that of many economists who see the developments of the 1980s as the result of a temporary conjuncture of events whose consequences could be reversed through proper economic policies. It is also more pessimistic than the implications growing out of theories of cooperation concluding that liberal order is possible even without hegemony.

Gilpin is legitimately and explicitly skeptical about the possibility of securing empirical evidence that would decisively substantiate realist, Marxist, or liberal perspectives. But he has

written a study that makes an important contribution to the vitality and lucidity of that debate.

STEPHEN D. KRASNER

*Stanford University*

**ASEAN in Regional and Global Context.**

Edited by Karl D. Jackson, Sukhumbhend Baribatra, J. Soedjati, and J. Soedjati Djiwandono (Berkeley: Institute of East Asian Studies, University of California, 1986. viii, 357p. \$20.00, paper).

In its twentieth year, ASEAN faces considerable challenges. The organization has lost momentum in its political leadership of the opposition to the Vietnamese occupation of Cambodia. Economic integration within ASEAN still remains to be accomplished. In order to meet these challenges more effectively, the organization must move toward greater structure and institutionalization in its organization and decision making. ASEAN continues to play a central role in Southeast Asian politics.

This volume evolved from papers presented at the third U.S.-ASEAN conference, "ASEAN in Regional and International Context" held in Cheng-mai, Thailand in January 1985. The papers have been revised and reflect events up to February 1986, including the change in government in the Philippines.

This volume meets a variety of needs in the discipline. It provides excellent individual summaries of contemporary politics and foreign policies of the member states. ASEAN is well placed in its regional context with regional decision makers and specialists contributing. Vietnam's occupation of Cambodia and economic integration in ASEAN are each addressed with separate analyses by a regional and a U.S. specialist. Considerable attention has been paid to the selection of contributors to this book. Regional scholars are equal in number to U.S. contributors. All contributors are well known in the Southeast Asia and U.S. academic communities.

Part titles and authors are as follows:

"Overview," Thanat Khoman, Robert A. Scalapino, and K. S. Sandhu

"Politics and Foreign Policy of the ASEAN

States," Ansil Ramsay, Suchit Bunbongkarnand Sukhumbhand Paribatra, Jusuf Wanandi, Donald K. Emmerson, Carolina G. Hernandez, Larry A. Niksch, Shafrudin Hashim, Chan Heng Chee, and Michael Leifer

"Regional Relations," Evelyn Colbert, Douglas Pike, and David A. Steinberg

"ASEAN and the Major Powers," Karl D. Jackson, Hadi Soesastro, Bruce Glassburner, Leo E. Rose, Sheldon W. Simon, and Zakaria Haji Ahmad

This book will be of use to those searching for a paperback text or supplementary work on ASEAN. It is also appropriate for supplementary use in an international relations or politics course on Southeast Asia. It is the most comprehensive work on ASEAN, its member nations, and the region published in the U.S. in the 1980s.

ROBERT L. RAU

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**Partners in Conflict: The United States and Latin America.** By Abraham F. Lowenthal (Baltimore: Johns Hopkins University Press, 1987. xi, 240p. \$19.95).

Lowenthal argues that all administrations since World War II have paid too little attention to the needs and problems of Latin America or, conversely, have overreacted to perceived security threats, especially in the Caribbean-Central American region. In his view, the Reagan administration is one of the worst offenders. Lowenthal proposes that the U.S. government substitute for its hegemonic striving in the hemisphere a policy of special attention to the region embracing long-term cooperation and economic assistance. He argues that such a new approach not only meets the reality of a Latin America greatly transformed over the past three decades but also enhances U.S. security and economic interests.

To support this thesis he traces the enormous economic changes that have taken place in Latin America since 1960. He also expresses the hope that the recent replacement of a series of authoritarian regimes by electoral-constitu-

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tional systems represents a substantial move toward democratic reformism, mixed economies, and evolutionary social reform. He regards bureaucratic authoritarianism, military populism, and socialism as unsuccessful approaches to the drive for economic growth. Following this, he briefly reviews U.S. policies toward Latin America from the 1960s to the present, particularly toward Mexico, Brazil, and the Caribbean Basin. In the three areas he examines specific issues and applies his own perspectives as to how U.S. policy should be conducted and to what ends. He ends with a strong appeal for his new approach.

How well does Lowenthal present his case? The answer of course depends on one's own values and perspectives. It is doubtful that he will move the Reagan administration to modify its policy in Central America or to propose a major new response to the debt crisis and other economic problems. Those of us who have been critical of U.S. policies in Latin America find much to applaud in his prescriptions. I particularly support his view that no security threat emanates from any Latin American country but that it might come from Soviet submarine or strategic air bases in friendly countries. Many of us also believe that the debts should be scaled down, fees and interest rates reduced, and U.S. markets opened more freely to Latin American products, even those in stiff competition with U.S.-manufactured products. Lowenthal argues forcefully that if the Latin American economies cannot produce and sell their products abroad, they cannot possibly develop their economies, improve quality of life, or pay off their debts.

But I have reservations about the work, some of which are serious. First, Lowenthal frequently equates democracy with electoral constitutionalism. Second, he fails to clarify the relative responsibilities of the United States and the several Latin American countries for the economic development of the area. He is clearly no *dependentista*, but he does speak of dependent economies on occasion. More importantly, he fails to address social inequities in Latin America adequately. He notes the maldistribution of wealth, income, health care, education, and so on, but offers no analysis of local political restraints inhibiting reform. Finally, he fails to assess the political feasibility of his new policies or to of-

fer credible strategies and scenarios for implementing them. Lowenthal wants it all: U.S. cooperation for moderate social reform, economic growth and development, and democratic government, the program of the Alliance for Progress. With the demise of the alliance, many of us came to believe that "not all good things come together," that the three "goodies" could only be packaged separately, and that the U.S. role could only be minimally beneficial, when not harmful.

KARL M. SCHMITT

*University of Texas, Austin*

**Islam in a World of Nation-States.** By James Piscatori (New York: Cambridge University Press in association with the Royal Institute of International Affairs, 1984. viii, 193p. \$34.50, cloth; \$12.95, paper).

In the midst of politicization of "the children of God," particularly the Muslims, who seem to have risen against all established international norms, Piscatori's book is refreshing, delightful, and surprising. It is surprising because it is a grave, but sober and well thought-out, stand against the current strong wave both within and outside the Islamic world. It is generally held that Islam as a universal and comprehensive scheme for ordering human existence strives for a universal government, thus contradicting the present international system of territorial states. Piscatori tells us that he will write two volumes, one dealing with Islam and nationalism and the other treating "the transnational character of Islam" (p. vii). The present volume addresses the first issue.

Piscatori's basic argument is that Islam has been much more tolerant of political realities as compared with other ideologies and religions. In terms of the present international system, Islam and the Muslims are in agreement both with "its rules of the game" and more importantly with its philosophical foundation, "territorial statism." After a well-reasoned argument, Piscatori concludes that "in Islam, the nation-state is no less possible, or no more fraught with problems, than it is in non-Muslim world. Indeed, over centuries of historical experience and the evolution of theory, Muslims have largely freed themselves

of a model which denigrates territorial pluralism and demands monolithic unity. In the process, another, rather different, consensus has emerged which implies that the nation-state is, or can be, an Islamic institution" (pp. 149-150).

All Muslim countries are active members of the international system and its central club, the United Nations, including the Islamic Republic of Iran. Piscatori proves that such a participation is not simply a tactical move. Rather it represents a new consensus that has emerged among Muslims. This consensus is proven by an examination of "the classical and medieval theory," "the practice of Muslims" (or what the author calls "consensus of action" [*ijma' al-fi'l*]), and the works of Muslim writers (or "the consensus of speech" [*ijma' al-qaml*]). They all point to the harmony of Islam and the international system. One more factor in his thesis concerns the notion of development in modern Muslim thinking. According to Piscatori, "the view that Islam retards to the process of development . . . owes much to Marx and something to Durkheim" (p. 117), who connected nation-building with secularization. Recent Muslim experience displays, in Piscatori's view, a great deal of development even on the part of "radical" Muslims.

If Piscatori is right, then what about the so-called "radical Islam" or "fundamental Islam"? Piscatori discusses them under the rubric of "non-conformism." According to him, "while not the majority's sentiment, [it] has a distinguished place in Muslim thought on nationalism and the nation-state" (p. 101). It is true that nonconformist leaders consider features of the present international system as contrary to Islam, but Piscatori rightly observes that this kind of thinking "coexists with nationalist thinking. . . . Particularly when charged with running a society, these idealists become realists—of a sort—who recognize that the nation-state is a functional institution" (p. 116).

Piscatori's argument may raise a general objection. This way of interpreting Islam, its history, and the Muslim's life, it may be said, constitutes interpreting Islam according to the world rather than the world according to Islam. But this objection arises from a rigid way of defining Islam. Islam recognizes the right of Muslims to exercise independent judgements (*ijtihad*) wherever their religious tradition is silent. "The classical and medieval

theory does not refer to nation-state" (p. 144); and Muslims' words and deeds reveal a consensus that answers the objection. Piscatori's book is a major contribution to Islamic studies as well as an important treatment of the issue of the boundary between religion and politics in Islam. And if the author's promise is materialized and the companion volume on the transnational aspect of Islam is published, many of the ambiguities that overshadow politics in Islam will be answered.

FARHAN RAJAEI

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**Modern Diasporas in International Politics.**

Edited by Gabriel Sheffer (New York: St. Martin's, 1986. viii, 349p. \$32.00).

In recent years the activities of ethnic minorities within nation-states have become of interest to scholars of both international relations and domestic politics. Not only have these populations presented serious challenges to the internal social, economic, and political practices of individual countries around the globe; but they also have become a source of irritation in several interstate relationships. One has only to consider the recent roles of the Chinese in Vietnam, the Tamils in Sri Lanka, or the East Indians in Uganda to realize that such ethnic minorities have become actors in the international arena as well as players on the domestic stage.

Gabriel Sheffer's edited work attempts to draw our attention to this fact. Each of the essays contained within the book focuses the reader's attention on the role and influence of ethnic diasporas on international and trans-state politics. Whether it be the case of Turkish "guest workers" in Western Europe, Indian immigrants in East Africa or the overseas Chinese population in Southeast Asia, an effort is made to detail the manner in which such groups become important—and often controversial—linkages between two societies. Similarly each author explores the character of the triadic networks of interaction and influence that develop among "homeland," "host country," and the diaspora group that develop from such linkages.

This inquiry into the significant international dimension of ethnic minorities repre-

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sents an important advance in the study of immigrant populations. Until quite recently most of the literature in the field focused its attention almost exclusively on the internal political activities of immigrant groups and ignored their linkage role to the international system. The several contributors to Sheffer's volume have done an excellent job in attempting to conceptualize and empirically explore this linkage function. They have endeavored to discover how and under what circumstances immigrant groups become active participants in the international arena as well as the effects of such participation on the foreign relations of homelands and host countries. Milton Esman's concluding essay is particularly helpful in this regard.

The comparative focus of the inquiry is one of the major strengths of the volume. In most instances the cases that have been selected for study aptly illustrate the various ways in which immigrant groups become involved in interstate tension and conflict. Unfortunately, not all of the more recent examples of this type of behavior are noted. This is particularly the case with instances in North America. One of the few shortcomings of the book is that it tends to neglect Vietnamese, Haitian, and Hispanic migrations to the United States and Canada, while devoting disproportionate attention to Jewish and Palestinian diasporas.

Another concern relates to the contributors' desire to establish a distinction between the behavior of diaspora groups and other immigrant populations that are more assimilated into the host community. While it is true that the former groups are likely to be more visible and prominent participants in the linkage function between homeland and host country than the latter, this difference in behavior may be a difference more of degree than in kind. Certainly much of the existing literature on non-diaspora immigrant groups has useful insights into the international behavior of ethnic minorities and should not be ignored.

Overall, *Modern Diasporas in International Politics* should be welcomed as a useful contribution to the growing number of studies examining the external behavior of ethnic groups. It provides a fresh look at the behavior of one type of immigrant group and reminds us that political activity by such communities is a contemporary reality and not simply a historical condition. Interstate relations in the lat-

ter part of twentieth century will continue to bear the imprint of immigrant influence.

DOUGLAS C. NORD

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**Space and National Security.** By Paul Stares (Washington: Brookings Institution, 1987. xvii, 219p. \$28.95).

Space and national security remains a major issue of discussion in academic and policy circles. A number of volumes on the subject have been published in recent years as well. In contrast to Stares's first work, *The Militarization of Space*, this one focuses squarely on the role of antisatellite (ASAT) weapons and their role with respect to strategic diplomacy and existing weapons systems. While there has been some work conducted in this area, this is one of the first comprehensive treatments of the subject matter.

Stares argues that the United States need not pursue the development of an antisatellite capability to meet the Soviet threat to U.S. assets in space. If in fact the United States proceeds with the development of its program, we can surely expect the Soviets to reactivate their ASAT program and throw the United States and the Soviet Union into a new and dangerous arms race in space. It will make the costs of increasing security for existing U.S. assets in space more costly and difficult to ensure. In addition, the increased complexity of ASAT systems might add greater uncertainty to decision making in crisis situations. As an alternative to this situation, he proposes several alternative strategies for resolving the current stasis in U.S.-Soviet relations regarding antisatellite weapons. In the absence of a full scale effort of the kind envisioned by the Reagan administration, Stares proposes that U.S. officials continue to improve U.S. capabilities for surveillance of activities in space; continue to take measures to reduce the threat from Soviet satellites; develop a U.S.-Soviet ASAT test moratorium; develop a U.S.-Soviet space weapons ban; develop an accident provision in a space agreement; and develop new guidelines and clarifications for permissible types of antimissile research and development.

The evidence for these ideas are based on a thorough examination of the utility and value

of, and threats to space systems posed by, existing technologies and formal arrangements between the United States and the Soviet Union. In addition, he presents a comparative analysis of the balance between the U.S. and Soviet space programs, and argues that any basic understanding of the strategic balance in space requires an assessment of the contextual framework within which satellites and anti-satellite weapons fall. Stares begins his work with a comparative assessment of the two programs, and concludes that the United States is still the dominant power in space. Furthermore, this is not likely to change in the near future. In fact, Stares points to the panic of the Soviets regarding the Strategic Defense Initiative as a clear sign of their fear of U.S. power. In effect, the Soviets want to avoid an arms race in space because they are fearful of the long-term consequences. He argues persuasively that although the Soviets have a larger number of satellites in space, the quality and longevity of U.S. satellites more than makes up for their smaller number. The Soviets build simpler satellites dedicated to single missions. However, one disadvantage for the United States remains outstanding. Because the United States has fewer satellites, the level of dependence is greater; and this could have horrible consequences in a crisis situation. Under current circumstances, according to Stares, neither the United States nor the Soviet Union is likely to attack the other's assets in space. Should the current situation change and the relative values of satellites change in response to a new set of conditions, we might face an entirely different set of problems.

The value of satellites are assessed in terms of their contribution to national security and enhancing the effectiveness of operations on earth. In contrast to this rather broad-based statement, Stares argues that the value of satellites depends on a number of critical variables that change from one context to another. It is therefore senseless to make general statements about the value of satellites without allowing for this kind of change. Such variables as geography, the nature of a crisis, redundancy, and so on play an important role in the relative value of space assets. Moreover, many satellites are used for essentially the same purposes, and can be used for more than a single mission. Stares' major conclusion, based on his review of the evidence, is that given U.S. dependence

on satellites and the diverse uses different satellites can be put to, the United States has much more to lose than to gain by proceeding with an antisatellite weapons program.

In that the principal reason for the development of a U.S. ASAT program is the threat posed to the United States by Soviet assets, Stares proposes that basic enhancements of existing space-based systems would serve to limit the effectiveness of a Soviet attack. Such devices as attack and warning sensors, emergency maneuvering aids, and adding decoys to U.S. satellites would be sufficient to limit and deter a Soviet attack. The author draws on a considerable body of literature to support this position. The overwhelming consensus is that the Soviet threat to U.S. systems is exaggerated and need not be taken seriously. Once again, Stares notes that an arms race in space of an offensive nature can only make matters worse.

The most interesting feature of the study addresses the circumstances under which ASAT weapons might be made operational and employed, but this is also the weakest part of the whole work. The more important point is that contextual factors are embodied by each scenario where ASAT weapons might be used, and might play an important role in the outbreak of war. Several scenarios are possible. The first is what the author refers to as "client-state conflicts." The Arab-Israeli war of 1973, the 1978 war between Ethiopia and Somalia, and the Falklands-Malvinas war of 1982 were all circumstances of this kind and moments where satellites came into play. While the United States and the Soviet Union have provided intelligence to their clients via satellite, we should not expect much more. The United States and the Soviet Union are not likely to attack each other's satellites simply because one state provided intelligence to a client. Escalation between the superpowers is very unlikely under the circumstances. The second major form is the "client-state-versus-superpower conflict." The Libyan crisis is cited as an example of this type. It is a situation where a superpower provides information to a client state engaged in a conflict with another superpower. Intelligence provided to a client state could have an important impact on the ability of a lesser state to defend itself and inflict damage on its adversary. While the Soviets and U.S. leaders would certainly act to com-

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plicate such transmissions, once again it is very unlikely that either the United States or the Soviet Union would use ASAT weapons against the other as a result. Once again, escalation would be very unlikely. The third major type is "superpower-versus-superpower regional conflicts." In contrast to other types of conflict, this form is more open to danger and could very well throw the whole international system into chaos, and Stares therefore gives greater attention to this form.

In a kind of cost-benefit analysis Stares concludes that even in the latter context the costs of using ASAT weapons would probably outweigh the benefits. Reciprocal action by Soviets and U.S. leaders would no doubt serve to deter each state from attacking the other. If either power struck the satellite assets of its adversary, this would provide an important set of signals to the other power that could play a decisive role in subsequent action. This action could prove to be catastrophic. In the case of a possible Soviet invasion of Western Europe, ASAT weapons would not be very useful. The extensive quality of Soviet satellites in the European theatre are bolstered by measures of redundancy that would make any loss of Soviet satellites a minor matter. The Soviets could move quickly to levels of reconstitution commensurate with their security requirements.

DARRYL ROBERTS

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**The Specialized Agencies and the United Nations: The System in Crisis.** By Douglas Williams (New York: St. Martin's, 1987. xvi, 279p. \$29.95).

This is a book produced by a committee. The committee is entirely British in membership and is overwhelmingly composed of former practitioners of intergovernmental organizations. That it is British means that the book is calmly and gracefully written; that it is practitioner-shaped means that it is more experientially "wise" and less "scientific" than many U.S. readers might prefer. It is also pro-United Nations, in its fashion. These practitioner-consultants all see the UN "system" as being severely threatened in our time, they want to protect and rescue it, and they try to

tell how. In the process and in any event, they wish to inform.

This last they do successfully. Anyone who wants a quick, comprehensive update on, and evaluation of, the UN "system" of specialized agencies will find it here. It does not often treat the agencies individually and it usually and diplomatically declines to name names, but it organizes its survey of their operations sensibly and covers them well. The analysis is almost laboriously even-handed: yes, most specialized agencies have been exploited by their third-world members, who force the consideration of allegedly extraneous political issues into their sessions (and shouldn't); but (1) hardly any political issue is truly irrelevant to some of these agencies (especially UNESCO) and (2) perpetual and unpersuasive frustration of their successful resolution in the designedly "political" organs of the UN has encouraged third-world delegates to bring them repeatedly to whatever other forums they can find. As this commentary observes more than once, UN member states have got "the UN System which they deserve" (pp. 205, 207).

The prescription on how to save it is thus readily come by: Disputing members must see the advantages of the system that accrue to all and adjust the conflicts that have arisen between them into some new international consensus, vaguely delineated but devoutly to be wished. Can it be done? "Despite Kassebaum, the reaction to African famine and the Chernobyl accident may indicate that this could be happening" now (p. 239).

"Despite Kassebaum." The United States is the major villain of the piece. This book's account of "Kassebaum and Gramm-Rudman" (chapter 8) is a small gem of analysis and indictment. The real *bête noire* is the Reagan administration, in this and in several other respects. Only rarely does this study lose its characteristically weary cool and point an angrily accusing finger. U.S. refusal to approve a \$12 billion "replenishment" for the International Development Association in 1985 occasions one such outburst: "It is the degree of arrogance of the Reagan administration and its unwillingness to demonstrate any spirit of cooperation on such matters, even with its close allies, that has alarmed its friends and many governments in the richer as well as the poorer countries of the world" (p. 162).

But even here the study remains cautious.



Friends of the UN should not simply wait for the Reagan administration to pass and for another less anti-UN administration to take its place. The affliction is deeper than that, everywhere. This book may not prescribe for it very convincingly. But it analyzes it sympathetically and carefully. In doing so, it recognizes national variations of outlook toward the UN clearly enough. One commentator's views are said to be "characteristically French, and it is doubtful if they would be shared by, say, the Scandinavians" (p. 188). The work of the committee that did this study might have been enriched by a few, say, Scandinavians—or third-worlders, or rigorous U.S. social scientists. But within the limits of its genesis and format this is good work. Both friends and enemies of the UN everywhere would do well to pay attention to it.

KEITH S. PETERSEN

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**Positive Sum: Improving North-South Relations.** Edited by I. William Zartman (New Brunswick, NJ: Transaction Books, 1987. 262p. \$29.95, cloth; \$14.95, paper).

Throughout the 1970s most negotiators treated most North-South encounters as zero-sum conflicts. The rulers of the wealthy states believed that any moves toward the South's New International Economic Order would come at the cost of the North's wealth and power. Representatives of the Group of Seventy-seven acted as if accepting mere reforms of international economic institutions would undermine their more radical goals. This collection of essays is motivated by the belief that more positive, mutually advantageous outcomes could be achieved.

The book consists of eight case studies of North-South negotiations, mostly in the late seventies and early 1980s, framed by Zartman's introduction and conclusion, which link the studies to broader themes from different theories of negotiation. The subjects of the case studies have been well chosen. They cover all of the major North-South issues and institutions usually discussed in the literature: commodities, debt, the law of the sea, the United Nations, the European Economic Community-African Caribbean Pacific forums, and the various ad hoc forums created in the

1970s. The cases also include a few that are perhaps even more important than what is usually covered: negotiations over wheat and third-world food security, the multifibre arrangements, and the debates about telecommunications in the International Telecommunications Union.

Some cases are better researched and more thoughtfully written than others. Raymond F. Hopkins's study of the wheat negotiations and Barbara A. Fliess's study of the World Administrative Radio Conference of 1979 are particularly outstanding. Even with some weaker essays, the overall quality of the case studies is much better than one might expect in such a broad collection. All the authors are significant authorities with expertise in their particular subject.

This does lead to one of the book's drawbacks. A great deal of what appears here has appeared before, in only slightly different forms, in other articles or books by the same experts. The book would have been a good deal more timely if it had appeared immediately after much of its original research had been completed. As it is, the importance of these essays must be historical; the North-South conflict has changed quite a bit since the early eighties.

Nevertheless, the book is a unique resource, a broad analytical history of the most important North-South negotiations in one period, a major resource for students of negotiation, North-South relations, and international organization. I know that over the next few years I will recommend it many times to many students with very different interests.

In the end, of course, the book cannot answer its central question. It does not tell us how North-South negotiations can move from zero-sum impasses. Theories of negotiation can only tell us that people often can construct common visions of their mutual interest. Those theories cannot tell us what those visions might be. To understand the current North-South impasse fully we also need to understand why the visions of mutual North-South interest that have been offered (the most important example being the international Keynesianism of the Brandt Commission) have failed to convince. But that is the job of another book.

CRAIG N. MURPHY

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## Book Reviews: Political Economy

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### POLITICAL ECONOMY

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**Wage Determination and Incomes Policy in Open Economies.** By Anne Romanis Braun (Washington: International Monetary Fund, 1986. xix, 380p. \$28.50, paper).

The field of international political economy covers a range of analytical levels, from the systemic interactions of global economics and politics to the comparative study of domestic industrial relations. We hear continuing arguments over the relative importance of international over domestic factors and over the value of economic versus political analysis. Anne Romanis Braun, formerly of the International Monetary Fund, falls clearly and not surprisingly into the category of those relying on economic analysis for her arguments. This book sums up her long experience studying incomes policy and wage-bargaining systems in advanced industrial economies and concludes that incomes policies have been used, historically, in response to weaknesses in the international payments system. The most interesting aspect of her book is that in the debate over incomes policies as a method to moderate the trade-off between inflation and unemployment, Braun concludes that "in the last resort, the problem is an essentially political one of encouraging the formation of consistent and feasible real income aspirations of organized groups . . . and of promoting a system of checks and balances to prevent excessive and disruptive gains by particular groups" (p. 57).

Braun has undertaken a complex task, and her many arguments can become confusing. First, she defines incomes policies as those that restrain increases in money incomes by restraining powerful organized groups; she explicitly does not include in her study those incomes policies implemented with purely redistributive objectives. Next, she divides her analysis into four time periods based on the type of incomes policies implemented by the industrial countries: the 1940s and 1950s, the early 1960s, the late 1960s and early 1970s, and from 1973 to the present. However, the book as a whole consistently focuses on a com-

parison between the policies implemented in industrialized countries during the period of fixed exchange rates before 1973 and their reactions after the switch to flexible rates. Prior to 1973, she argues, the industrial economies approximated a single closed economy where incomes policies could be effective in maintaining price stability. Under the pressure of differing rates of inflation, during the late 1970s the macroeconomic goals of the industrial countries increasingly diverged. Braun goes on to analyze four large industrial economies (the UK, Japan, the Federal Republic of Germany, and Canada) and three small economies (Austria, the Netherlands, and Norway) and their adjustment problems under flexible exchange rates.

Braun departs from mainstream macroeconomic analysis by detailing the flaws in applying neoclassical economic theory to her subject. Her familiarity with the history of incomes policies and wage bargaining leads her to the somewhat surprising conclusion (for an economist) that the assumption of perfect clearing of all markets, a key tenet of general equilibrium theory, is too far from reality to explain wage and price behavior in the short and medium term (though she makes no comment on its utility in predicting long-term trends). Braun describes both labor and product markets as characterized by administered prices in some sectors, which act as barriers to competition. The idea of monopoly behavior is not new, but Braun brings together many of the arguments about such behavior on the part of both management and labor, devoting an entire chapter to a critique of neoclassical economic theory, particularly monetarism and its policy prescriptions.

The main problem with Braun's work springs partly from the complexity of her subject, and partly from the conclusions she draws. One of the more surprising aspects of a book by an economist is that it is theoretical and historical-descriptive, with a weak empirical foundation. There seems to have been no intention actually to test the numerous interesting hypotheses she proposes, despite the

great amount of information she supplies. For instance, while the last half of the book consists of detailed case studies, her rationale for the particular countries she chooses is not clear; she simply divides them into large and small industrial economies, without giving a theoretical justification for this categorization, a justification that would have been possible by incorporating current arguments on state size and openness to the international economic system as factors influencing policy options. Another example is her choice of actual incomes policies to study—in the first chapter she lists eight different kinds of incomes policies that fit her limited definition. Only wage indexation gets a thorough examination, as part of Braun's excellent critique of monetarist policy recommendations.

For a political scientist, this book can be very frustrating to read. Braun offers some interesting hypotheses and some contentious conclusions but without enough supporting data. In her discussion of wage indexation policies, she states that history shows they are most effective as a means of mitigating the political consequences of rapid inflation and lessening conflicts arising from changes in the distribution of income. Further, "indexation may also have been an important element in ensuring the survival of a democratic, free enterprise system" (p. 206). This is strong stuff and not fully supported. The crucial political variables are subsumed in the discussion under wage-bargaining structure and macroeconomic policy. Braun does not explicitly address the type of regime as a variable in her analysis. Nevertheless, in her conclusions to the case studies Braun states that the 1970s demonstrated that rapid inflation could be controlled by either moderate demand restraint implemented by a labor or coalition government with a union-supported incomes policy, gradually slowing inflation while avoiding sudden unemployment; or by very restrictive demand policies implemented by right-wing governments intent on restoring price stability rapidly by increasing unemployment to moderate wage demands (p. 313).

Braun's final chapter was for me the most interesting, though sadly incomplete. In it she outlines a program of research into the effects—theoretical and empirical—of dividing the national economy into two sectors: the large enterprise sector manufacturing for the inter-

national market and the national manufacturing sector producing for the domestic economy. The author hypothesizes a relationship between the scale of an enterprise, the difficulty of entry into that sector, and the ease of organizing the labor force. The relative distribution of these two sectors within an economy would influence the range of policies available to it and their relative effectiveness. All of this is presented simply as hypotheses to explore, a prelude to what I hope will be the next work by Anne Romanis Braun.

This book brings together detailed information and ideas on the subject of wage bargaining structures and incomes policies, and will be interesting to those who study problems of adjustment and industrial unrest in the advanced industrial economies. However, reading this book can be likened to taking a long drive on a country road: there are a lot of bumps, and it would be helpful to have a map or some familiarity with the way; but despite the problems, there are occasionally some fabulous views.

VIRGINIA HAUFLE

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**Public Choice. An Introduction.** By Iain McLean (Oxford: Basil Blackwell, 1987. viii, 222p. \$45.00, cloth; \$14.95, paper).

As its back cover suggests, this is indeed a timely publication. Although the *Papers on Non-Market Decision-Making*, the predecessor of the journal *Public Choice*, was first published in 1966, the public choice approach was not made well known to others outside the field of political economy until James M. Buchanan was awarded the Nobel Prize for Economics in 1986.

Apart from Dennis Mueller's *Public Choice* (1979), this is the only available volume to date that surveys major works of public choice theorists. Like Mueller, McLean has chosen to organize his book into two main parts. Part 1, "Practice" deals with positive public choice, and part 2, "Theory" deals with normative public choice. This classification may cause critics of public choice to raise objection, since part 1 itself includes various public choice theories in explaining the behavior of politicians, voters, interest group members and

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civil servants. Perhaps this problem arises from the difficulty that no description can be manifested without an interpretation of "what is."

Part 1 begins with a chapter on the "scope" of public choice before it proceeds to review the works of public choice theorists who view politicians as political entrepreneurs (Frohlich, Oppenheimer, and Young), voters as consumers and political parties as firms (Anthony Downs), interest group members as free riders (Mancur Olson), bureaucrats as budget maximizers (William Niskanen). The first part of the book ends with a discussion on political coalitions that centers around the works of William Riker.

One of the strengths of this book is that after each model has been presented, both U.S. and British cases are used to test it. However, while McLean is conscientious to attract an English-speaking audience from across the Atlantic, he makes no mention of the development of public choice in continental Europe or in Japan. Moreover, it is unfortunate that McLean has not included a section on the intellectual development of the public choice approach in this introductory text. It is equally unfortunate that unlike other review articles (e.g., Louis Weschler in *Public Administration Review* 42 [1982]: 289 and Mark Sproule-Jones in *The Politics of Canadian Public Policy* [1983], pp. 127-47), this book has not laid out the fundamental assumptions used in public choice theories.

Part 2 includes a chapter on three prominent problems in collective action: assurance game, chicken game and prisoner's dilemma game, as well as a chapter on Arrow's impossibility theorem. Although concrete examples are given throughout the discussion on these "games" (pp. 127, 129, 131, 133, 139), McLean has made little effort to relate the meaning of the logic derived from them to more concrete social and political problems. While the concluding chapter deals interestingly with the "unfinished business" in collective action, the book suffers from an omission of a closing section outlining the common criticisms of the public choice approach. Nevertheless, McLean seems to be implicitly addressing one criticism from some economists who charge that public choice is too simplistic, by repeatedly echoing that the problem is "more complicated . . . than it looks" (pp. 132, 147, 154, 170-72).

Two founders, James Buchanan and Gordon Tullock, made their entry into public choice with their publication of *The Calculus of Consent* (1962); however, McLean has neglected to review this important work. Equally important is Vincent Ostrom's book, *The Intellectual Crisis in American Public Administration* (1973) because it introduces a significant variant of public choice to the field of public administration. McLean has chosen not to include Ostrom's book in his discussion. Ostrom's approach to public choice might appeal to an audience that does not subscribe to quantitative analyses and hence would not employ mathematical proof in resolving intellectual problems (see PS 20 (1987): 242). This might be important, since McLean's first intended audience "is a student of politics or sociology" (p. 2), who will have little training in mathematics or economics.

Unlike many other public choice reviewers, though, McLean has by and large fulfilled his promise not to use mathematical presentations in his review. He succumbed to the mathematical temptation on a few pages only (138 and 144-45). In other parts, he has included the mathematical logic, when necessary, in appendixes at the end of a chapter.

Another strength of this book is that an informative list for further reading and a glossary of technical terms are provided at the end. But this plus is counterbalanced by the frequent use of abbreviations with no reference list. This might cause great inconvenience to those who wish to read only part of the volume. Examples of abbreviations are numerous: MW (minimal winning coalition), SW (minimum size coalition), MCW (minimum connected winning coalition) (p. 113); AGs (assurance games), CGs (chicken games), PDGs (prisoner's dilemma games) (p. 126); TFT (tit for tat, p. 135); CSS (collectively stable strategy, p. 137); STV (single transferable vote, p. 156); AV (alternative vote, p. 159); SWF (social welfare function, p. 165).

There are other minor debatable issues. McLean seems to equate the meaning of *self-interest*, an important concept in methodological individualism, with the term *selfish* (p. 13). While selfish individuals are those who act with little regard for others' welfare, self-interested individuals can be understood as those who act consistently in order to promote their own interests without necessarily causing

harm to others' welfare. The confusion between these two terms has caused some to accuse public choice theorists of being too cynical about human activities in the public sector. Also, McLean asserts that "anarchy" could be a viable method for producing public goods (pp. 14-16). This might be another debatable issue (see Buchanan, *What Should Economists Do?* [1979], p. 273).

Apart from these minor flaws and debatable

issues, the value of this book must be judged by its success in introducing the public choice approach to the unfamiliar reader. As such, this book will undoubtedly remain for some time an excellent introductory text for both undergraduate and graduate students.

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